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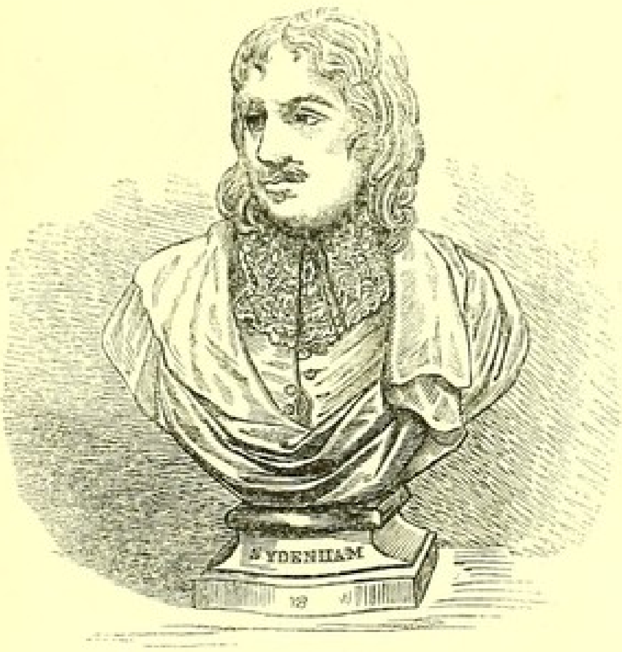

LONDON

MDCCCLV.

MEDICAL LOGIC.

BY

F. OESTERLEN, M.D.

TRANSLATED AND EDITED

BY

G. WHITLEY, M.D.

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PRINTED FOR THE SYDENHAM SOCIETY.

MDCCCLV.

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TRANSLATOR'S PREFACE.

If the study of Medical Logic be advisable for practitioners in a country such as Germany, where no branch of education is deficient in metaphysical training, it appeared to the Council of the Sydenham Society that it would be valuable in England, where so little of metaphysics is taught during any portion of a student's career, and where the deficiency of logical accuracy in our medical literature is universally felt and admitted.

In fulfilling the commission with which the Council has intrusted him, the translator has aimed, not only at rendering the original faithfully, but also at making it as intelligible as he might to all who are disposed to grapple in earnest with the subject. He has found his task difficult even for one long acquainted with Germany and the German language, and he fears that his version may, by some persons, be pronounced un-English. He has, therefore, thankfully availed himself of the assistance of friends, to get rid, as far as possible, of foreign idioms and expressions, even where it seemed necessary to retain, or to exemplify, foreign modes of thought.

By such German scholars, however, as will take the trouble of referring to the original treatise, he hopes to be acquitted of anything like carelessness. The author, however solid and accurate, is not remarkable either for the clearness or the elegance of his style, and has largely availed himself of that peculiarity of his mother tongue by which new words are created, almost at discretion, to express thoughts and conditions of the mind —words for which no exact equivalent can be found in any other vocabulary.

G. WHITLEY.

FINSBURY; *June,* 1855.

PREFACE.

The purpose which this work is intended to fulfil is more fully explained in the Introduction. I shall therefore only say here as much as is necessary to obviate, at the outset, a misunderstanding to which its title might give rise. It can scarcely be necessary, indeed, to assure the reader that Logic, in its ordinary signification, is not here meant, but only the application of certain rules and principles of logic to the study of natural science in general, and of medicine in particular. Our aim, therefore, is a purely practical one—*viz.*, to show, clearly and impressively, the mode in which we must proceed in our observations, investigations, and conclusions, in order that our Theorems and Problems may become more clearly intelligible, and that we may arrive at experimental truths and definite laws in our department of science, as well as at scientific principles of practice.

The methods and paths by which alone we can arrive at all these ends, and the technical details and peculiarities which encumber them in a study such as ours, have nowhere been set forth, or at least not so that the philosopher, or physician, could derive from them all the advantages and services which he requires at every step: for philosophers and professed logicians have seldom or never touched upon what *we* have most stood in need of, perhaps because the subject matter—the individual questions and objects with which the natural philosopher or physician has to deal—were not sufficiently familiar to them. The latter, on the other hand, seem often not to have closely considered or understood all the processes which they have themselves adopted in their investigations and operations, and by the aid of which they themselves have perhaps arrived, not only at their deductions and theories, but also at their most important discoveries. In short—many considerations have concurred to prove that Logic has not always so harmonized and entered into combination with Medicine, or Natural History, as is indispensably necessary for the interests of science. It would seem to me, that philosophy and logic have, on the one hand, by no means given us the precise aid which we require; and that, on the other, we have not always sought for the support and guidance of a sound logic, where we could scarcely advance safely without it. We may, perhaps, have learnt philosophy, but not to philosophize—we may have learnt logic, but not to form correct opinions and conclusions from observation and investigation.

But since medicine and the study of natural philosophy have begun to assume a closer relation to each other, and physicians have engaged more frequently in scientific inquiries, and advanced from the field of mere empiricism and conjecture to that of comprehension and induction, the desire for scientific methods of inquiry and adequate demonstration has everywhere grown upon them, and has thereby become a trustworthy guide. Men speak more frequently than heretofore of the necessity of a logical mode of proceeding, of a correct process of induction, &c.; but we have nowhere attained a distinct and complete understanding of all that which we presuppose, nor have we learnt how to search and investigate in our department of science, so as most quickly to arrive at safe principles and conclusions.

The present has seemed to me, therefore, a seasonable and worthy attempt whereon to venture so far as my powers admit. And the hope of doing some service to my professional brethren, and, through them, to medicine, or even to natural science in general, has been increased by the circumstance that a period of calm research, in which facts have accumulated, has arrived, free from the excitement which new theories and systems usually produce. The most favorable moment has thus, perhaps, arrived, for making a psychical analysis of the general progress of the human mind; for observing the mind itself in the act of investigation, as well as the mode in which it investigates; and for inquiring why we have hitherto made no further advances in our department of science, and how we may, with the greatest degree of certainty, do so.

At this point, however, I wish to obviate the suspicion that the following pages contain nothing more than a pretended elucidation of ideas, and a critique, or that they will sweep away the past and leave only a "*tabula rasa*" for future investigators. The entire aim of this work is rather to endeavour to promote improvement and progress, than to overthrow, or to undo; nor am I called upon to become the judge of my predecessors or contemporaries. It is with results only that we have here to deal, as our progress towards improvement—our future success—depends upon a clearer insight into any deficiencies in these results, and especially into the causes of such deficiencies or errors.

If even a Bacon quoted himself as an instance that as a tyro in philosophy one can do much service, much more may a similar excuse be admitted for the author of this work, if his attempt should not fulfil the expectations of his readers. But it is always a difficult task to cement together the detached fragments of a subject never yet treated as a whole. It is, however, consolatory to bear in mind, that "he who cannot go to Palestine may still go to Rome," and thus have I, who am self-taught, endeavoured to weigh as exactly and carefully as possible for myself, and to explain, in a manner free from the garb of scholastic philosophy, what has appeared to me to be at once most important and most difficult. Moreover, my guides have been "special," that is, men who have been engaged in one particular study, philosophers on the one hand, as well as naturalists and physicians, both of ancient and modern times, on the other. I am indebted, however, to J. Stuart Mill's excellent work for the greater part, the true pith and thread of my system, and the following is, in a great measure, but an application of his doctrines to the particular questions and objects of our department of science.

OESTERLEN.

HEIDELBERG; *September*, 1852.

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INTRODUCTION.

As I feel conscious that the title of my book may, with many readers, lay it open to a degree of doubt respecting its precise purport, I think it desirable to anticipate some of the questions which may naturally arise, by a few words in the way of introduction.

What, it may be asked, are we to understand by the term Medical Logic? In what relation does logic stand to medicine and to natural science in general; and in what degree is the study of it likely to promote and facilitate the peculiar objects such sciences have in view? Will it assist our investigations into the structure of man; or may we, by its aid, expect to arrive at a fuller comprehension of natural science in its various branches, and thus be enabled to advance more safely and practically? Will it afford the physician means whereby to understand the condition of his patients more clearly, or endow him with more skill in the treatment of disease, or render him more successful in its prevention? Such were among the questions which I put to myself and carefully considered, before I could resolve to proceed with this attempt.

As regards the title "Medical Logic," I might have found many others as fitting, or even more so; but none so short. It expresses at once the aim of this work; which is to set forth the importance of a correct application of logic to medical facts and inquiries, and to show the advantages it may render to natural science in so far as logic is connected with such questions.

I need scarcely remark that we are not here dealing with the ordinary logic of the schools, which I must leave to professed logicians. In the promotion of the practical object which we have in view, the term "logic" is taken in its widest sense and considered as *"the science which acquaints us with the processes by which the mind of man, whether working in direct or indirect channels, may arrive at that establishment of fact in the correct appreciation of nature which constitutes empirical truth."*

For the natural philosopher or physician no more knows by intuition what he needs to understand and learn, than he does anything else throughout nature. He must owe his insight into all these subjects to observation and experiment; or, in other words, to his experience and the inferences with which it supplies him. By his own efforts, and by many collateral means, he must endeavour to arrive at knowledge; for truth is the philosopher's stone which we all seek and are still, except in miserable fragments, so far from having found. Yet there are certain mental processes, by the aid of which we are able to investigate the actual occurrences in nature; and through these logic is our guide. It teaches us how, by observation, experiment, and inference, we may overcome the obstacles that stand in the way of a clear comprehension of whatever natural phenomena we have to do with, and thus brings us, by a careful induction of processes, to the knowledge not only of their causal connection with each other, but also of their governing conditions and laws. It further shows us to what extent observation and deduction must be carried, and what requirements they must fulfil, before we can rely upon them as upon acknowledged facts and truths; and explains to us the mode of demonstration by which an assertion or inference, whether drawn from our own particular province of inquiry, or from the wide field of general natural science, may be established as true and proven; and an individual experience be accepted as a permanent truth. It will enlighten us in any given case, as to the degree of correctness with which an experiment or a fact has been observed and interpreted. It will thus enable us to judge whether the conclusions we are inclined to draw from it (our general abstractions of laws for the science, and of rules for the art), follow strictly in the track of experience: it will also teach us to what extent we are justified in relying on them as on thoroughly demonstrated experimental truths and facts. In short, if it be the object of logic to teach us to think and judge correctly; if it be in all cases the science of exact demonstration, holding up the test whereto to bring our judgment, assertions, and conclusions, then must it (if employed so as to promote our interests and those of natural philosophy in its widest signification,) assist us in finding that which is the especial object of our search. By teaching us to appreciate our discoveries, whether real or imaginary, at their exact value, it will enable us to employ them to the fullest advantage, whether in the more immediate service of science, as in elucidating its laws and principles, or in extending the practical application of them, *e. g.* to the rules of therapeutics.

Logic, therefore, in this more special signification, has much to accomplish. It must also be confessed that we are now asking of it services which have not often been demanded of the logic of scholastic philosophy, and such as it has still less frequently rendered. The father of logic, the great Bacon, has complained that the latter is almost useless for discoveries in the sciences, and for the investigation of truth. Men, says a certain Frenchman, have often believed themselves to be profound whilst they were only hollow. However this may be, we here take logic and its objects only as they are to be understood in medicine and in natural science in general; and we therefore give it an acceptation in which philosophers and professed logicians cannot always employ it, because they are often too little acquainted with all the details and peculiarities of the subjects and objects of investigation with which we are occupied, and, consequently, with our particular wants and difficulties.

Logic, in this sense of the term, is the appointed cultivator of a soil, more or less crude and barren, wherein it must sow with patience, and in due time it will reap, a sound and nutritious aliment. It then becomes, both to the physician and the naturalist, the science of sciences, the "ars artium," in the fullest sense.

Thus, when we speak of Medical Logic, we simply mean the application of the science in question to our own special province. We only seek by its aid to explain and develop more clearly the ways and means, the various processes, by which the natural philosopher, the physiologist, and the physician are accustomed to proceed in their investigation of phenomena, occurrences, and influences. It is the province of Medical Logic to point out how we may best observe, investigate, and experimentally test all those processes, influences, and occurrences; those causes, so to speak, and effects, under whose influence man and all living things are born into the world and continue to exist. It must lead us to ascertain the conditions of the living body, and to observe through what peculiar changes it passes from health to disease, and from this again to recovery. This science must determine the exact point to which our observations and experiments ought to lead us; it must limit the inferences which we draw from them in relation to causal connection, and will thus preserve us from building up rash, uncertain conclusions upon generalizations derived from a doubtful, or even erroneous experience, and conduct us, by an exact method of demonstration, to that accurate investigation of nature, which, allowing her to speak for herself, enlightens us as to the real state of things, and so becomes our guide in deducing, with the greatest attainable certainty, those rules by which we must abide in our technical progress as physicians.

It is not, indeed, from logic, but from the science, whatever that may be, with which he has to deal, that the natural philosopher or physician must gain the knowledge of the facts and principles he requires; its province is but to guide him in the correct application of them and to enable him to judge whether the processes which are at work, either in his own mind, or in those of others, for the elimination of certain truths, attain to, or fall short of the desired aim. It leads him, in the case of any branch of science, to discriminate between the certain, the doubtful, and the erroneous. For we may at all times and in all cases rely upon our investigations, abstractions, deductions, and generalizations, as correct and true, in proportion as they stand the test of an exact and comprehensive logic; and should they fall short of its requirements, it will not fail to point out the path by which we may arrive at more certain knowledge and more genuine truth, by providing us with an infallible test to try and prove (whether in observation or deduction), both our own thoughts and those of other men, and by bringing us, through a careful avoidance of error, to a more perfect comprehension of truth.

But those phenomena and processes in the living body which, under the action of the manifold influences of external nature, have ever been the especial object of medical experience, have in their nature nothing isolated, or tending to separate them from the general field of philosophical inquiry. What we are accustomed to call disease, is but a peculiar change or process in the living organism, under the action of some of those influences from without, which affect the maintenance, loss, and recovery of health; and these changes are a part—a very small one—of the great whole of nature. A given living being can no more be regarded as perfectly normal and healthy than as perfectly the reverse; the latter condition would evidently be equivalent to death. It may therefore easily be understood why we cannot here confine ourselves to those phenomena and processes which are the special province of the physician. A logic such as medicine is in want of will, in truth, be equally applicable to the whole wide field of natural science, and, indeed, to all the so-called experimental sciences; its doctrines and rules being essentially the same, whether they serve the physician or the physiologist, the physicist, chemist, or geologist, in the investigation of his particular objects, phenomena, and processes. Its application to questions and objects in the medico-physiological department is, however, of special importance here.

After what has just been advanced with regard to the aim and province of logic, as we here understand the term, there can be little doubt of its value to medicine, as well as to natural history in general. Whether the problems which these offer it can, in their whole compass, be solved by any one individual, and especially by the author of this work, is a matter of reasonable doubt; not so the essential nature of the service which their solution, if once effected, would render alike to the individual investigator and to the study of natural history and of medicine, whether considered as a science or as an art. Our object here is simply to establish truths and facts, so as to employ them correctly for certain practical purposes.

And these services acquire a double importance in a study such as medicine, where one of the most valuable possessions of man, his health, is the mediate or immediate object of investigation and treatment; and where, on the other hand, our comprehension and capability of action have attained to less certainty and infallibility than in any other department; for even to the present day, it is acknowledged that there is scarcely any department of human knowledge, in which the number of established truths and facts, which no one doubts or attempts to shake, is so small; in no other field are our rules of action so uncertain and disputed as in medicine, the noblest and most important of all arts.

The safest data which we possess are individual observations, or empirical propositions in reference to the surface of the living body, and to the occurrence and succession of certain phenomena and processes within it. Of their internal connection, on the other hand, and of the essential conditions of their existence, course, and cessation, we know, as yet, next to nothing. We have more or less admissible views and hypotheses concerning them, but no thoroughly understood and firmly established facts and laws—therefore, no system. And in medicine, as in all cases where personal views and interests, uncertain and often arbitrary theories, are defended or attacked, we sometimes find a rashness, or even presumption in the hypotheses, and a bitterness between the disputants, such as do not obtain in departments where a scientifically established, and, consequently, calm insight, has taken the place of mere opinion and subjective, *a priori*, conjecture.

Just as medicine and physiology have entered into closer connection with natural history in general, and with chemistry and physics in particular, so the methods of investigation and the whole process of demonstration in the latter have been more and more adopted in the former. Scientifically educated and thoughtful men now ask themselves other questions, and put their experience and scientific investigations to other tests than they were of old accustomed to do. But through this the discontent of many has but reached a higher pitch. We have seen faith in medicine—as an active agent—shaken on many sides, and a disposition to doubt and question has gained the more ground in proportion as what has been offered has the less corresponded to the demands of the increased intelligence and wider views of the age. A more exact calculation has in so far lowered the standard of our supposed wealth that it has seemed to us to be brought down to poverty itself; and thus one man, in his first excitement, has discarded, perhaps, too much of his previous knowledge, and despaired, more than was reasonable, of the powers of the human mind; while another has been oppressed in a still higher degree by the responsibility of his duties as a physician, and this through despairing, perhaps, more than was necessary, of his art.

But as one extreme generally calls forth and accompanies another, so has it also been with us. We have seen this scepticism and tendency to critical negation opposed by the greatest confidence, or even arrogance, often combined with the superstitions and mysticism of times long passed away. We have seen the circumspect and, perhaps, too timid physician, who endeavours to instil into his practice a truly scientific spirit, confronted with the boldest speculation, and with the most thoughtless and narrow empiricism: just as we see, in other departments, a boundless and injudicious admiration of, and adhesion to what is old, opposed, without discrimination, to all that is new.

It can scarcely be doubted, however, that the greater number of these disputes and errors have frequently arisen, less from the insurmountable difficulties of our subject, than from the want of a better understanding of the actual state of things. We have to blame the deficient employment of the necessary methods of investigation, and to accuse ourselves, in treating so delicate a subject, of a neglect of the doctrines of sound logic. And we must remember, in order to judge of the fitness of the present attempt, that many as are the difficulties of our subject, it is less to these difficulties than to our own want of caution, that we are indebted for so many false steps and failures. Against these, a careful logic would arm us, by imparting a clearer insight into nature, and into the special requirements and difficulties of our subject, while it at the same time provides us with rules for the critical appreciation of all our experiences and our theories. It not only exacts scientific perception, but also teaches us, D. V., more and more to abhor exaggerations and aberrations, in whatever direction they may point; and these will be the more carefully shunned, the more physicians and physiologists become imbued with, and elevated by its doctrines.

Many of my readers will perhaps think, that, after all, logic can but assist us to theorize somewhat better than before, or at most to clear up and facilitate our critical examination of the notions and views presented to it. Of what value, it may be urged, will be all the theorizing and speculation through which it would profess to guide us, when we come to practise at the bedside? Who has not heard so-called practical men say that medicine is a purely empirical science; that everything depends upon facts and correct experience; or, perhaps, that the power to cure is the main point? All arguments and theories, they say, do not enable the physician to treat his patients more correctly; in an art like medicine they rather do harm, or, at best, no positive good. It is there we are in need of experience—facts, and above all, remedies and their correct employment: all the rest is evil.

Further on I hope to point out what, in medicine and natural science, is the true signification of theory; and, while I establish a distinction between correct and incorrect speculation, to show how frequently these are confounded. But it is the very fact of this confusion which explains, and in some degree excuses, the mistrust of all theories and so-called systems, and brings contempt upon the idea of methodical progress in our science. It is unfortunate that there is, perhaps, in no other department, more real ground for such dislike and mistrust than in ours, where all theories and systems have so far proved deficient, and at least, in some degree, erroneous; and where the very nature of the subject induces a necessity of their appearing less certain in proportion to our success in making them more comprehensive, subtle, and profound. But these false theories, &c., have always returned to their nothingness, and often immediately upon their construction.

Yet the merely practical man, the so-called empiric, is none the less blameable in his feeling of repulsion and mistrust. Would he, who, on the basis of every insignificant fact, opposes mere experience to theory and speculation, seek to apply that which holds good of monsters and precocious births, to that which is brought forth after the usual manner? He who sees in theory and experience two antagonistic states or tendencies, only proves that he has by no means attained to a clear perception of the essential meaning of theory and reasoning. It may be sufficient here to state that the physician who seeks to understand the condition of his patient, and by the aid of such knowledge, to cure his disease, cannot, however earnestly he may endeavour to do so, accomplish this without theorizing. He who doubts this does not even know how impossible it is to reflect upon, *i.e.*, to form for himself any notions of even the simplest vital phenomena and diseased conditions, of the mode of operation of a remedy, or of any other occurrence in nature, without having already theorized and speculated concerning it—more or less correctly according to circumstances. Even the savage cannot observe anything in nature around him, *i.e.*, simply recognize it by his senses, without seeking to interpret and to form for himself some notion concerning it. Even he endeavours to determine its origin and the mode of its development; he assumes one thing as an effect, and another as its cause; though, perhaps, in the end, only some god or demon who, as he imagines, produces and controls it.

Let us take the case of a patient suffering from inflammation of the lungs,—a disease of frequent occurrence, and comparatively easy to recognize. The physician does not see this disease, however, any more than he does any other. He infers its existence from a number of local or general phenomena; and traces it through symptoms which he knows have been observed in essentially the same form, combination, and order in other persons suffering from the same disease. But yet he knows next to nothing of the conditions and causal connection—in short, of the exact nature of those phenomena or derangements which, taken collectively, he interprets as and designates "inflammation of the lungs." The most detailed and conscientious investigation of the physical, chemical, and objective, as well as subjective, symptoms in the chest, pulse, temperature, blood, excretions, or sensations, &c., in the patient, does not give him the least information on this subject, any more than the post-mortem appearances. And yet, every one who feels any interest in his science will wish to learn, and will therefore reflect, why his patient has been attacked by this disease, and what is its peculiar nature; what changes or derangements are essential or determining; and which of them, on the other hand, are dependent, comparatively subordinate, and accidental. But so soon as he thus reflects at all, and even the most strictly practical man cannot refrain altogether from doing so, he also theorizes, and that, for the moment, very much at random. He entertains, perhaps, certain views concerning the operation of frost, moisture, and what is termed "taking cold," upon the living organism; and, on the other hand, concerning plethora and similar general predisposing circumstances in his patient. With all this he will soon endeavour to account in some way or other for the development of the disease. He will further form certain notions concerning the origin of the fever, of the buffing of the blood, of its accumulation in the parenchyma of the lungs, and of their so-called hepatization, and of the relations these bear to the buffed, or otherwise altered blood, to fever, heat, and other analogous derangements or phenomena.

And if he now proceeds to the attainment of his chief object, viz.: treatment, he must ask himself how this state of things in his patient may most certainly and quickly be obviated. If he proposes to employ bloodletting, nitre, calomel, or antimony, cold, &c., he must again have known, or at least surmised, something of the properties and effects of those agents. He must expect, for instance, some favorable effect from them upon the composition and circulation of the blood; upon absorption, temperature, innervation, fever, or other derangements in his patient, or he would not, as a strictly practical physician, employ them at all. He must, therefore, have already formed some notion concerning the mode of operation of his remedies in this disease: in short, he must himself have theorized, or others before and for him, and that more or less correctly according to circumstances.

He who only observes his patients for the purpose of ascertaining approximately the species of disease, or, perhaps its mere denomination, and then prescribes his remedies, simply because he knows or surmises, from his own experience, or that of others, that they had done good service in similar cases,—such a one, it is true, does not require much theory. He does not require it so long as he is indifferent to knowing how and why his patients have fallen sick, and what is the nature of their disease. So long as it remains indifferent to him whether he treats his patients solely in accordance with routine rules, or on principles founded upon some comprehension of the things with which he has to deal as a physician; so long as he does not strive to understand why he treats his patients in this or that particular way, and whether the means he employs are beneficial or not, he need not either reflect or theorize. To such a one Sismondi's remark applies "that we feel no curiosity concerning things of which we know nothing." He relies simply upon the experience of others, or upon that which he calls his own experience, which, however, a Bacon or a Zimmermann would for the most part have declared to be a dubious or even an erroneous one. No doubt it is, in many respects, quite right to depend upon experience and facts only, but they must be actual facts, not, as they too often are, mere opinions or views. They must be truths founded upon experience, and not, as is so frequently the case, partial truths,—mere errors and illusions.

The very men who are in the habit of insisting most strongly upon experience, and accustomed most to rest upon the necessity of simple facts, without further speculations or theories concerning them, viz.: the so-called exclusive empirics, have often been least able to observe facts positively, (that is to say objectively), and to employ them correctly. They frequently do not even know what observation really is, and how much it includes. What they advance as their positive experience, as actual facts, are more properly their notions of them, and are often but illogical conclusions and interpretations. They conclude that what they have seen, or believe to have seen, with their own eyes, must be so in all cases, and do not understand that of this very fact, others may take a different view. This explains how one man may regard as an established truth what appears doubtful to another, and to a third even absurd, and opposed to the whole tenor of his experience. They acknowledge no scientific or theoretical authority, but substitute for it their views at the moment—their *ego*. To proceed, who has not heard the practical man say that our object is to cure, not to theorize? In a locomotive engine, the object is to set itself and the carriages in motion; in a watch, again, the object is to indicate the time; but no one will assert that the face and hands are the main parts of the machinery of the one, or of the other, the turning of the wheels, or the rise and fall of the piston. For the patient, it is true, the recovery of his health may be all that he expects from medicine; to the physician, on the contrary, it is the final result of his knowledge and power of action, a keystone which presupposes many other things; a fact, in short, to be made use of, which he will not be able to do, unless he has previously acquired a certain insight into his science and a practical skill in his art, based upon correct experience and precise knowledge. But so long as physicians can appeal to nothing more certain in their practice, in their success or failure, than their experience at the bedside, every new patient, every fresh experience, may shake or contradict their views. They must be content to let every one else, even charlatans, appeal to *their* experience; and inasmuch as the latter can often avail themselves of the same argument, the *post hoc propter hoc*, which the physician employs for the success of his operations, viz. the recovery of his patients, the sick will necessarily place the same confidence in, and award the same thanks to them, as to the most able physician. Science, in the full signification of the word, can, in medicine, mean nothing more or less than the application, on the part of the practitioner, of a certain knowledge to certain objects. It presupposes, therefore, a certain knowledge and comprehension; or, rather, it is in itself *knowledge arranged and employed in the manner best adapted for the attainment of his ends*, i. e., *for practice*. It groups for him the doctrines and propositions of his system, and combines them, so as to enable him to found upon them his rules for the treatment of his patients. It is on this account that a sound logic is indispensable to every art, and especially to that of medicine, for, as we cannot conceive of the existence of any true art that is not founded upon knowledge and understanding, so, on the other hand, there can be no more valid demonstration of the accuracy of any knowledge, explanation, or theory, than that it furthers art, *i. e.*, practice, as well as insight and comprehension. Thus logic facilitates the physician's comprehension and appreciation of a given case of disease and of its treatment, and shields him, on all sides, from error; as in the words of Bacon: "inter signa nullum magis certum aut nobile est quam quod ex fructibus."[1]

And further, while, as has been said already, the chief stress is laid, and in some degree correctly, upon experience and facts, it is formally asserted that we should confine ourselves entirely to *them*, and attempt to classify *them* only. We must, it is said, observe and describe all the phenomena and processes of health and disease, according to their actual appearance, without any subjective aid, whether in the conception or interpretation of them, or in the comparison and classification of them with others. And this must be carried out with a resolute independence of all theoretical views, and without allowing ourselves to be influenced by any dogma, preconception, or peculiar mental tendency; as if this were in any way possible for man in the present state of his intellectual being! But this idea is most intimately connected with another still more erroneous,—one daily to be met with among the empirics, and even among many of the Coryphæi of medicine—the belief, namely, of its being possible to arrive, by mere observation, and by the collection and accumulation of observations, of so-called experiences and facts, *per se*, at a certain medical comprehension and science.

That this is entirely impossible, will, however, be explained at a later period.

Thus our ancestors, for instance, as well as the uncultivated inhabitants of all countries, constantly observed the wind and the weather, and contemplated the heavenly bodies and their course night after night, for centuries, without thereby ascertaining anything either of the connection and conditions of the weather, &c., or of the laws of motion of those heavenly bodies and of their own earth, or of the distance and magnitude of the former. Neither have physicians, with all their observation and palpation of their patients during centuries, gained a true insight into their real condition—an insight which we shall

[1] Novum Organum Scientiarum, etc., lib. i, aphor. 73.

never obtain by mere observation, whether in the way of auscultation, percussion, or mensuration; or even by our autopsies and microscopical and chemical examinations, be they ten times as minute and correct as they are at present. These observations, this collection of empirical facts, are no doubt important enough, but only as the first step,—a step which it would be equally erroneous and absurd to regard as the last. Even Bacon, who has in some degree encouraged the error in question by his almost too partial insisting upon observation, positively declares the mere accumulation of observations or empirical facts, to be a "res puerilis."[1] He asserts that our gain, in the attempt to deepen our scientific insight by means of such accumulation only, would be about equal to what we should acquire by setting ourselves to learn a diary by heart.

But the fact of experience being regarded by so many as a substitute for knowledge,—nay, even as being in itself both knowledge and understanding, has further encouraged the illusion of our being already rich in knowledge, while we possess, at the most, but the raw materials, out of which, at some future period, perception and true science may spring. It is not the heaping together of individual experiences and facts, but the understanding of them, that constitutes knowledge; and it is because there is in no other department of science such a dearth of this understanding that medical science remains behind all others. We are not deficient in preparatory materials,—for these experience has amply gleaned; but in that due understanding and arrangement of them, through which alone they can be employed and made available, whether in practice or in the construction of theories.

Thus, even in the present century some have believed that they had discovered the essential causes and ultimate nature of diseased conditions in certain anatomical changes in the organs: others have found them in certain chemical changes: others, again, in certain derangements of innervation. Yet all of these have rested their assertions upon experience, and appealed to it for their confirmation, but yet have not succeeded in demonstrating the truth of their theories, because, for the reasons already mentioned, it remained an impossibility.

This erroneous estimate of the value of experience in itself,

[1] Lib. i, aphor. 102, 105.

has not only involved a misapprehension of what, in medical science, remains our especial desideratum, but it seems to be the ultimate cause of the further circumstance that we have often omitted first to search for what our understanding and knowledge alone might have established.

We have only, however, one mode of arriving at the sure understanding of any natural phenomenon or process—*e. g.*, of a disease, or the operation of an influence or a remedy—viz., by bringing these obscure phenomena and processes themselves, as it were, more within the scope of our comprehension. We must first seek to ascertain their conditions and effects, to trace their causal connection and conformity to fixed laws; we must be able, by the aid of a carefully constructed theory, to reason consistently concerning them and, above all, we must be provided with scientific demonstrations of the accuracy of our views. But if we have once succeeded in the formation of such a theory, we have gained, in that particular department, that of which we must otherwise remain destitute viz., a science. To this goal, the nature of our intellectual being urges us almost instinctively, because the goal itself is a natural one. No one capable of reflection, and possessing an intellect in any degree active, will content himself with the mere observation and sentient perception of any phenomenon or object whatever. He will not only seek, by the aid of perception and observation, to interpret the phenomena or objects in question, but his observation will pass involuntarily, and in accordance with innate laws of necessity, into a more profound contemplation of them. He reflects upon them, and compares what he observes here with what he has observed elsewhere, noticing their similarity, or their distinguishing features. He strives unconsciously to digest and master, as it were mentally, what he has observed, by seeking for the conditions of its development, for its causal connection and laws. Mere observation, however, would not suffice to satisfy his yearnings. Being conducted by means of the outward senses, it can but aid our perception of things exterior, it does but acquaint us with this simple fact—that they appear and present themselves in a certain form. It falls far short, therefore, of such a comprehension of them as our whole nature urges us to seek for. This applies also to the phenomena and processes in external inanimate nature—*e. g.*, to wind and weather, to light and colour, &c.—as well as to those of living nature, whether in health or disease.

But while we obey this impulse, and inquire into the connection and causes of these phenomena or processes, we must form for ourselves some notion of them, and, as it were, render a certain account of them to our minds—whether a correct or a false one matters not at the moment. The so-called practical man, or empiric, theorizes also at that moment, whether he intends and acknowledges it or not. In this sense we may say, and others have already said, that to theorize concerning such things means nothing else than to reflect upon them. But this in all cases presupposes our own subjective co-operation, and in the present state of things, in medicine proper, this can never be carried out without rash hypotheses, and an advance, more or less arbitrary, in our interpretation of what we have observed, in the separation or combination of its individual parts, and in the abstractions or general conclusions with which it furnishes us.

We have, perhaps, said enough to satisfy the reader, if not convinced beforehand, that in medicine, as well as in the general study of nature, we cannot keep asunder theory and practice, or separate our experience, *i. e.*, our view of the actual state of things—from those our subjective notions concerning them, which aim at their right understanding and explanation. Let us at once admit that all artificial attempts and operations, what is called practice, is itself, fundamentally, nothing less than the result of theories more or less detailed and comprehensive. For our practice is, after all, but the more or less conscious application, either of our own general views, or of those furnished us by others; *i. e.*, we take the principles and generalizations deduced from certain individual experiences and cases, and apply them to any given case. We judge of this individual case, we interpret and treat it in one manner or other, conformably to those more general views, or principles and theories, because, without some such *a priori* groundwork for our operations, we could neither form a correct notion of it nor treat it consistently. Our grounds of action, however, whether sound or otherwise, are furnished us by theory, and the sum of them—arranged in a certain form—is, in the end, synonymous with theory itself. This shows us plainly how injurious, in such a study as medicine, the long existing dissention between empirics and theorists must have been to the advance of art as well as of science. This strife between theory and empiricism, between speculation and sober investigation, exists now, and meets us on either side in its extremest form, just as it has existed for centuries; because it has its root in the different dispositions and tendencies of different men. As nature teaches birds to fly, and other animals to walk or crawl, so the natural tendency of the mind, and subsequent experiences and circumstances, lead one man more especially to speculation and theory, another to the sober and circumspect accumulation of facts, and a third to performance. The position which one man takes, or seeks to take, in the field of thought, conception, and mental combination, another will, perhaps, take in the field of action. If physicians, however, had always clearly understood what theory and experience, science and practice, essentially mean, how they all blend with, and are supported by each other, and can only thrive in intimate connection, many bitter disputes would never have arisen. Reflecting and well-meaning men would have been less ready to place them in opposition, or to be led through an unfounded prepossession for the one, to the undue disparagement of the other. If physicians had always had a clearer comprehension of the questions here at issue, and which, when answered, can alone decide, they would not for the mere sake of what simple experience and practical tact can give, have overlooked the value of theories and the indispensability of their services, or have allowed the possibility of deficiencies or errors in them to have diminished the importance of such aid.

If it is acknowledged that every theory not firmly based upon experience is built upon the sand—and liable to be thrown down by the first wind, and that, from this cause, all theories in the science of life, and especially in medicine, have hitherto been defective, and will, perhaps, always remain so—it is equally certain, that without theory there is no genuine progression. Even the first imperfect attempts to theorize, the first uncertain steps towards scientific perception, have rendered infinitely more service to physiology and therapeutics, and furthered the progress of the human mind in the dark chaos of investigation, more than would ever have been attained by adhesion to simple empiricism. Such theorists and speculators often, perhaps, reason too abstractedly, too much *a priori;* they may be even dreamers and visionaries. But, in the end, they have always been those who have contributed most to decide the fate and the progress of sciences, and, amongst others, of medicine; for they have laid open new veins of knowledge, which the mere empiric, as such, would have been no nearer lighting upon than would the diligent collector and labourer in the sweat of his brow. Every new discovery is a species of creation, almost in the same sense as, for instance, every poetical fiction is. And though such a discovery does not produce the thing itself, but only conveys to our minds the knowledge of its existence, theorists have extracted the honey from the materials often collected by others. If their discoveries have frequently been set down as refuse, some grains of gold have, perhaps, eventually been found therein, some truth worked out, or, at least, the way prepared for doing so. Theorists have often surmised the truth, and led to its discovery through hypothesis and combination, while its demonstration remained for sober and more patient investigators. If, on the other hand, theorists had always understood with sufficient clearness that the true object of speculation is not the conception of an idea, not the indefinite surmise and vague assertion of a truth, but the establishment and actual demonstration from experience of a certain connection and conformity in nature, they would have been less averse to labour and investigate for themselves. In becoming aware of the means by which alone they could safely carry out their hypotheses and abstractions, they would have seen the sooner the necessity of investigation in the direction and by the methods through which alone they can furnish themselves with fit materials to work upon; and instead of being in such haste to make known their views upon first adoption, they would have the rather striven, by adequate investigation, to convert their *a priori* surmises into infallible truths or laws, and have thus given to theory a higher value than it has hitherto acquired. They would do well also to bear in mind that with all our philosophy and speculation, when these are left to work alone, we have never arrived at genuine truth and perception in the study of nature, or even at a single discovery, and that they can but guide us by hints and guesses to investigation, and then aid us in forming a correct estimate of its results.

What has been said will explain why the ancient Egyptians, who regarded every discoverer as worthy of being worshipped, set up infinitely more idols to animals than to men; for animals, by the aid of their instinct, have not only made many discoveries, but have put men upon the track of many more, which human philosophy and speculation would not easily have made. And so it has continued to be to the present day. If we had waited till some one, entering by scientific insight into the proximate and remote conditions, into the essential nature of ague and bronchocele, had hit upon the treatment of them by cinchona, burnt sponge, and iodine; or, conversely, until he had inferred the efficacy of these substances in those diseases, from his insight into their properties, and the mode of their operation, no one would yet, or so far as we can now judge, for centuries to come, have been cured. The very fact that accident, or simple experience, has hitherto furnished medicine with its most important, perhaps its only materials for practice, may, in our peculiar department, serve as a good check upon the self-conceit of *a priori* speculation and philosophical abstraction.

It has been, on the other hand, a no less great and hurtful error on the part of the empirics, and of not a few physicians and natural philosophers, to suppose that they can dispense with the aid of logic in the observation and determination of the materials which experience furnishes; or in that accurate deduction and generalization in which the scientific appreciation of facts and observations consists. For many are of opinion that with a knowledge of the individual facts and doctrines of their particular branch of science, that sort of acumen which goes by the name of common sense and natural tact will furnish all the rest. Unfortunately, however, the history of medicine—one of the most melancholy of all histories—plainly shows into what errors and exaggerations even those whose "common sense" no one would be inclined to doubt, may nevertheless be led. No man can attain to the logic that we require, or know how to employ it correctly, any more than he can understand any art or science—as it were *a priori*, of himself.

As we must learn the use of our legs before we can walk properly, so, regarding logic as an instrument,—complicated indeed but useful—we must first understand and be able to manage it properly, before we can employ it to due advantage. In this a man's natural capability may assist him; he may be able to apply the rules of logic, and to a certain extent successfully, without being conscious that he employs it at all. But to reap from it all the advantages of which it is capable, he must advance yet further in the comprehension of its nature, and especially in that of its application.

I have already endeavoured to point out that many of the impediments to our progress lie less in the intrinsic and inevitable difficulties of our subject, than in an injudicious choice of our objects of investigation, and in an unproductive, if not absolutely erroneous, method of conducting investigation in general. Such failures and aberrations pre-suppose a deficient insight as well into the end in view as into the means by which we propose to attain it, the cause of which we must seek in the want of logical proficiency. In a study such as medicine, we can take no single step; we can neither observe nor determine anything accurately, nor undertake anything for the cure of disease, without at the same time feeling the necessity of forming some notion or inference, and giving both to ourselves, and, if need be, to others, some explanation of what is before us. But to attempt to accomplish this on the mere ground of experience, and with proofs whose very nature is dubious and fluctuating, must be a rash, and may be a fruitless and purely illusory undertaking.

Physicians in particular, and many other natural philosophers, do not seem at all times able to track out the difficult and devious paths by which they are accustomed to arrive at what they briefly term their experience. Even in respect to observation, they seem to distinguish less than is to be desired between what is positive, and refers solely to the things themselves, and what their minds construct out of these given facts. Many cannot even distinguish clearly between the thing observed and their own conception or interpretation of it, but deal with their notions, abstractions, and allegorical phrases as if they were tangible realities. It is dangerous, however, as we but too often learn from daily experience, for the mind of man to be left to deal arbitrarily with such an obscure and chaotic subject; to leave the discovery or non-discovery, the comprehension or non-comprehension of enigmatical phenomena to chance, instead of putting definite and clear questions to nature, and of so arranging everything as to obtain, if possible, decisive answers. Medicine, as it is at present practised at the bedside, does not, it is true, demand abstruse reflection. Almost everything flows from the memory, and from simple, often superficial reminiscences. Most of what is to be done is confined chiefly to the simple sentient perception of certain appreciable phenomena: we are guided (under the influence of proximate conclusions, or mere surmises concerning their condition and character) by the combination of these phenomena with each other, or by comparing them with other cases of a similar kind with which we have been furnished by experience. We are accustomed to choose our remedies and treatment without the aid of profound calculation, and only in accordance with general—often arbitrary and vague rules and doctrines, and, in the end, more from memory than from a comprehension of our subject. We might even say, perhaps, that men have expended more acumen and labour upon the interpretation of writings in cypher, or even upon many games, and certainly with more success, than upon the interpretation, prevention, or cure of diseases which may, at any moment, prove fatal to themselves, or to those around them.

But if any improvement is to be effected in medicine,—if the physician would approach to a certain insight into what he terms health and disease, and judge rightly of what concerns the maintenance of the one and the cure of the other, he must strive to the utmost to give to medicine a scientific character, and must be, therefore, in the first place, himself convinced both of the necessity and the possibility of doing this. Instead of contenting himself with the fact that phenomena vary and run a different course in different patients, —sometimes disappearing under one mode of treatment, sometimes under another, he must seek, with unremitting perseverance, to gain positive grounds for his views and active interference, that he may expect in future to be able to give, whether to himself or others, a more correct account of them. He must gradually acquire a clearer notion of that in which his knowledge and capability of action consists, and for this he requires a definite idea of the objects submitted to him for examination: he must know their characteristics and weigh their special demands upon his methods of investigation; and to this end he needs a clear insight into the ways and means which can alone lead him to a comprehension of them, and equally into the others which can never render him any assistance. What a strange thing is the mind of man! In proportion to its activity and progress, he is capable of penetrating into the most hidden depths of nature, and of inquiring into her laws; he calculates with certainty the motions of those distant heavenly bodies which are scarcely discernible by the telescope; he weighs the earth to which he does not even stand in the relation of the millionth part of an atom, and subjects forces of nature to his will, which his ignorant fellow-man at the best fears and gazes at, as at powers above his control. On the other hand, the same human mind, through want of development, or through an erroneous employment of its powers, while it receives absurdities as established facts, may overlook the simplest truths, or even declare them to be illusions.

If we then, as physicians, have any desire to approach nearer to our object—a certain insight into the things with which we have to deal—we must not omit, above all, to proceed methodically, *i.e.*, according to the doctrines and principles of a sound logic, in our observations and investigations, as well in the scientific appreciation of facts, as in the conclusions we draw from them. We must regard this as the first indispensable step towards our end, and be thoroughly convinced that this effort is not less important for our practical purposes, as physicians, than for scientific investigation. He who understands nature can alone hope to rule it, because he can only do so by a knowledge of its laws. Such knowledge is, however, very difficult to acquire, and the main object of our logic here is to make us acquainted with the intricate paths or means through which we may be led to it. The man who possesses a genuine talent for the investigation of nature may, perhaps, dispense, in a certain sense, with its aid and with its methods and laws, but even he can only do so because his peculiar nature and his instinct make him in a manner independent of it, by leading him, unconsciously, to employ it correctly. He constructs his own rules, he seeks and discovers his own path. Geniuses are, however, rare, and the mass of ordinary labourers in the field of science probably effect more than individual prodigies. But these will accomplish yet greater and better things, by a more methodical mode of progress, and a stricter adhesion to a correct mental process; and may thus raise themselves from the mass of handicraftsmen and day-labourers to the rank of scientific investigators.

Every special and methodical attempt at guidance towards this end has, however, hitherto failed. Little as the more general abstractions or doctrines of ordinary philosophy and logic are capable of furthering our ends, the axioms and suggestions that are daily offered to the investigator and physician fall equally short of rendering us any positive service; such, for instance, as these:—that we must observe correctly and impartially; that we must first interpret our observations, and then draw from them the conclusions to which they legitimately lead; that we must avoid all prejudice and one-sidedness; and the like trite and trivial instructions. We rather require an exact description of our problems, as they are modified by the peculiar nature of our branch of study, and of the special objects and questions with which it has to do; we need a detailed explanation of the various ways and means which can lead to the solution of those problems, and of their relative advantages and dangers. In short, as we require the most exact guidance possible for our progress in the investigation of truth, we must, first of all, observe and understand the human mind, as it is accustomed to proceed in the examination of natural objects.

To fulfil this requirement, as far as may be, is the object of logic, in the sense in which we here understand, and wish to employ it. Its office is evidently, not to discover and divulge the truths and doctrines of any science or art, *e.g.* of medicine. It no more pretends to unravel at one stroke the mysteries, enigmas, and questions of our science than to render a stupid man clever. To any one who understands the nature of logic, in the sense which we here attach to it, it will be plain enough that an immediate solution of the problems of medicine, or a speedy discovery of many of its truths and laws is not reasonably to be expected. We have had many men, it is true, who have asserted that they could more or less completely accomplish all these tasks, and give to medicine a scientific basis, and many more who have brought forward their own theories and doctrines as established truths. But to all who have acquired a clearer comprehension of this subject, it will be evident that every one who pretends to have accomplished anything of this kind, in the form, for instance, of a system or correct theory, does but mislead himself and others, and may thus be regarded as, in some sort, a deceiver, or as ignorant of the true nature and aim of logic. Medical logic does not profess to acquaint us with the truths, facts, and experiences at which our science has already arrived in various branches. Its office is not, for instance, to explain the nature of respiration, or digestion, of an inflammation of the lungs, or typhus; it neither instructs us with regard to their symptoms, nor gives us the reason of their occurrence; it does not tell us what to set down as cause, what as effect; it neither enlightens us as to the origin of diseases, nor guides us in their recognition, prevention, or cure. It does but profess to acquaint us with the real value of the ways and means by which we are to acquire an insight into these phenomena and processes, and to warn us of those which promise but fail to impart it. It enables us to judge whether our choice has fallen upon the right methods by which to proceed in a certain investigation, and gives us data whereby to test the correctness of what physiology, for instance, would teach us concerning respiration and digestion, or medical science impart concerning inflammation of the lungs, or typhus fever, and the most efficient mode of treating such diseases. This information would be of considerable value did it aim at nothing higher than the more accurate calculation of probabilities in the case of empirical rules, for just such a guide will be needed so long as the most dissimilar, often antagonistic, views and doctrines lay equal claim to truth, recognition, and belief; so long as it is very difficult to decide whether the theory of A, B, or C, may be most correct; and which are the instructions that may be most safely followed. In like manner, a further insight into all the methods and auxiliary means for the attainment of experimental truth, and a suitable guidance in the employment of them, can nowhere be more important than in a study whose difficulty consists, and whose object lies, not merely in observation and deduction, but in the manner in which the mind performs these necessary offices, and makes for itself a judicious selection among many possibilities. For the necessity of a clearer comprehension of every individual question and case, *i.e.*, of a certain practical logic, is nowhere greater than here, if the investigator is anxious to fulfil all the requirements of his science and of his duty as a practical physician.

We cannot, it is true, expect that every natural philosopher and physician should be a finished philosopher and logician. But we are justified in demanding so much philosophical insight and proficiency as is necessary for them to proceed according to the rules of true science in their own department; we may claim a logical acumen sufficiently nice for the correct observation of such facts as may come under their own experience; sufficiently clear to guide their discrimination of such as they accept upon the testimony of others; and sufficiently exact to conduct them, in both cases, to a due working out of results. We cannot, perhaps, expect of the physician, any more than of the general natural philosopher, that he should himself solve some even of the most important questions and problems of his department, or that he should understand and know more than his whole science, taken collectively, knows. But we may expect of each individual that he should recognize, above all, what his science ought to know and be able to teach him, if it is really to serve him as a science; that in his own department he should know, therefore, what explanation, knowledge, and comprehension, essentially mean. We are also justified in expecting that he should not regard anything as an explanation of his questions and problems, or as a comprehension of them, which is not truly such. It would thus, for instance, scarcely be unreasonable to demand of the practical man and of every physician, that he should no longer, as has so frequently been the case, apply to the anatomist, the physiologist, and the chemist, for the explanations and precepts for which he must be indebted to himself, however much he may found them upon his own physiological and chemical knowledge.

We may demand of him a sufficient insight into his own subject to be aware that no single individual—though he were the greatest and most successful genius—could impart to medicine a scientific form, a knowledge of the conformity of the phenomena and processes submitted to it for investigation, or to therapeutics a comprehension and a certainty such as, even with the aid of the most diligent and circumspect investigation and labour of all the physicians and natural philosophers in existence, is scarcely to be looked for for centuries to come. He would run less risk of being constantly the dupe of those who disseminate, from time to time, their various theories and infallible systems of treatment amongst the medical public. And if the physician so frequently laments that little progress is made in medicine; that it has gained but little in scientific and accurate knowledge from the labour of centuries; that his art, instead of acquiring, by cure and prevention, more power over disease, has gradually declined in credit; it might fairly be expected that he would inquire into the reasons of all this, and strive to ascertain why medicine, instead of progressing with its sister arts and sciences, has remained in the same position as astrology or alchemy, instead of assuming the scientific character of astronomy or chemistry. He would further seek to understand why his notions of the great majority of his objects, and even his language and technical terms, are obscure, fluctuating, and arbitrary, and are used in a manner befitting, at best, that priesthood with whom his art, amid the dark mysticism of ancient times, originated.

It is not without good reason that the practical physician complains that quacks of every description, and even unprofessional men, participate in the confidence of his patients. What has been thus expressed by the Preacher, has been, with regard to us, the thought of many:[1] "Then said I in my heart, as it happeneth to the fool so it happeneth even unto me: and why then was I more wise?" Such complaints are, however, evidently useless, and seem the more out of place here as the physician has the remedy in his own hands, and has but to help himself.

The first step for us all in this direction is, no longer to

[1] Ecclesiastes, chap. ii. Compare Fr. Baconi de Verulam, Opera omnia: de dignitate et augmentis scientiarum, p. 104; Frankfort, 1664.

regard our poverty, the reality of which has already been explained, as wealth, or our weakness as strength, but to test whether, besides the natural difficulty of our subject, some blame may not also be fairly cast upon the faultiness of our starting-points and methods, and upon the defective employment of all our auxiliary means. We must seek more and more, by an insight into the peculiar nature of the problems, questions, and objects submitted to our examination, to discover other, perhaps more productive, methods of investigation, and to avoid, more carefully than we have yet done, the errors committed by our predecessors.

But these and similar questions obtrude themselves in every branch of knowledge, and in every art which has hitherto failed to make any safe and notable progress; for here also it may be said "by their fruits ye shall know them." I can but indulge the hope of in some degree facilitating their answer, and, if I am not so fortunate as to attain to the solution of the problems which medicine, whether as a science or an art, holds out, I may at least stimulate others to aim at, perhaps to accomplish, better things in the paths I shall endeavour to open out. And the result of this attempt, so far from furthering an inclination to scepticism and mere unprofitable criticism, or leading to the depreciation of the healing art, will tend in the precisely contrary direction. The attempt itself has been the further urged upon me by my profound conviction of the infinite value of medicine to our common humanity; and by the firm belief that it can more and more attain its almost god-like end if it did but make the attempt with sufficient earnestness and consistency. But it has seemed to me that the first step, necessary to all that follow it, is this: that medicine should emancipate itself, more than it has yet succeeded in doing, from many of the trammels and swathing-bands of its infancy, and strike out more and more into the paths by which alone it can arrive at its goal, and assume the rank of a more exact science and more serviceable art.

These paths are by no means new ones, and I, at least, make no pretension to having first discovered, or proposed them. They are fundamentally, *mutatis mutandis,* those which have been adopted in other natural sciences and arts which have long outstripped our own. I am, however, not without the hope of being able to contribute something towards the better explanation of what, in our peculiar department, yet needs to be investigated and proved; to show what, in a scientific point of view yet remains to be accomplished; and to indicate the means whereby such ends may be most fitly attained.

My professional brethren will perhaps not take it amiss that one of their body should attempt, what to the best of my knowledge no one has yet attempted: viz., to explain clearly and simply, without philosophical pretension, what science and precision really mean; to arrange scientifically and logically for the numerous diligent labourers in the field of medical investigation, all the requirements which every observer and practical physician must fulfil; and especially to attempt to provide them with the ways and means by the aid of which they may best do so.

This leads to the final question whether our logic can render any service to our younger brethren, and, above all, to the student. I have this point the more at heart from the fact of this attempt being rather intended to serve the younger than the older members of the profession. The former in all cases represent the future,—the future, therefore, of a science or an art. It may even be said, that the past,—so-called older times, are, in a certain sense, the younger,—the first childhood of a science; while, conversely, their subsequent development, *i.e.*, the youngest,—most recent period, represents that science in its riper age. Those who come after us must therefore seek to attain a higher degree of scientific knowledge, than we, their predecessors, have attained to; and to do this they have but to make the attempt in a serious spirit. They must, above all, accustom themselves to investigate and appreciate what they meet with at the bedside and elsewhere, more exactly than has hitherto been done. The period of mere belief and arbitrary, more or less unprofitable speculation, appears to have passed away; and that of positive investigation—of clearer and more dispassionate reasoning—to have taken its place. It can therefore no longer appear indifferent to any one whether or not he has learnt to proceed and judge more correctly in matters and questions of natural science, and especially of medicine: he must feel anxious to obtain a finer proficiency in distinguishing the true from the doubtful, and this again from the altogether erroneous: he must aim at obtaining clear and connected explanations of the methods of investigation in his department, and learn, by recognizing his own errors, to shun the rocks upon which thousands before him have made shipwreck. If our predecessors had arrived at a truer estimate of this sober, practical logic, they would not have regarded so many impossibilities as possible; or viewed so many uncertain hypotheses and hollow theories in the light of genuine truths and established facts; neither should we have been so ready to believe them.

It is naturally the inexperienced man, the beginner, who is most ready to accept whatever is offered him as established truth, and this cannot but be dangerous in such a study as medicine, where we have scarcely one scientifically ascertained and demonstrated truth, scarcely one established and universally applicable rule for practice; where almost all depends upon the circumspect testing and appreciation of an individual case or question, and much even upon mere instinct. The student, before proceeding to the study of medicine, has, perhaps, become acquainted with other sciences, has learnt to appreciate them, and acquired a certain taste for scientific investigation. He now plunges into the individual special branches of medical science, each of which imparts only its own series of objects and doctrines, and that often drily, and without any allusion to their intrinsic connection. He now feels himself almost crushed by the mass of individual, and apparently trivial facts, amongst which, deficient as they generally are in scientific interest, but little is to be found for comprehension. Or—what is worse—his teachers and authors dispense their vague doctrines, opinions, and *a priori* abstractions categorically, and vend them to him as so-called science and truth. Their time and, possibly, their inclination fail to point out the paths by which the branch of science in question has arrived at its ascertained results and principles, or to give him a connected explanation of their intrinsic reasons and proofs.

Thus the clearing away of certain mole-hills from the path of the beginner does but leave whole barren mountains standing in it. His memory is, perhaps, filled with a confused medley of innumerable facts, but he has yet to learn and practise what every natural philosopher and physician especially requires, viz., the habit of clear and circumspect investigation, correctness of insight, and facility in experiment. He has, no doubt, learnt much, but not yet how to seek so that he may find; he has, perhaps, become acquainted with philosophy without learning how to philosophise. Herder complains almost too bitterly when speaking of the Schools (Facultäten) that "youths, on leaving the university, are for the most part perverted growths; they have been taught to create their senses and reason *a priori*, rather than to employ those created for them."[1] And if this should, even now, sometimes be the case, the beginner, as well as the more experienced physician and natural philosopher, must, above all, learn to despise and reject the kind of aliment which is offered to him, if he would hope ever to obtain a better and more nutritious one.

In this respect, also, medical logic may be regarded as designed to fill up for the student and embryo natural philosopher an important gap in the course and connection of his studies. Its office is to teach him just what his various special branches of science have failed to teach, and indeed what it was not in their power to impart. It further groups together for him all those requirements of true natural philosophy and science, which their individual branches must fulfil, and to which they must conform before they can acquire that scientific character to which the progress of their development tends. This again will naturally recall to the mind that connection of medicine with nature in general, that intrinsic oneness of all these detached sciences and branches of science, which is so often lost sight of in common life. And yet perception and progress in each of the individual departments can only thrive by constant attention to, and restoration of, their intrinsic oneness; for as no plant torn from its roots can maintain a healthy state of existence, so medicine can never thrive and expand unless it maintains its natural connection with the investigation of the whole field of nature.

[1] Herder's Works; Stuttgard, 1830; vol. xvii, p. 207. As this passage clearly admits of more than one reading, the original is annexed: "Die von der Universität abgehenden Jünglinge seyen grossentheils verderbte Gewächse; man habe da wohl ihre Sinne, ihre Vernunft *a priori* schaffen, nicht aber die erschaffenen brauchen gelehrt."

SECTION I.

MEDICAL PROBLEMS IN A LOGICAL POINT OF VIEW, I. E., IN REFERENCE TO THE POSSIBILITY OF SOLVING THEM.—TOPICS AND QUESTIONS WITH WHICH LOGIC HAS TO DEAL, BOTH AS A SCIENCE AND AS AN ART.

WE have already stated, in the Introduction, that the precise object of this work is simply to set forth the means and processes for the attainment of experimental truth in medicine; that it is merely the arrangement of certain rules and principles for the investigation and due appreciation of the various objects presented to the notice of the physician. To facilitate this object, and more especially to obtain an insight into the nature of the means of its accomplishment, we must first consider what are the phenomena and their mode of occurrence; or in other words, the objects themselves the investigation and comprehension of which is our special aim. For, before we can be in a position clearly to understand the entire process through which the human mind has gone—the method of observation adopted, and the nature of the deductions and generalizations that have been made—in short, the methods of investigation and demonstration most appropriate for this department of science, we must ascertain and accurately appreciate what it is that has to be observed, investigated, and proved. We must first of all determine what are the phenomena and processes to be recognized, and ascertain their nature, operation, and causal connection. We must scrutinise the nature of the therapeutical operations and influences, ere we can advance any definite propositions in therapeutics. We must endeavour to understand the peculiarities of the phenomena which modify the methods requisite for their investigation.

Medicine—in the signification which we attach to it here—is, however, divisible into scientific and practical. Of the latter, the precise object and final aim are the prevention or cure of disease, and the maintenance or restoration of health. Of scientific medicine, the object is the comprehension of all that relates to the invasion and progress of disease, as well as to the period of convalescence, and the plan of treatment. By its study we are to gain a clear insight into all the phenomena occurring from the last moment of health to the time of its restoration. Its first object, therefore, is to acquire a correct knowledge, founded on scientifically accurate experience, of all the conditions and influences which combine to induce disease, or recovery. In other words, it must convey to us a correct perception of the conditions and laws of the maintenance of health, as well as of the various diseases and their cure. Medicine can only be regarded as a science in so far as it demonstrates, or attempts to demonstrate, the natural and regular connection of the phenomena and processes that are presented to our notice, and then gives such explanations of them, and advances such propositions in relation to them, as are founded on experience. As an art, medicine, like every other art, speaks in the imperative. It tells us what to do and what to leave undone, in order that we may best attain a given object, *e. g.*, the maintenance or restoration of health. As a science, it speaks like every other science, in the indicative. It sets forth certain propositions in reference to those phenomena which it is its province to investigate; it tells us that they have certain definite relations to each other, that they occur in a certain manner, and are subject to certain conditions. However much, according to this view, these two branches of medicine may seem to diverge, they are, nevertheless, intimately connected. The primary object of therapeutics, as a practical art, is to prevent, or counteract the influence and deleterious effect of all those conditions or agents which induce, or maintain, a state of disease. Should this have been impracticable, or, as too frequently happens, should no such attempt have been made, its next object is to render available all those means, by the aid of which the return of the living being from a diseased to a healthy state may be facilitated and effected. To do this with any degree of certainty medicine presupposes a knowledge of man and his requirements, and of the various incidents and influences, external and internal, which affect him and his state of health, and on the co-operation of which the maintenance, loss, or recovery of health depend. It is, therefore, not only necessary to determine precisely how the phenomena and processes of a healthy or diseased organism are manifested externally, *i. e.*, how they are presented to our observation both in reference to characters appreciable by the senses, and to the order of their occurrence, but also and especially to ascertain their essential nature and the conditions to which they are subject. We must, therefore, learn by experience their causal connection with each other, and with the influences of the external world. All this knowledge, necessary for practice, it is the province of medicine, as a science, to impart. In order to promote this object all the principles or doctrines of scientific medicine must not only be correct in themselves, *i. e.*, conformable to truth, but they must at the same time be such that practical medicine can with certainty deduce from them its rules and precepts.

Medicine was originally something very different from this. It arose, as the offspring of necessity, from the instinctive impulse of man, in common with all animals, to maintain a state of health; or, when sick, to free himself from his maladies and sufferings. The most ancient designations of medicine, as ἰατρικὴ, ars medendi, pharmacology, point out this. Its whole object was purely practical and technical; its whole substance consisted in a collection of rules and precepts for the preservation or restoration of health, *i. e.*, for the prevention or cure of diseases. All the indications which it could furnish were taken from simple experience, and the observation of the sick; but we may doubt whether such experience and observation deserved to be so called in many instances. Single cases were collected and compared, in which some mode of treatment or some remedy had seemed beneficial; and the deduction was made, that what had been useful in those, might be employed with advantage in other similar cases.

The sick demanded then, as they do now, nothing more from the men who had collected this experience, and were acquainted with the remedies, than aid in their diseases and freedom from their sufferings. It is owing to the circumstances that men received, or imagined that they received, such aid, that medicine, from the beginning, has maintained its importance and its credit in the eyes of the public; *i. e.*, as the art, not as the theory, of healing. It was the imperative, not the indicative, which made the impression, as it always does. Medicine was incapable of asserting or proving anything positively, simply because it knew next to nothing of what, or how, it cured. Physicians were also generally at the same time priests,—they were the wisest and ablest men of a nation, as they still are amongst uncivilized people. They were called upon to relieve and restore, as best they might, before they could arrive at any comprehension of what they had to prevent or cure. An individual in danger of dying from hemorrhage was, equally with his professional attendant, unable to wait until the effect of this or that remedy had been established, and still less, until the cause and mode of curing the hemorrhage had been positively and scientifically ascertained. The medical man employed at random the means he had at hand which seemed likely to be of service, *e. g.*, red objects, according to what was called the doctrine of signatures. And because these curative means and manipulations were frequently successful; because, *e. g.*, the hemorrhage ceased after their employment, they acquired the reputation of remedies, which they have maintained, more or less, to the present day. Thus, in all cases, art has necessarily preceded science, and simple experience true comprehension: this assertion applies alike to medicine and to agriculture, navigation, education, or politics. The whole question for both parties—the patient and physician—was, by what means the health of the former could best be preserved, or disease obviated. This object was to be attained as quickly as possible. For this, both the patient in his need, and the physician in his desire to cure, equally strove. It might in many respects be a logical error on the part of the latter, but it was surrounded by the halo of philanthropy and the desire to relieve. The poetical mythology of Greece shows how great were the respect and gratitude rendered to those who practised the healing art, by the noble origin which is assigned to it in their fables. No less a personage than the god of light himself, Helios or Apollo, who developed and maintains all living nature, is said to have been the father of Esculapius, the god-like teacher of the healing art. The same tradition makes Circe to have been the sister of Esculapius; and even to the present day, conjurors, quacks, and mystics, have continued to be the fellow-craftsmen or the rivals of the physician.

Gradually, however, the mind of man ceased to content itself with that which had formerly satisfied it, and which had sufficed to do so in the then existing state of things. If his increasing knowledge, for instance, refused to recognise every thing around him as governed by the gods, or by the rude powers of nature, and by accident, he could not rest content, in matters appertaining to medicine, with simply observing the sick and the phenomena of disease. By observation alone, he could not comprehend the phenomena, nor could he learn anything by such means of their conditions and intimate connection. When, in consequence of the general development of intelligence, the desire was excited to obtain some further insight into the results of simple observation or experience, medicine could not remain exempt from this common impulse and law of the human mind. In this science, as in every other, the mind strove to advance from mere observation and belief, or opinion, to knowledge, and, through this knowledge, to a better practice. Men began to inquire in what manner, on what account, and by what means, their object was to be attained; they aspired to a perception of the laws and mutual relations of things, and of their conditions and effects.

The history of medicine teaches us how, from such a commencement in simple empiricism, it has, in the course of centuries, acquired a comparatively scientific form, after having passed through phases of superstition, mysticism, alchemy, and the like. It may suffice here to remark, that men gradually arrived at the conviction that the physician could not satisfy the requirements of his art, or the most pressing demands made upon it, without a previous more intimate acquaintance with the constitution of man himself, and with the external world with its thousand influences and agents. Physicians became convinced that in medicine, as in other arts, practical success pre-supposed a certain degree of experience; that a man must know and understand many things before he can act with any certainty of success. As the seas, the winds, and the weather gradually engaged more of the attention of the sailor, and man, in his intellectual and moral capacity, occupied the thoughts of the teacher, the moralist, and even the statesman; so the laws of nature, the conditions of the body in health, and the essence of disease, especially in relation to the means of cure, became by degrees the subjects of scientific investigation with the physician. And as soon as the need of such a study was recognized, and physicians had ceased to rest content with observing the more evident effects of disease, and employing at random a few remedies for its cure—as soon as they began to ponder how diseases arose, in what they essentially consisted, how they might best be obviated, and what was the action of the remedies employed—from that moment they required the aid of medical logic. Men's minds were no longer satisfied with simple observation and experience, and the appeal to *them* was no longer sufficient. Having once endeavoured to arrive at a comprehension of the varying phenomena of the human frame, and the processes going on within it, they formed, from the observations they had made, certain inferences concerning their connection, conditions, and laws, deducing from individual instances more general dogmas and propositions. It was the part of logic to prove that the inferences and deductions were correct, and founded upon experience. History may teach us the manifold errors and contradictions in medicine and natural history which arose during their obscure advance towards the rank of sciences, and the various differences of opinion and disputes which have continued to the present day. We must here confine our attention to the present state of medical science, in order to determine what phenomena and processes may be regarded, at the stage of intellectual development at which man has now arrived, as the legitimate objects of medical investigation—as the problems to be solved in medicine.

I. An intimate acquaintance with the human, and particularly the living, body was soon recognized as the great object of investigation; for those changes and processes, to which we give the name of disease, occur in the economy of the body equally with those going on in health, and which are called physiological. To understand the former, it was necessary to put them side by side, and compare them with the latter, for which they are substituted. The first step towards this object being a knowledge of the material structure of the body, in its external (anatomical) form, it was soon found necessary to study its properties, as well with reference to its formal (anatomical), as to its substantive (chemical) elements. This again was the first step—in a higher sphere—towards the perception of what we term function, or activity of the individual organs of the body, *i.e.*, towards the perception of the part played by each in the whole series of vital processes. It then became evident that between this condition of health, and what was called disease, between the various processes going on in each, which had hitherto been regarded as unconnected, no line of demarcation could be drawn, simply because none existed. This is true of all the occurrences, and operations to which matter is subjected, whether in an animate or inanimate form. We cannot define the boundaries between the various meteorological phenomena which constitute good or bad weather—between the different states of any substance by which it appears to us to be cold or warm, bright, electric, or magnetic; neither can we discern the exact limits of the phenomena going on in the living body when it is said, in common language, to be in a state of health or disease.

As, however, in the other natural sciences, all the objects and phenomena proper to them must be investigated, so also must we investigate and strive to understand the occurrences which take place in what we call disease, and their mode of origin in the living body. This task may be divided into several departments and categories, but the real object of investigation is essentially the same in all; for the so-called physiological and pathological phenomena and processes occur, as has been said already, in the same body, and in the same substances. They will, therefore, depend upon the properties and modes of operation of the organs and elements of the body, being subject to the same essential laws, and only differing in consequence of the changed condition of each.

II. Such may be said to be the internal aspect of the objects of our investigation, but we must also strive to attain a perception of the relations of the living organism to the external world, *i. e.*, to all which exists and occurs without and around it. For, notwithstanding its apparent isolation, its dependence upon the external world could not long escape observation. The first glance must have shown that the phenomena and processes peculiar to the body had their origin and continuance in external influences, which were constantly acting upon it, and a progressive insight into all these mutual relations must have brought with it the conviction, that an exact boundary can no more be drawn between a healthy and a diseased state, than between the beneficial and fostering, or pernicious influence of external agents upon the living organism. The facts that the air we breathe, or the food we eat, may tend to support the body, or to injure it, that electricity may contribute to the cure of disease, or be fatal to life—these facts produce no change in the agents themselves, or in the laws of their action. In the one case, as in the other, they are the same agents and influences, with the same properties, preserving a definite, constant conformity to fixed laws in their occurrence, manifestation, and action. If we limit our view to their effects in reference either solely to hygiene, or to ætiology (the theory of the causes of disease), the division would be as inadmissible, in a scientific point of view, as the distinction between physiology and the theory of disease. To facilitate as much as possible the investigation of the two, it will be necessary to combine them in a higher scientific process including both.

III. With this great comprehensive aim of investigation is associated the further one of studying the properties and modes of action of all the agents and materials which we employ for the cure of disease. That these are used and regarded as remedies on account of certain properties and effects can make no change in the manner of their investigation. We have to learn their properties and consequent relations to the living body, as well as their effects upon it, in order that we may employ them as our means of cure appropriately and intelligently.

This may suffice for the present to show, in some degree, the series, or kinds of phenomena, incidents, and objects which it is our recognized aim to investigate and comprehend. When any particular process of investigation is demanded by peculiarities in the subject matter, its dependence upon this circumstance will be shown when we come to their detailed consideration.

SECTION II.

OF THE METHODS AND MEANS IN GENERAL, BY WHICH THE NATURALIST, AND ESPECIALLY THE PHYSICIAN, ENDEAVOURS TO ATTAIN A PERCEPTION OF NATURAL PHENOMENA AND PROCESSES, FROM THE STUDY OF THEIR CONDITIONS AND LAWS.

It follows, from what has already been said, that all the objects which we are especially called upon to investigate are, in reality, very similar in their nature. For they are essentially phenomena and processes in the living body, whether it be healthy or diseased, and whatsoever be the mode of their commencement and termination under the influence of various circumstances. As surely as the vitality of the living body is maintained only by the constant and regular continuance of its own processes, and by the aid of a constant cooperation from without, so surely can those changes in the vital processes, which we call diseases, be by possibility, occasioned only by an alteration of vital conditions, and of internal and external influences. And only by the cessation of those changes or derangements affecting the vital processes—by the restoration of the normal relations, both external and internal, which are requisite for their healthy performance—can they regain their ordinary state of health, whether by spontaneous recovery or by the help of artificial cooperation on our part.

According to the present state of our knowledge, we are compelled to assume that these processes in the living body are the results of a specific mechanism, which is invariable for each, and, in its general principles, similar for all of them, just as is the case with the processes in the atmosphere, in all the changes of weather and seasons in different regions. For diseases are, in fact, only certain species of phenomena and processes in the living body, or their results and effects. If, therefore, those which we call vital occur and manifest themselves conformably to definite laws and in a certain order, the same principle must apply to those to which we give the name of diseases. Their laws, *i. e.* the mechanism and exact mode of their origin and occurrence, must coincide with those of the former in all their essential conditions.

Finally, if it be certain that all the agents and influences of the external world act in their fixed order, and in obedience to definite laws, it is also certain that they cannot have acted otherwise when they have produced a derangement of the vital processes—when they have caused or prevented disease, or even effected its cure. It is equally certain that all the effects of these agents and influences in the living body—whether beneficial or deleterious—consist in certain definite changes in the body itself, *i. e.* in some of its parts, tissues, or processes. These changes, therefore, necessarily commence and proceed conformably to the laws of action of the external agents and influences, on the one hand, and to the vital laws and processes on the other.

Having been convinced of this similarity in the nature of the phenomena and processes which it is our province to investigate, and having clearly recognized the reasons for this conviction, we cannot avoid perceiving that the methods and paths of their investigation must be essentially the same. But they are also the same for each department of natural history, whether the object of study be the heavenly bodies, or our earth, with all that is upon or around it. In each case there are certain things, certain phenomena or occurrences, which we do not understand of ourselves, and of which we can form no idea from merely looking at and observing them; but they are of such a nature that we may hope to arrive at a comprehension of them by systematic investigation. From this it does not follow that each series of phenomena, processes, &c. belonging to different departments of natural history, or medicine, *e. g.* the chemical or physiological, the pathological or pharmacological, does not demand, by reason of individual peculiarities, a special mode of investigation; but the progress of the human mind will always be essentially the same, varying only in comparatively subordinate points, and in the details of the processes adopted.

All the phenomena and objects with which we have here to deal are recognized through the medium of our senses; in other words, they would not exist for us if we were unable thus to appreciate them. The first indispensable step, therefore, if we wish to learn anything concerning them, is to observe them, on all sides and in all the relations discernible by the senses, whether they be such as occur in a healthy or a sick person, or in a dead body, or whether they be phenomena connected with the atmosphere or displayed by an electrical machine. The most careful observation of them, indeed, does not suffice to render them intelligible to us: but we learn that they exist and possess certain properties appreciable by the senses, we know that certain things have occurred, though their mode of occurrence is incomprehensible to us. They are still covered by a thick veil which the mind seeks to pierce, and must pierce before they can be brought within the range of our comprehension. We may, for instance, have observed an animal breathing in a particular way, or crying out and shrinking when a nerve was irritated—we may have observed in a diseased person a cutaneous eruption, an increase of temperature, a rapid pulse, or cough and expectoration—or in the atmosphere the formation of clouds, rain, wind, or storm. All that we have been able to gather in relation to these phenomena has, however, conveyed to our minds no insight into, or comprehension of, their real nature. For they are not intelligible by the mere sight or touch, neither is the constitution of natural objects such as to be readily comprehended. Instead, however, like the lower animals, of stopping at this point of simply recognizing them as they present themselves, we feel the mental impulse and possess the mental power to acquire, by the aid of manipulation, reflection, and investigation, a knowledge of their nature. The interest they excite is proportioned to our acquaintance with them, for we can only deal with them and employ them for our own advantage and that of others, in so far as we have learnt to understand them. We must not only recognize their reality, but also ascertain by what means they are brought about, and why they are thus and not otherwise, in what manner and order they have occurred, what conditions they are subject to, and what other objects they may themselves modify. In short, we seek to know the causal connection, conformity to fixed laws, and definite order of the phenomena and operations in nature, and nothing else will serve the requirements of the case; we must know what operations, what causes and influences, we have to deal with, in order to bring them within the scope of our comprehension.

Mere observation in itself, *i. e.*, all that can be learned by the senses, is insufficient to enable us to do this. All the astronomers in the world, for instance, might have watched the stars night after night as they rose and set, without gathering therefrom anything more concerning the laws of their transit, or the conditions of their visible movements, than uncivilized nations have learned up to the present day. Solar eclipses were observed, and the periodicity of their return recognized long before it was known upon what they depended. In a similar manner, we have seen thousands of men affected with ague, typhus, or inflammation of the lungs; we have observed manifold phenomena in them variously grouped, and following a definite order from first to last, and have found certain changes in the bodies of those who have died of these diseases; but we have never learnt, and never shall learn, from mere observation, what are the conditions of these phenomena and changes, and what essentially constitutes disease. Natural historians might continue for centuries, as heretofore, to note the variations in height of the barometer and thermometer without thereby alone discovering anything concerning the causal connection, and laws of atmospheric pressure and temperature—without learning anything of the still more obscure subject of their possible influence, and mode of action upon human beings in reference to health, or upon the origin and frequency of disease. The physiologist again could not, from the mere observation of respiration in an animal, or of exhalation in a plant, comprehend the exact nature of the process itself. The same holds good in reference to the observation of the phenomena or changes which occur as the effects of our drugs or poisons. We constantly see purging result from rhubarb, and some of the saline substances; vomiting from ipecacuanha; or stupefaction and sleep from opium; innumerable cases of ague are cured, under our observation, by cinchona bark or quinine, and of bronchocele by iodine, but we are at present unable to say in what manner, and by what means these results are brought about.

The most scrupulous observation of any object, or natural phenomenon—*e. g.*, of a healthy function, or of a condition of disease—simply enables us to say that it exists, and has been presented to us in a certain form. We can also examine the subject of our observation with reference to such of its properties as are appreciable by the senses, or with reference to the impressions it makes upon us; and we can compare it with others in order to acquire a knowledge of their similarities, or differences. We may further ascertain in what order phenomena occur, follow each other, and cease, in relation to space and time. All this may, perhaps, satisfy our first curiosity, or even a superficial, aimless thirst for knowledge, but not the comprehensive longings of the mind. It still seeks to learn in what these phenomena essentially consist, and why they have assumed the exact form in which they present themselves to us. In a word, we must discover their causal connection, the mode and mechanism of their origin, their internal arrangement, and their conformity to fixed laws.

Were all this but once known, the longings of the mind would be satisfied and all the requirements of science fulfilled.

For the "knowledge" of a thing means here nothing less than the comprehension of the constitution and formation, the conditions and causes of all that we have observed, *e. g.*, the phenomena and processes occurring in the living body in health or in sickness. We see, or believe that we see, an object before us, but it seems still strange to us, we feel that we are not familiar with it until we have comprehended its constitution and origin, and thereby made it our own, our intellectual property. The impulse in man to appropriate all things, as far as possible, and render them subservient to his reason—to be master abroad as well as at home, urges him on to understand and subject to himself the mysteries of external nature and of his own being and vitality. Thus Virgil sings:

"Felix qui potuit rerum cognoscere causas."

Man endeavours to discover what nature has concealed from him, and this he can do, more or less, if he undertake it earnestly enough, and with the knowledge how he ought to proceed in his attempts. But as the information conveyed by his senses does not enable him to do this, he takes a step further, and properly gives the name of science only to what he has recognized and discovered concerning the objects of his simple observation or experience. Whenever, therefore, a phenomenon, an object of any kind, strikes our senses, the simple observation of it leads naturally to further examination, to its attentive consideration and investigation. We complete and combine almost instinctively, in our minds, what we had observed in an isolated and, perhaps, fragmentary form; we represent it to ourselves in a certain combination, and assume in it, for the moment, a sort of causal connection. We next seek for the relations in which the phenomenon or object in question stands to other similar phenomena or occurrences, in order that we may arrive at a comprehension of their condition or peculiar effects—in a word—of their causal connection.

In this further step, which our consciousness and our understanding demand, we set out with the innate conviction, though not always realized, that every thing which we observe must have its causes and laws of action, whether it be a vital phenomenon, or a condition of disease, a certain state of the weather, or a conflagration, and having once learnt to reason correctly on such subjects, we feel convinced that among the circumstances which have been observed to accompany or to precede it, some one or other, or, perhaps, several in combination, have given rise to the incident in question. Whether this conviction may be inherent in the human mind, or merely the result of previous experience, or of deductions from that experience—in short, how we may have arrived at it—is of little interest here; it is enough that it is felt.

It is of infinitely more importance to us to know what we are to understand by "cause and effect," in all the natural phenomena and processes; what, for instance, really constitutes cause and effect in what are called the functions of the body, or in diseases and in their origin and cure. A clear understanding of this relation is evidently essential to a successful inquiry into the causal connection, conditions, and laws of any natural phenomenon—*e. g.*, of vital processes—and thus to the attainment of the objects of science. These phenomena and processes become more intelligible, and their causal connection unquestionable, when such a relation can be established between them and some other occurrence or circumstance, as would enable us logically to deduce them therefrom, or, in other words, to accept the latter as the adequate cause of the former. Thus, the overflow of a river, and consequent inundation of its banks become intelligible to us if we hear that heavy rains have fallen in the neighbourhood. The fracture of a bone in A, or the sudden falling down, loss of consciousness, and paralysis of B, would be intelligible if we heard that the former had met with some accident, or that the latter had been the subject of a considerable extravasation of blood into the brain, which we call apoplexy. We should also understand why every animal, and even plant, does and must respire; why the former gives off carbonic acid, and the latter oxygen, so soon as we knew with what other changes of material, or other processes in their structure the giving off of certain gases stands connected in such a manner that we can safely regard it as occasioned and modified by them. In the same way, the purgative effects of a substance, such as sulphate of magnesia, would be explained if we knew all the essential modifying changes and processes in the alimentary canal to which its presence gives rise, and which are requisite to cause the purging itself.

We have now to determine what paths must be taken in order to discover this causal connection, these conditions, of the phenomena and processes which form the subject of inquiry —*e. g.*, of a vital function or of a disease—and thus bring them nearer to our comprehension.

The first indispensable step towards this end must be, in this, as in every other department of nature, the observation of the phenomena, and the determination of their precise condition. In other words, we must start from our experience concerning them, and carry out the inductive process of reasoning. Every thing which occurs in nature is so constituted that we cannot attain a full comprehension of it by direct observation alone; and inasmuch as we should in vain question nature directly concerning it, our object can only be attained by choosing other routes, and sometimes even various by-paths. For this object is not merely to ascertain that certain phenomena and processes exist and become known to us by properties appreciable by the senses, but also to discover how they are modified by others; and to this end we must study the modifying circumstances and processes themselves. We have thus, first of all, to ascertain what other phenomena or processes—whether in the external world, or in the interior of the living body itself—constantly precede or accompany that which is the subject of observation. There is, at the outset, only one mode of attaining this object, viz., to observe with care each one individually, as they occur simultaneously, or follow in succession in the case before us. We must, therefore, endeavour to determine each phenomenon presented by a function such as respiration or by a person labouring under some disease, &c.; also taking into account the changes in the external world, or in the materials supplied to the body for the purposes of respiration or digestion, as well as those occurring in the body itself, from the administration of medicines. In short, our first office is to determine the exact state of things, and to observe, as correctly and comprehensively as possible, every point in the case before us, since all experience and knowledge of natural objects begins with individual cases. But, to arrive at a proximate insight into the connection and conditions of any given process or phenomenon, it is necessary to compare together numerous examples in which it has been observed to be preceded by or to have accompanied certain others. We have next to make out what has uniformly occurred in all these cases, conjointly or consecutively, and what has not occurred, in order to distinguish constant coincidences from those which are merely accidental.

Under circumstances favorable to this determination we are justified in concluding from the constancy of the coincidence, that, in all cases, the phenomenon or process in question would find its adequate cause in certain other phenomena, processes, or incidents, *e. g.*, the excretion of carbonic acid in respiration, the existence of paralysis, or the action of a purgative or corrosive substance; and this is no more than has been established by experience with reference to various facts in medicine. But before this can be assumed, it must be proved that the occurrence has, in all the cases which we have observed, been brought about by that process or circumstance which we have assumed to be the essential one, and by no other. Having determined this point, we may venture to bring the subject of our investigation into causal connection with, and fixed and constant dependence on, other phenomena and processes. For if we have observed a given phenomenon or process to accompany or follow certain others, and if we have shown that it depends upon them, we may certainly regard them as standing to each other in the relation of cause and effect. And by fulfilling all the requirements in the determination of this fixed causal connection, we not only demonstrate it for the cases immediately observed by us, but we establish a law which equally holds good for all cases or instances of the same kind or class; for we have discovered the invariable order of succession and the conformity to fixed laws of all the phenomena, processes, &c., in question, in the mutual dependent relation in which they stand to each other. This was done, for instance, when it was proved that, in the fall of bodies, their weight, *i. e.* the attraction of the earth, was the common determining circumstance. It may be said also to have been done with reference to certain kinds or series of paralysis which depend upon extravasation of blood into the brain; and also with reference to the corrosive action of certain substances, which is due to their energetic affinity for, and combination with, certain elements of the organic tissues, such as albumen, water, &c., when brought into contact with them.

We have thus attained what we call a theory, and indeed a correct one, of these phenomena, or occurrences, *i. e.*, one of such a nature that it explains to us all the processes and phenomena of any definite kind or series, and gives us a correct elucidation of their conditions and conformity to fixed laws, as well as of the immutable order of their simultaneous or consecutive occurrence.

Enough has been said to show the end we must steadily keep in view in the study of natural phenomena, and the method by which we may hope to attain to a comprehension of such as were previously obscure. Our task, therefore, is, to prove from experience in individual cases, and from actual observations and experiments, that they do stand to each other in this fixed relation and causal connection. We shall then be able to advance certain propositions in reference to them, *i. e.*, in reference to the uniformity of certain kinds or series of natural phenomena and processes.

The whole method of reasoning by which we seek to make natural phenomena accessible to our comprehension, to arrange them methodically, and make our knowledge of them available, is called the "Inductive Process." It is, as Bacon expresses himself, the "ars interrogandi et interpretandi naturam," the "via a sensû ad intellectum."[1] For "omnis interpretatio naturæ incipit a sensû, atque a sensuum perceptionibus, et rectâ, constanti, et munitâ viâ ducit ad perceptiones intellectûs, quæ sunt notiones veræ, et axiomata."[2]

It is essentially the same process in every science which has for its object the attainment of knowledge by the means of special experience, and the examination of individual instances, *i. e., a posteriori.* The unvarying character of the human mind leads it to adopt, in its struggle for the comprehension of natural phenomena, essentially the same expedients through its whole progress, although they may differ in minor respects with the objects to be investigated, and though they may be applied with very dissimilar results in the various departments of nature. The astronomer, as well as the physicist (Physiker) and geologist—the chemist as well as the physiologist and physician, must all seek to discover, from experience and exact observation, in what the phenomena and processes, with which they have each to deal, essentially consist, and what may be their causes and the effects of certain agents and influences upon them. And only from the results of individual observations of the actual state of things, or from artificial experiments—in short, from the empirically ascertained behaviour of the phenomena and processes in question, in a certain number of cases—can any legitimate conclusion be drawn as to the behaviour and conformity to fixed laws of other phenomena and processes of the same kind.

Our whole course of demonstration consists, therefore—

I. In the observation of the objects to be investigated in a certain number of cases.

II. In the comparison of these individual cases or instances with each other, and in extending to all cases of the same kind what we have discovered and established by the aid of such comparison.

[1] Lib. i, aphor. 76.

[2] Lib. ii, aphor. 38.

III. In the deduction therefrom of the "causal connection," the so-called "laws," which affect this particular series of phenomena and processes.

From the fact, that in all the cases which have come under our observation, any given phenomenon has constantly occurred under certain conditions established by experience, and in consequence of certain processes or influences, we draw the conclusion, that it will invariably recur under the influence and co-operation of the same circumstances. The task to be accomplished—the difficulty to be overcome—is, to ascertain empirically all the conditions and modifying processes, and to distinguish those which are really essential (constant and determining) from those which are not. If we have accomplished this, we have established what is called a "natural law" in its most extended signification, and have solved, by induction, the problem which we had to investigate with reference to any particular class of natural phenomena; for we have demonstrated the causal connection, the conformity to fixed laws, and definite order in the relations which certain phenomena and processes bear to each other. And the fact that under the same given circumstances the same thing will always occur, is called a "law." It would, perhaps, be better—because simpler and less ambiguous—to call it a "general natural truth."

The study of physics, for instance, has demonstrated that the variations of the atmospheric pressure, as indicated by the barometer, as well as the atmospheric currents and winds, have their common cause in the changes and inequality of the temperature of the atmosphere itself; that the motions of all the heavenly bodies are regulated by their mutual attraction. A similar law in chemistry is, that all combustion depends upon the energetic combination of oxygen, or matters analogous to it, with certain component parts or materials of combustible bodies. In no department of living nature has any causal connection or natural law been established, equally definite, or which can be taken in the same extended sense. We have only met with approximately similar relations among certain of the phenomena, such, for example, as the dependence of respiration upon a process or chain of events remotely resembling those of combustion.

We regard any series of natural phenomena and processes as explained when we have obtained such an elucidation of their causal connection and conformity to fixed laws as has just been alluded to. Explanation in this sense means nothing else than the demonstration by experiment of all the co-operating conditions and agents which have produced them —the demonstration why any single occurrence happens in a definite manner, in fixed order. When we have cleared up these hitherto obscure points, we are said, in common language, to comprehend such natural phenomena and processes; just as we are able to comprehend and explain to ourselves the conduct and actions of an individual, as soon as we know his character and motives, and all the modifying and opposing impulses.

But it is of the highest importance to define the exact sense in which the words "explanation" or "comprehension" ought to be used in every department of natural study, and to restrict our acceptation of them to this sense only; viz., that by comparison and investigation we have so far explained certain previously unintelligible phenomena, as to be able to deduce them from one or more processes and phenomena which have preceded, or accompanied them—in short, that we have shown them to be the results of a certain other process or action. We have, for instance, referred the fall of a body, or the motions of the heavenly bodies, to the effect of gravitation; we have shown that respiration depends upon certain relations between some of the component parts of the blood and the oxygen of the air; and that certain cases of paralysis are the result of an extravasation of blood, of pressure upon the brain or spinal marrow. The incident or phenomenon is, however, by no means literally and completely explained thereby. For we do not yet understand it thoroughly, any more than we are able, in the case already alluded to, to comprehend or explain the conduct of a man, *i. e.*, to acquire an insight into the ultimate reasons of his actions. We know, perhaps, that he was ambitious, ardent, or amorous, and we thus explain to ourselves his conduct, nay we can frequently predict it. But we do not know why he was so, or why these precise qualities were in a state of activity in his disposition. His conduct and actions are therefore as enigmatical to us as before. To the layman a disease, or a case of death, seems intelligible when he hears that A had caught cold or had overloaded his stomach, or that B had swallowed poison. He further understands the cure of A's disease when he hears that he has had medical advice, or taken some drug, or perhaps adopted a superstitious mode of treatment. He is not accustomed to proceed further, in such cases, in his inquiry into the causes of things, and is, therefore, not inclined to regard such occurrences as obscure or incomprehensible. To the physician, however, they would only be intelligible, if he knew that in A, a catarrh, an inflammation of the lungs, a fit of indigestion, or a diarrhœa had been produced; or in B, narcosis, or inflammation of the stomach, &c. But if he have employed certain remedies in such individuals, and seen them recover, he is apt to regard their cure as sufficiently explained by the supposed effects of the remedies employed; for he concludes that the modifying changes and conditions which produced the disease have been removed by the precise effect of the remedies, and he regards the subsequent recovery or improvement as a sufficient proof of their effects and value. The reflecting physician—the investigator of nature—would, however, be far from satisfied with this explanation. He is not content until he has made himself acquainted with every individual change in the functions and tissues of the body, as well as with the manner of its occurrence, whether produced by cold, by overloading the stomach, or by poison. And first he must learn the individual, chemical, or physical changes and elementary processes which constitute conjointly such inflammation, indigestion, or narcosis. In like manner he must determine the individual influences acting upon the body from without, *e. g.*, of the atmosphere in the case of catarrh; and the particular modifying properties of the substance acting from within, *e. g.*, the poison, or the remedy. We have thus, as it were, several strata of problems and questions lying behind and above each other. And in the attempt to explain them a greater depth is reached, just in proportion to the demands of science, the technical knowledge and skill of the inquirer, and the more or less scientific manner in which the attempt is made.

We are therefore led to the conclusion that, throughout nature, "explanation," in its ultimate signification, means nothing but a reference of certain phenomena and processes to others, or the resolving the one into the other. The latter may therefore be designated as lying deeper and being more general in relation to the phenomena modified by, or referred to, them. We might therefore correctly call this whole process of investigation and explanation (*i. e.* the course pursued in attempting the elucidation of the causal connection of these phenomena, &c., according to the inductive process), the generalization of a fact or incident. Thus, when we referred many cases or kinds of paralysis to pressure upon the brain (either from extravasation of blood, or from the presence of tumours within the cranium), and deduced them from this cause; and when we referred many acute diseases, which were regarded as so many forms of what was called "idiopathic fever," to inflammation in different tissues, we extended the application of both these conditions to a series of other phenomena and processes,—*i. e.* we generalized them. Another instance is afforded by what we now call "crases;" *i. e.* certain changes in the constitution of the blood, and in the conversion of tissue, &c.; for we now regard the most varied changes and derangements, both of organs and of functions, in the living body, as modified by those crases. We have not to inquire here how far all this is correct, *i. e.* is the result of demonstration. It is sufficient for our purpose that of late certain phenomena and processes have been connected together, which were previously regarded as differing in character and standing widely apart the one from the other; and that we have discovered, or at least believe we have discovered, in all of them, the same condition and process to be determining and essential. In like manner the physiologist has partly traced the phenomena connected with the functions of "absorption" and "secretion" to the physical processes of endosmose and exosmose. He has also traced the development of the most dissimilar tissues to the previous formation and further growth of cells. In the former case, he generalized the processes of endosmose and exosmose; in the latter, that of cell-formation. The same principle applies to the discovery of the chemist, that the bleaching effect of chlorine—*i. e.* the destruction of colouring matters, as well as its power of destroying certain fetid gases, &c.—depends upon its energetic affinity for, or combination with, the hydrogen and other basic substances contained in those colouring matters, or fetid gases.

But since, as already stated, we cannot regard any phenomenon or process as thoroughly explained because it has been referred to others, or deduced from them, so we cannot be content to stop here. The fact of its occurrence, and the conditions under which it happens, as well as its explanation, have been referred to other phenomena or processes; *i. e.* to those by which it is modified or to which it has been reduced, —in paralysis, to states of brain caused by pressure and the like; in the processes of absorption and secretion, to the principles of endosmose and exosmose; and in the destruction of colours and smells, to the affinity of chlorine for hydrogen and other similar elements.

The onus of the proof, to use a forensic expression, has been transferred, and thrown upon those latter incidents and processes which have been recognized as modifying the former. Those have now to be explained which rendered the former sufficiently intelligible to us; for their own conditions and causal connection are still obscure.

We are thus transferred from the consideration of one mystery to that of another far less penetrable, while a new mark of interrogation takes the place of the former one. Our impulse to investigate, and the requirements of science, with its inexorable demand for the *cause* of every phenomenon, or occurrence, have many questions to ask in this newly-acquired territory. We have, as it were, pushed our scientific outposts into this region of comprehension and explanation, and fortified them there. It now remains for us, in like manner, and by the aid of similar processes, to conquer the unknown country lying before us, and to make its subjugation subservient to the advancement of science. We must seek for a new series of modifying processes and circumstances which may explain those just discovered, and hitherto enigmatical, in the same manner as *they* had explained the former series. Our task, therefore, is, to search for those other intermediate or deeper-seated elementary agents or circumstances by whose action or impulse the rest are modified. To succeed in this will be to discover those more general circumstances and processes which present themselves with uniformity and in a certain sequence in a much greater number of phenomena, and, through such discoveries, to advance with certainty upon the highway of generalization.

This has already been attained in the case of sciences such as astronomy, physics, and chemistry, which can boast of having arrived at a certain degree of exactitude. Human science, working from age to age, has not only explained the falling of bodies to the earth by their weight, *i. e.* traced it to the law of gravitation, but has also referred the revolutions of the moon, of the planets of our solar system, and of all the other heavenly bodies, to the influence of the same cause. It has proved that the common determining circumstance, as applicable to all these phenomena and motions, is, that all bodies mutually attract, or strive to approach each other in the ratio of their weight and distance; *i. e.* they are influenced by the centripetal force, while this tendency is opposed by another, viz. the centrifugal force of the heavenly bodies. In other words, it has been shown that the conditions and laws of all those so-called forces are essentially the same, *e. g.* the tendency of a given body, say of the moon, to fall upon the earth, or of all the planets to fall upon the sun. Only a few years ago we were accustomed to regard the electric, the galvanic, and still more the magnetic phenomena in certain bodies, as being perfectly distinct in kind, or as the manifestations of essentially different and peculiar forces which, under the name of imponderables, were distinguished as electricity, galvanism, and magnetism. Step by step, however, it has been made apparent that these mutually call forth and modify each other (see Faraday, Oersted, &c.) We can now see the oneness of their essential and governing laws, and trace the diversity of their modes of exhibition, as phenomena, to its true source—a difference in the characters of the bodies in which they occur. And analogy has already hinted that the phenomena of heat and light, and the laws which govern their peculiar manifestations, may possibly have the same origin with the so-called electric and magnetic forces. Chemistry has received an important aid towards generalization, or the reduction of its numerous individual phenomena to one and the same circumstance, or law, in Dalton's well-known "law of equivalents;" and we shall elsewhere have occasion to allude to several nearly similar generalizations with which modern times have enriched it, particularly to that of Liebig. Thus chemists have now, with great probability, referred respiration and the whole conversion of tissues in the living body to the higher general law of the continuous oxidation, or slow combustion of its oxidizable or combustible materials, more especially of its carbon. As a remote yet similar instance of this occurring in the theory of disease, we may observe that the process of inflammation, which was previously known as a common condition, or at least as a common phenomenon in many diseases, has resolved itself into another, which has been called the process of exudation, and obeys the same laws; while that again has been approximately demonstrated to be dependent upon certain alterations in the pressure of the column of blood, or in the calibre of the blood-vessels and capillaries, or upon certain changes in their contents, *i. e.* in the blood (*e. g.* in the amount of blood-corpuscles, or of albumen, fibrin, or water, in the plasma of the blood), which may be regarded as essential and determining circumstances in relation to the process of exudation. In a similar manner the common determining cause of certain cases of paralysis has been shown to be either an extravasation of blood in the brain, which has been reduced to other more elementary and general changes or processes; viz. to ossification, or fragility, of its arteries; or its cause has been found to be a softening or analogous condition of the substance of the brain itself; or finally, an interruption of the communication through the nerve fibres by certain more remote changes in their substance. These modifying states again, may, perhaps, be referred to deeper and more general processes, *e. g.* to certain derangements or anomalies in the whole conversion of tissues, or in the nutrition of the brain, &c.

This generalization must be carried out on a similar plan throughout the whole science of natural history. It is our task to show that certain series or kinds of natural phenomena and processes, certain occurrences which have hitherto been regarded as heterogeneous and modified by entirely dissimilar circumstances, have, fundamentally, the same connection, the same mode and regularity of origin, commencing and proceeding in conformity to the same essential laws. We must point out the essential oneness which underlies the accidental diversity, and show how, with some comparatively subordinate variation, the same thing will recur, under similar circumstances, with unchanging uniformity; and by establishing the accidental, secondary, and subordinate nature of the modifications and variations in question, we must arrive at the knowledge of the higher and more comprehensive law. We shall thus make our way into several consecutive or alternating phases of conformity and causal connection of natural phenomena and processes, and our comprehension of them will, at the same time, advance further and deeper. While often—as in the strata of our earth—whole centuries lie buried in a single stratum, centuries more pass away before investigators can succeed in carrying a shaft into the underlying stratum. More than a century intervened between a Keppler and a Newton. We can form no surmise as to how far, in the various branches of natural history, this process of reduction may yet be carried; least of all in that field of vital phenomena which has to do with the treatment of disease. We know, however, that it has its limits, and that these limits must coincide with the boundaries which the imperfection of our senses imposes upon our observation of general nature, and upon our investigations into it. And this will be the more evident as we remember the paths by which alone, in the case of any given natural phenomenon, we can arrive at the understanding of its causal connection and conditions, and thus gain a competent knowledge of the laws which govern it.

This understanding has never been attained to *from without*; we have not derived it, so to speak, *immediately* from nature, but rather from our own deductions from the facts which she has been pleased to present to our consideration. Our knowledge of the conditions and laws which regulate the origin and progress of certain phenomena has been, as it were, created for ourselves. Our minds, in attempting to determine and arrange these laws, have translated the enigmas which the phenomena present into a language more intelligible to us, in converting them into our technical formulæ and familiar modes of expression. And thus, as Hegel expresses himself, "external phenomena have been transmitted to the internal perception."

When we have, by the aid of experiment, obtained some knowledge of any given series of natural processes; when we have ascertained in them a fixed mode of origin, and a simultaneous or consecutive occurrence, we are justified in concluding that there exists between them either a definite connection, or a certain internal relation, and are warranted in calling the one process or phenomenon, the condition or cause of the other. But by this we can only mean that, according to our conception of them, the things in question bear this definite relation to, and causal connection with, each other, and that we must regard the phenomena and processes which we have ascertained by experience to be constant in their occurrence and effects, as the real condition, or adequate cause of certain others. As it is, therefore, by means of our own mental operations that we have succeeded, in the case of these phenomena and processes, in ascertaining their conformity to certain laws, and in bringing their various conditions within the scope of our intelligence, we are justified in regarding all such occurrences, whether standing apart or connected together, just as our experience has presented them to us. As such they have ever appeared, and we may therefore conclude that they can never appear to us in any other way. We thus end by recognizing ourselves, *i. e.*, our own intelligence and intuition, as the standard whereby we must judge of external nature. Do these phenomena, we ask, arise and recur consistently with our previously conceived notions of them and of their natural connection, and we are satisfied if we receive an answer in the affirmative; but this must not flatter us into the idea that we have penetrated into the true secret of their connection, or have learnt what is the real cause or nature of certain phenomena and processes, or why these should call forth and modify certain others. Through investigation, discovery, and the experience to which they lead, we can but arrive at understanding the definite order of the simultaneous or consecutive occurrence of such phenomena and processes, and must remain as far as ever from comprehending their origin and conditions. Man, reasonable though he be, is but a minute, subordinate atom in the universe, and does not know whether all that exists and occurs in it, does so conformably to his reason, any more than he knows whether our atmosphere, with its waters, nutritious matters, &c., is constituted exactly in the manner most beneficial to him. By the aid of his own reason, and by that alone, does he know and understand external nature, *i. e.*, all that exists and happens around him, or can he render it comprehensible and available to himself. It is vain, therefore, to believe or assert that we have discovered the reasons, or traced the ultimate causes of what occurs around us, because our limited capacity does not permit us to discern, far less to penetrate into, all the various events in nature.

Not even in the most certain and positive of all natural sciences, physics and mechanics, are we able to inform ourselves why a given substance has certain properties, or why certain bodies act upon each other according to laws which may be established with mathematical precision. They do not tell us, for instance, why the impulse of a body in motion produces motion in another body struck by it, or why a stone which slips out of the hand, falls to the ground. For between the stone and the ground, or between the body in motion and the second body put in motion by it—*e. g.*, a struck billiard ball—we can detect no connection necessarily involving such a phenomenon or effect. The natural philosopher has long ago shown that different fluids evaporate, boil, or freeze, at very different temperatures; the fact is established, but it still remains without a reason. Even the highest and most positive datum which the mind of man has acquired in the comprehension of nature, Newton's law of gravitation, is, in reality, only a fact, and not an explanation of the fact; *i. e.*, it cannot give us the essential and ultimate cause of the motions of the heavenly bodies. Like all other so-called natural laws which we have discovered, it remains in the end simply a subjective conception of the human mind, a notion, explanation, or scientific hypothesis, which we only regard as correct, or fully adequate, and therefore receive as a positive law, because it is sufficient to explain all hitherto known facts concerning the motions of the heavenly bodies. It is possible, after all, that the heavenly bodies may not attract each other, for we have never seen or directly proved that they do so. It may not be from this, but from some other and entirely different cause that they perform their stated revolutions with their known regularity and order. In a similar manner, we know of many bodies, that under certain circumstances they present certain phenomena and produce certain effects or phenomena in other bodies, which we speak of as heat, light, and colour, or which we call electric, galvanic, &c. But we do not know in what these phenomena and processes essentially consist, or why a certain body (in itself) is able to produce them in other bodies.

And if it is thus in even the most exact and most advanced branches of natural philosophy, how can we look for anything better in the science of vital phenomena, which is acknowledged to be the most difficult and complicated of all, and which must ever remain so. In reality, we do not even know why we feel, see, or taste a substance; why the volatile particles of a rose smell pleasantly, and those of assafœtida the reverse; or why common salt has a different taste from milk or butter. Much less shall we ever learn why certain bodies possess vitality; *i. e.*, present the phenomena and processes which we include in the expression "life;" why their organs are developed, and converted, in a fixed order and manner, from the simplest cell. Equally little can we ever hope to know of the causes why, under certain circumstances, they present those other phenomena and processes which we call disease; or of the changes which have occurred within those bodies, to produce in them phenomena and processes such as pain, heat, fever, inflammation, &c., which have no existence in their ordinary, or healthy condition. We shall never learn why living bodies either live, or become diseased in this or that respect, or why what are called diseases present themselves to us under such aud such conditions; neither shall we ever understand the so-called essence of these phenomena, simply because it is beyond the sphere of man's comprehension.

It is irrational to seek after what has once been recognized as impossible, and few things have so much impeded our advance in the comprehension of vital phenomena, in their relation to the treatment and cure of disease, as the circumstance that we have either striven to accomplish the impossible, or, seeking after the possible through impracticable processes, have often failed to find it when it was near at hand. In this, as in every department of natural history, the true aim is to establish facts; to determine, in any given case, under what circumstances the phenomena and processes present themselves simultaneously or consecutively, and to reduce them to fixed order and conformity, but *not* to trace out the ultimate and specific causes of such occurrences. We have done all that is humanly possible to explain the causal connection, or laws of a given phenomenon or process in nature, when we have demonstrated from experience, under what circumstances, and under what coincidence of other phenomena and processes it constantly occurs, and under what others it is never observed, especially if we can ensure that it will *always* recur under the circumstances which we have so determined. And having done this for one series of phenomena, we are prepared to demonstrate in any other series, which manifests the same kind of simultaneous or consecutive occurrence, the existence of the same laws; and we may now, as has been already shown, extend the principle of causal connection to entirely different phenomena and processes, and thus advance, step by step, in the generalization and reduction of a number of things to one and the same kind of conditions (Bedingtseyn).

We may speak of the cause in consequence of which certain bodies or substances present certain phenomena or processes, or give rise to their manifestation in others, as a "property" of these bodies and substances; and may therefore imagine that they produce such phenomena and processes by virtue of these properties; *i. e.*, that their capability of producing certain effects, springs from these their properties and nature.

And we may further, if we are so pleased, give to these properties, to this capability, the name "force;" and, as such, briefly distinguish the whole sum or aggregate of the circumstances by which any given phenomenon or process, taken singly, or in series, is wont to be modified. We thus ascend, as Laplace says, from the observation of natural phenomena to their laws, and mount from the consideration of laws to that of forces. Our minds (Verstand) are compelled to acknowledge, in nature, as many distinct properties or forces as we recognize series of phenomena, or effects distinct in their conditions and causal connection. Thus we are warranted in assuming a force of gravitation and attraction, or of chemical affinity, an electric or magnetic force, as well as a vital, or muscular and nervous force.

But we must not deal arbitrarily with these forces, &c.;

neither must we think that by the assumption of a force we have made any advance towards the comprehension of a phenomenon, or occurrence in nature, or that we have thereby made it more intelligible to ourselves or others. For we are unable to learn anything concerning these forces and properties themselves, as such, simply because we cannot immediately observe or investigate the mode of their action. We have been induced to assume the existence of a "force" from the necessity of accounting for certain phenomena and effects, and for the regularity with which these follow each other; for we have imagined that these consequences must depend upon some such force. But of the force itself, as such, we know and can therefore assert nothing; neither can we hope, by the aid of this abstract, hypothetical term, to understand or explain more clearly than we could do without it, what is really going on in nature. As we can discern nothing of the mental power of the most intellectual brain unless we see certain manifestations of it, so we can attain to the understanding of a force, or property, and of the effects ascribed to it, by no other path than the ordinary one,—*i. e.* the same which we must have adopted without the assumption of this force, or property, and which, in fact, we *had* adopted long before the study of natural history was sufficiently advanced to bring us to the abstract notion or assumption of any force or property whatever.

If we further investigate those natural phenomena, the peculiar mode of whose appearance and commencement, occurring and following each other in a fixed order and sequence, first excited in us the notion of their being dependent upon some particular "force," we shall find that this fixed order and conformity is made, by a kind of mental transfer, to serve as the law according to which the force acts and manifests itself. Instead of saying, for instance, that the velocity with which a body falls to the earth is in the direct ratio of its weight, and in the inverse ratio of the squares of the distances, we now say, that the cause or force by which the body falls (the force of gravity, or the earth's attraction,) acts in the direct ratio of its weight, and in the inverse ratio of the squares of the distances. Or, in place of saying that the peculiar phenomena and processes which we include in the term "life" are the result of the special co-operation of certain organs, &c., in short, of the combination of certain solids and fluids, and of their acting with and upon each other, we give to their common cause the name of "vital force." The laws which we are able to discover for those processes now serve as its laws; and all that acts upon the living body and its organs and functions we now imagine, primarily, to affect this vital force; and we are thus accustomed to speak of its different phases and bearings, or modes of manifestation, of its increase or decrease, and of influences acting upon it and the excitement produced by them. Yet through this our deduction of certain phenomena from certain forces, we have attained no clearer understanding of their causal connection and laws than we were in possession of without its aid. What we have gained through it is but the hypothetical foreboding, or supposition, of an "unknown quantity,"—a determining circumstance, or influence (to which we now give the name "force"), acting beneath and behind those occurrences in nature which we have been studying. To many minds, this hypothesis will no doubt afford a certain relief; it satisfies, in a manner, their longings after a fuller insight into the causal connection and ultimate essential conditions of the various natural phenomena that come under our notice. But a complete misapprehension of what is necessary and possible is involved in the idea that, by the mere assumption of such a force, we can better understand and explain any natural occurrence whatever. For we do not arrive at that which we know and are able to affirm with regard to such forces through *a priori* assumption, but through patient *a posteriori* investigation and deduction,—in short, through our experience. And instead of all these occurrences becoming more intelligible, in respect to their causal connection and laws, by the assumption of such forces, these forces themselves can, on the contrary, only become in some degree more comprehensible in their nature and action in proportion as we become more familiar with the phenomena and effects deduced from them, and acquaint ourselves with their origin and with the definite mode and conformity of their occurrence.

It is, therefore, little less than irrational that many, through the adoption of an opposite path, have begun where they could only hope, under the most favorable circumstances, to have ended. Many, it appears, have entertained the hope of being able to explain entirely unknown and unexplored series of phenomena and processes by the operation of forces, or by the laws by which such forces act, without being sufficiently careful to deduce the existence of the latter from the observation of facts as they actually exist. For the very existence of these forces must be grounded upon experiment and deduction, and these have need to be of no trivial, arbitrary, or *a priori* nature. If even the physicist, by the assumption of a force of gravity, of an electric fluid, or of the materiality of heat and light, has attained to no positive understanding of these forces, as they exhibit themselves in their peculiar phenomena and processes, how can we hope to succeed better in the field of vital phenomena? Yet many have believed that the mere attributing various occurrences to the workings of a vital force would enable them the better to understand their origin, and the more plainly to point out their ultimate and special nature and conditions. They have believed that they had comprehended the nature and essence of the processes in the healthy or sick, when they had been able to describe them as the increase or decrease of an irritability, as an excess or want of stimulus, and had included all the obscure processes which induce a return to a healthy condition in the term "natural healing force." Or they have hoped, perhaps, to explain the effects and efficacy of certain remedies, by assuming, as their ultimate and adequate cause, special medicinal virtues and properties; or by assigning to them a certain inherent and mysterious relation to certain processes and organs in our bodies.

With all this, however, no advance has been made either towards the comprehension of ultimate causes, or towards an insight into the effects of the remedial substances in question.

Whoever professes some insight into the things submitted to our investigation, and into what is demanded of us in the way of their examination, must have clearly understood that it is impossible by such a mode of advance, or by the employment of any such abstract notions, and forces by way of explanation, to arrive at a better comprehension of the objects submitted to us. He must be aware of the insufficiency, nay even absurdity, of attempting thus to explain the internal nature and essential connection of any series of phenomena, or processes in the living body, before their individual conditions and laws have been investigated and determined. To do so would be a dangerous and illogical abuse of our most important and useful faculty, viz., the intellectual; and it is this assumption of forces, &c., this *a priori* jumping at conclusions, in short, this arbitrary taxation (Octroyiren) of the human mind in the investigation of nature, which has produced false theories, and, perhaps, more than anything else, prevented the establishment of better and more correct ones.

Such are the views which, since a more sound and positive method of investigation was first introduced by Bacon, have been confirmed by the ablest investigators in every department of natural science—whether in the case of a Galileo, a Keppler, or a Newton, a Cuvier or a Humboldt, a Berzelius or a Liebig. Formerly other views obtained, and they are, even now, not altogether exploded, especially from the domain of physiology and medicine. Were it otherwise, how could we, even in the present century, see new theories and systems not only hazarded *a priori*, but accepted by thousands as established and correct? But to force this conclusion upon us, we need nothing more than the fact that such *a priori* attempts, and the propensity to hypothetical theories, drawn from the subjective views of the individual, are inherent in our nature, and that this mode of proceeding, corresponding to a natural requirement of the human mind, may find therein, in a certain sense, its justification.

And this further leads us to consider the part which our *ego*, *i. e.* our whole subjective contemplation and interference, plays in the observation of natural phenomena, as shown in the notions we form and the deductions we draw from what we observe. We have already alluded to the influence which, even in the most sober, circumspect, and *a posteriori* induction from experience, our mental *ego* will obtain by way of irrepressible co-operation. Let it here then suffice to remark, that we cannot proceed to the investigation of any conformity to fixed laws, any causal connection of natural phenomena, without having first formed for ourselves some notion of them; without having represented to ourselves this or that mode of their relations as being the more probable, and having thus formed certain suppositions, certain conjectures or hypotheses concerning them. Much less, without such, could we hope to bring any previously unconnected and unexplained series, or kinds of natural phenomena and processes, into a due and more intelligible order and conformity, by means of which to arrange and connect them consistently with our notions. He only can hope, in the investigation of nature, to take rank as a discoverer, who can form some surmise as to what he has to seek for and discover, and be thus in a position to put his questions so advantageously as to obtain an answer where another might ask for one in vain. Thus we see that the most skilled and successful investigators have arrived at their best and most important discoveries by both routes, i. e. *a priori* as well as *a posteriori*—*e. g.* a Copernicus, a Keppler, or a Liebig—and we need nothing more to establish the high value of scientific imaginativeness, even in the investigation of the simplest natural phenomenon. The impulse, the conscious foreboding of certain relations, or analogies in nature, which first led a Copernicus to his discovery, induced a Harvey to seek for a circulation of the blood, and finally conducted him to its establishment as an ascertained fact. He himself says: "Cæpi egomet mecum cogitare, an motionem quandam quasi in circulo haberet, quam postea veram esse reperi." These men carried out by strokes of genius—apparently *a priori* and by *synthetical* conception—the most comprehensive generalizations, and discovered the most important natural laws, without, to all appearance, having to pass through the tedious, gradual, and preparatory labour of systematic induction from experience, *i. e., a posteriori*. In their hands both these modes of investigation and discovery became one by merging in a higher process peculiar to minds of their exalted calibre.

The interference of our individuality in the observation and investigation of actual occurrences forms a connecting link between the sober and strict process of induction from positively established facts—in short, from experience and *a posteriori*—and our whole *a priori* progress, or mere hypothesis and speculation. In each case we advance in our investigation and comprehension of nature conformably to certain innate and fixed laws of our intellectual being, which are always fundamentally the same. The difference is only that the men best qualified to take the highest rank as investigators of nature, are they who are neither inclined on the one hand to overestimate, nor, on the other, to undervalue this same co-operation of our *ego*; they have been fortunate and wise enough to attach to their *a priori* conceptions and surmises the precise value which these have actually possessed, using them only as the first preliminary step in the whole process, which aids them in testing and feeling their way towards further investigation. And thus, while they have made the faculty in question assist them in the discovery of an order previously undemonstrated by experience, they have been on their guard in not permitting such views and *a priori* conjectures (however useful) to take the place of established truth. They have been careful not to advance what they thought possible, or probable, and the form in which they conceived it, as a demonstration of the actual course of things, but rather what they had proved by positive investigation, *i. e.*, experimentally.

A person capable of reflecting upon the things around him, and of considering them in an intellectual point of view, will naturally first seek to become acquainted with the object set before him, and to inform himself with regard to its conditions and powers of influencing other objects. But the activity of this impulse naturally varies with the individual, and some persons will advance far more speedily on the path to which it urges than others.

Let us now place the phenomenon in question in juxtaposition with others which perhaps resemble it in some respect, or with which we may happen to be better acquainted in their causal connection; let us compare and distinguish them, and endeavour, through the process of induction already alluded to, to arrive at a due understanding of them. To carry out all this correctly, we must be more or less familiar with all the phenomena and processes having a relation to, or connection with, the phenomenon in question; we must be at home in this department, perhaps also in several others. But seldom have two minds been so in the same degree, and still less in the same manner. To one memory and imagination will offer a large number of analogous and pertinent phenomena and objects, to another fewer; to the one certain series of phenomena will present themselves, to the other certain others; to the one all these facts will arrange themselves in a certain manner, while to the other they appear very different. Then ensues a necessary divergence in their attempts to explain what is before them; they will, in pursuit of the same end, adopt very different paths and methods, and still more, will carry them out with a very different degree of consistency or perseverance. For every one is not able to place that restraint upon his cravings for an explanation of the things before him which another may have attained to. While one follows the more certain but unfortunately more difficult path of investigation in detail, and of strict induction from experience, *a posteriori*, another omits or abridges much of this labour, perhaps from a conviction that in the case in question he may safely do so. His imagination and whole creative power, are, perhaps, greater, the flight of his genius bolder and more comprehensive than is usual with ordinary men; or he may be utterly incapable of that patient preparation of the separate pieces of his structure, with which another cannot possibly dispense. Many a one, in times past, impatient of a gradual advance, a slow ascent, has discovered, by a bold grasp *a priori*, the most important laws and correct theories. Others, therefore, will make the same attempt, and place the phenomena and processes in question in that connection with each other which they conclude to be the most probable and correct, *i. e.*, they will explain them *a priori*. They now persuade themselves that the things which they observe are truly such and no other than they appear to them. And thus arise those rash attempts at explanation, those theoretical *coups de main*, by means of which it has been attempted to explain whole series of phenomena and processes, or even an entire science, *e. g.*, Physiology, Pathology, or Therapeutics, before any of the elements and processes have been determined experimentally, or their causal connection demonstrated.

But it would be unjust, on the other hand, to reproach this whole *a priori* mode of advance—whether it may have led merely to individual hypotheses and combinations, or to connected theories and entire systems—with having proceeded altogether arbitrarily, from mere views and conjectures, without the support of an experimental basis. Even the most precipitate and incorrect theory starts from experiments and facts, and is, indeed, essentially founded upon them, because they are of necessity indispensable as materials to which the theorist must apply his explanations. No reasonable man will pretend to demonstrate his views and conjectures by mere hypotheses and opinions, or to explain by his theory things which have no existence save in his own brain. Every one, in short, grounds his theory upon certain facts and experiences, and seeks to demonstrate it by no other aid than theirs.

The error of the less correct and successful theorist does not lie in indifference to facts, but in his having ill understood how to employ them. His failure or error consists in the regarding as proofs what are not truly such; in the drawing of false conclusions from them, and thus arriving at an incorrect theory. On the other hand, the characteristic of the correct theorist—of the successful investigator—is, that he is able, from the previously unarranged and unexplained stores of his observations, and of the materials gleaned from experience, to surmise the exact connection between them, and to define and classify them in the relations they bear to each other. It is he who can, from a much smaller number of experiences, from more doubtful facts than are possessed by the former, deduce the truth, *i. e.* their real connection and precise laws.

But in each case it is from experiences and facts that information is sought, and it is these which claim our fullest attention if we would learn to understand and explain them. They speak to all, but it is only men of genius, it is only the chosen few, who, as of old, in the case of the ancient Proteus of Virgil's Æneid, are gifted to hear and understand them aright. And for those who fail we must remember that many allowances are to be made. The human mind, when confronted with nature, can only advance in the path of investigation, or explanation, according to its own peculiar laws; and an exact appreciation of the nature and modifications of these governing laws might perhaps, could we obtain it, serve to adjust the old and often troublesome schism between empirics and theorists —between absolutism (Positivismus) and idealism, or between a too objective and a too subjective science.

Many of these precipitate and erroneous attempts spring from the natural tendency, one might almost say instinct, of the human mind, to embrace within itself all the enigmatical, because unexplained and divergent phenomena, or results of experience. To attain this end, it strives to show that all those phenomena and occurrences are essentially of the same kind and belong to the same series, and thus it seeks to reduce the greatest possible number of them to the same causal connection, or perhaps to conditions and laws with which it is already in some degree familiar. The ultimate and essential reason of all this seems to be, that it is natural to us to seek to explain things unknown to us by the aid of things with which we are better acquainted. In our haste as soon as may be to satisfy our longing for the comprehension, *i. e.* for the explanation, of the objects and mysteries before us, we begin to deduce them from similar modifying circumstances and processes, from the same laws which we have already established for other more or less analogous phenomena and processes.

Instead of investigating individual objects by the strict process of induction from experience, and determining their causal connection, and then advancing gradually by generalization or reduction to higher or more general laws, we draw conclusions—from what is already known to us in more or less definite form—concerning what is still unknown to us. We thus deduce from those causal connections or laws, which have been demonstrated for one kind or series of natural phenomena, the order and laws of an entirely different kind or series; and thus endeavour to explain their origin and calculate their probable manifestation under circumstances totally new, and even, perhaps, partly or entirely unknown.

We call this mode of advance the process of deduction, as opposed to that of induction; or synthesis, as opposed to analysis. Thus the physicist can, from the laws which he has succeeded in establishing, whether they be those of gravity, of the pressure of fluids (hydrostatic laws), or of heat, &c., draw conclusions concerning many other more complex phenomena and processes, *i. e.*, he can explain their origin upon certain given principles, whether they occur in inanimate nature or in the living body, and whether singly or in connection with others. So can the chemist, from his laws of affinity and of equivalents, often calculate with certainty what will occur in untried cases, and even under different and entirely new circumstances. From the results of his inquiries regarding a certain number of substances and the laws of their combination, he draws conclusions concerning others, and predicts their behaviour before he has had an opportunity of investigating or becoming acquainted with them.

It is self-evident that this is the most convenient and the easiest manner of arriving at the comprehension and explanation of what is still unknown to us. Instead of investigating unknown phenomena and occurrences, we have only to class them along with others already known to us, if we have good reason to suppose that they agree in essential points, *i. e.*, in the circumstances which modify their origin and progress. And instead of the tedious process of inquiring what happens in a given case, and determining the conditions and causal connection of such phenomena by numerous indirect processes, we can now briefly say that they are constituted in the same manner as others already long known to us. In short, instead of employing the process of *a posteriori* induction, we may, *a priori*, deduce many things from the little that is with certainty known to us, and from the laws which regulate that little. And instead of dividing a whole group of phenomena—*e. g.*, a vital function or a disease—in the case before us, into its individual members or elements, and investigating them singly, we may, conversely, construct and explain such a group by synthesis, from the conditions or effects of the elementary processes which accompany it. If we had learned the causal connection and conformity to fixed laws of some even of the more important processes in the living body, *e. g.*, of digestion, nutrition, conversion of tissues, or of nervous action; if we knew with certainty not only what occurs in each of them, but also what modifies it, and by the constant coincidence of what circumstances and influences it is rendered possible, we could more easily thereby explain all those occurrences, and finally consistently deduce therefrom the conformity of all the processes which constitute "life." The same knowledge would also sufficiently explain the nature of disease in its mechanism, origin, and cure. Every vital process, and every group of vital processes, in a state of health or disease, would then be an effect or a result occurring necessarily under given circumstances already accurately known to us.

It follows from all this, that the desire for such a deductive science is a perfectly natural and necessary one. What wonder is it if we long for a state of knowledge which would give us, by the aid of a few maxims and laws, an understanding of all that occurs in the living body? We should then, through an exact knowledge of what is taking place in our patients, be enabled to deduce with certainty from a few higher principles, all the individual rules for our clinical practice. When we consider this want, the formation of so many theories and systems to satisfy it becomes intelligible. The same circumstance has been propounded as the essential condition or cause of the most dissimilar diseases: *e. g.*, irritation or debility, according to Brown; inflammation, according to Broussais; so-called blood-crasis, certain derangements of the functions of different parts of the nervous system, according to many of the moderns. In like manner, the same essential type, the same ultimate effect, has been assumed as the basis of the mode of action of the most various agents and remedies: *e. g.*, debilitation or invigoration, excitation or change of tone, &c.

An entire misapprehension of the actual state of things could, however, alone have induced any such attempts. While these men were in reality not far from the starting point, they believed themselves to be near the goal, and pretended to take or give what did not yet exist. Physics, mechanics, and in some degree chemistry, have gradually attained the rank of deductive sciences; not so physiology and the theory of diseases. In the former sciences men have succeeded in determining the similarity of several series of phenomena and processes: *e. g.*, the laws of gravitation and of hydrostatics, of chemical affinity, &c.; which are in consequence regarded as effects or results which must necessarily occur under determinate circumstances. We can, therefore, now predict what effects and results will occur under certain given circumstances with a definite assemblage of co-operating agents.

But, on the contrary, so long as no such natural laws have been established,—as is the case in the whole theory of life,—and so long as we do not know what really occurs in any vital process or in the course of any disease, nor by the co-operation of what circumstances it occurs, any deduction in the above sense is impossible. It is useless to attempt to promote the comprehension of yet unknown phenomena and processes by grouping them with others almost equally unknown, or to attempt to explain them by adapting to them laws which regulate other groups of phenomena and processes before these last are themselves well understood and clearly demonstrated. Even the natural philosopher is unable to calculate with certainty, from the laws of atmospheric pressure and gravitation, and from the elasticity of gases, the distance to which a projectile may be thrown. Much less can he calculate from the laws of action of individual agents—*e. g.*, of heat and of the process of evaporation, of the currents and electrical phenomena of the atmosphere—how a given state of the weather is to be brought into conformity to certain fixed laws; and this because the concomitant circumstances and influences are too complicated, the importance and office of each individual agency not sufficiently known to him. The laws of gravitation, &c., which he has so cleverly discovered, do not enable him to determine with certainty the behaviour of three bodies which attract or gravitate towards each other; still less can he calculate with certainty, *a priori*, the orbit of one of the heavenly bodies, but must seek to discover and determine it by exact observation. In like manner, no chemist can say beforehand what will occur when certain complicated organic compounds are brought into contact with other substances: this, in such a case, must be determined by experiment.

And how shall we pretend to say that in the theory of life, in which the phenomena and processes are confessedly obscure above all others, we know even so much as in the cases alluded to above, and that we are now in a position to venture upon a method of explanation which possibly for centuries to come cannot be fitly entered on?

We have endeavoured, in the foregoing pages, to give a general view of the means by which, in the department of medicine, as in all the natural sciences, we must enter upon the understanding of the causal connection and conformity of certain phenomena and processes. In considering the various modes in which it is possible to conduct the investigation of nature, we must be careful to bring the data submitted to us to the closest test; and while we contemplate the peculiar circumstances and difficulties in the objects before us, we shall understand more clearly the manner in which we must apply the methods of investigation, and adapt them to the peculiar nature of the phenomena and processes with which we have to do. Yet, after all, we shall find that the manner of tilling the intellectual soil must depend entirely upon the nature of the soil in question.

Our first aim, therefore, must be, as has already been fully explained, to determine by the observation of the phenomena and processes going on in health or disease in the living body —in a word, by experience—their causal connection and the conditions of their development. We have consequently to discover the causes of each phenomenon and occurrence, and the effects of each cause and influence, in short the special conformity to fixed laws, of each incident and process in the living body, &c. The most certain, and, at the same time, the simplest means of doing this would evidently be to take each phenomenon, each effect or occurrence separately, and to investigate it as much as possible apart from the others. This is, however, impossible; for since we are incapable of producing life and thus establishing its peculiar processes artificially, we must confine ourselves to the investigation of life as we meet with it in nature and reality. There, however, no phenomenon or process, and consequently no cause or effect, ever presents itself for our observation in an isolated form. Further, it is for the same reason impossible, in our observation of actual occurrences, to bring each individual effect into connection with its constant cause, or each determining circumstance,—each cause into connection with its constant effect.

At first, perhaps we think that we need not investigate very deeply to find the knowledge for which we are seeking; we soon, however, learn our mistake, and find that in the attempt to seize what is lying before us in a state of order and quiescence, we rather disarrange and displace it than render it more easily understood; for all things in nature, both without and within us, are in the closest connection with each other, and nothing occurs which is not in some manner connected with something else, and thus again with all. Every phenomenon, every occurrence, is connected by mutual relation with many others; nothing is to be imagined and contemplated by itself. All here is in a state of constant change, in continuous though definite progress; what it was at one moment it ceases to be with the next. In the language of Job, "it goeth by me and I see it not; it passeth on also but I perceive it not." This holds good even with respect to what we call "inanimate nature," which we regard as such only, because the occurrences and changes in it—its life—are not contained in the limited space and definite form of an organism, but are distributed in boundless space, and because the occurrences and constant changes do not display the sharp, distinctive features, or divisions observable in the other part of nature which we call "animated." In short—even in this relatively more simple and quiescent world, nothing is in a state of rest, nothing occurs in an isolated form, or has an independent existence. Here also every phenomenon, every occurrence, is modified by, and itself modifies, many others, *i. e.* is at once effect and cause.

If it be difficult in inanimate nature, and even in physics, to determine experimentally what phenomena and incidents modify others, and by what others they themselves are modified—how everything occurs in a constant progression, according to some fixed order and uniform plan—the difficulty is infinitely greater with respect to the phenomena which it is the province of the physiologist and physician to investigate. In the living body all the phenomena, all the functions, are intricately blended with each other, and may be compared to a network of threads in which each, even the smallest, acts upon the whole tissue, and in which every effect reacts upon that which modifies or causes it. All these individual processes and agents in the living mechanism are not only themselves in a state of constant change and progress, but are incessantly influenced and essentially modified by the most varying agencies of the external world. These external agents, themselves subject to continual change and modification, are also in the most intimate connection with each other, and none among their number can easily present itself for observation in an isolated form.

We thus find everywhere a complexity of circumstances, an oscillation between wide limits, and a mutability, not met with in any other department of the study of nature; a mutability going on, not only from day to day, but from year to year, and from century to century. Here also every phenomenon or occurrence has doubtless its fixed causes, every modifying circumstance or influence its fixed effects, and the conformity or order in which everything occurs is just as constant and as necessary for the constitution of the organism as in inanimate nature. But what occurs in the living body does not occur so uniformly and immutably as in the physical, mechanical, or chemical processes. It is not immediately and simply the effect or result of its own peculiar causes, at least it does not appear so to us, because we can never observe the effect of an individual cause or agent by itself, as the physicist can observe the effects of gravitation, or the chemist those of a single substance. With us many causes or agents affect the development and progress of the same phenomenon or process, and the latter often reacts upon others, perhaps even upon those which modify it. In addition to this, the whole state of a living organism, *i. e.* the aggregate of its phenomena and processes at a given moment, is modified by its preceding state, and modifies that which is to follow, whence every occurrence in life becomes more complicated in its conditions and development, and at the same time more variable.

Let us take, for example, an organ of our body which appears to act in essential conformity to physical laws—the eye. It is a physical, optical apparatus, and one of the most complicated kind, but it differs *in toto* from those with which the physicist has to deal, inasmuch as it sees instead of letting another look through it, and is in connection with a brain and mental eye, which see with it, and move and use it at discretion. All its glasses, lenses, curtains, &c. have life, are nourished and undergo changes from the first moment of their formation, and they are not only more intimately connected with each other than is the case in any mere optical apparatus, but further, they are connected with the machinery of the whole organism. They are thus not only under the influence of light, upon which their functions essentially depend, but under many other external and internal influences.

From these causes the eye may become useless in a manner which cannot affect any physical apparatus.

It is clearly indubitable that what occurs in the living body, occurs to a certain extent in conformity with physical and chemical laws. The material substratum of the body falls more or less within their range; it commences, lives, and decays under the constant influence of inanimate nature, and draws from it the materials with which it builds up its structures. So soon, however, as these materials enter within the magic circle of the living body, they do not merely combine according to ordinary chemical laws, but pass into a new development; and no less novel and peculiar are the phenomena which they now offer and produce. These phenomena constitute something more than the mere summing up of their individual component elements. Albumen, gelatine, fat, water, salts, &c. may be furnished from without, and form the proximate components of our mechanism, but only within this mechanism are they converted into blood for the sustenance of the body; only in it do they live and unite to form muscles, or become the medium of sensation, of will, and of action. The living body has, so to speak, digested and converted them into itself, and from this moment they begin a new and higher cycle of existence and action. In like manner, in the comparatively simpler processes—in digestion and circulation, in the conversion of tissues, in respiration, or in the excretion of certain substances—chemical and physico-mechanical action evidently play a very important part; but not exactly in the same manner as in what we call inanimate bodies. Other processes and activities, including the most intellectual, are here at work, and this in conformity with entirely different and peculiar laws. Thus the final effect, modified as it cannot fail to be by these varying processes, is not simply a physical or chemical one. The same chemico-physical processes are, no doubt, going on in most diseases, *e. g.* in derangements of digestion, nutrition, excretion, respiration, transudation, exosmosis, &c. And while, in the case of remedies, we are obliged to take into consideration the many processes interweaving with many others, which are again dependent upon their own peculiar conditions and upon agents, acting each in its own peculiar manner, and according to its own peculiar laws, we must remember that the same principle in essentials, runs through the laws which influence the whole external world.

We are encountered, in inanimate nature, by enough of this complexity and intricacy. The physical, and still more the chemical, phenomena and processes are by no means immutably the same, even in our solar system. But the elements with the phenomena and properties of which physics and chemistry have to deal are, in a comparative sense, infinitely more simple and, for a given time, more stable than the living organism. They admit, therefore, of a more direct and adequate observation by individuals, and do not require to be observed so frequently and under such manifold varieties of circumstances. The mechanical properties or forces of these substances are constant, *e. g.*, weight, extension, elasticity, &c., and if a certain result or phenomenon is produced by a variety of entirely different forces, or rather, if certain substances have seemed to coalesce and appeared to act in different directions and according to different laws, each substance has none the less acted for itself, each of its effects has occurred by its own unchangeable laws, and the whole result, or total effect, is only the sum of the individual effects taken together. Even if, in some cases, the mutual limitation or suppression of individual effects has produced various modifications of the total result, or given, as it were, a medium result, the physicist will be the better able to comprehend their conditions and mode of occurrence, from the fact of his having previously determined their individual laws more or less precisely. With the chemist it is somewhat different, for even with respect to inorganic substances, the processes he has to attend to are not only much more numerous, complicated, and variable in themselves, but become so in an especial manner, from the fact that when two or more substances come together under favorable circumstances, no mere addition of the one to the other, but rather a new development, with peculiar properties and effects, is produced. These chemical processes bear a much closer resemblance to those of the living body than do physico-mechanical processes, and their determination is therefore a more difficult task. But if, by the combination of oxygen with hydrogen, of an acid with an alkali, or of a salt-radicle such as chlorine or iodine with a so-called base, an entirely new substance is produced, its properties, though indeed new and peculiar, will be found to be no less constant than they were in the more simple elements, and, like them, may be accurately tested and determined.

We may thus safely assume that the phenomena and processes with which the physicist or chemist has to deal, are more simple—that they occur under less complicated circumstances, and under the influence of less numerous agents, than those of the living body. They change also and oscillate within more confined limits, and even these oscillations and modifications often recur with a certain regularity (*e. g.*, the motion of the heavenly bodies, the hours and seasons, the weather, &c.) whence it is easier to determine these oscillations and variations, both as to their order and their extent, by antecedent or simultaneous circumstances. It must also not be forgotten that both the chemist and the physicist are able to produce artificially, and at will, by far the greater number of their phenomena and processes, and thus to place them in the circumstances most favorable for the investigation of their conditions or effects. The physician, and especially the pathologist, must confine himself to the investigation of life as it is presented to him. The physiologist, on the other hand, is more favorably situated, inasmuch as the phenomena and processes with which he has to deal, are less complicated, numerous, and variable, and are thus better adapted for investigation. Above all, he can advance more directly by the aid of experiment, and though he cannot indeed produce life, he can, in some degree, vary its conditions.

The particular disadvantage, however, under which both labour, is the circumstance that they are not able, like the physicist and chemist, so to isolate the manifold phenomena, processes, and functions of the living body, as to remove any one of them from the magic circle of the whole, without deranging or arresting the entire machinery. We are unable, for example, to place the lungs, the alimentary canal, the liver, the brain, the process of respiration or digestion, the circulation, &c., so as to observe the several occurrences in them singly, nor can we submit them to such changes and influences as may enable us to determine the conditions of their development and action, and to observe in what manner their whole series of occurrences conform to each other. Both physiologists and pathologists, but especially the latter, are thus forced to embrace the living organism as a whole in their observation, and when an experiment is made upon it by the former, the changes in phenomena or processes which occur—the effects produced—follow the same order, and are in essential agreement with the same laws of the vital phenomena which are the subject of our inquiry. In every experiment we must include most, if not all the vital processes, and we must therefore take into account a complicated whole in its possible variations resulting from certain influences. It is therefore difficult to ascertain and precisely appreciate either the mechanism of an individual process, or the changes produced in it by the different external and internal circumstances which affect it. Equally difficult is it to withdraw the living body for any considerable period of time from the action of these other influences for the purpose of investigating separately their mode of action upon each of its different processes because, as in the case of the air, temperature, light, aliment, (and this holds good also of certain remedies), it can only exist by the co-operation of them all.

We thus see how rarely, in our branch of science, it happens that an individual influence or cause, or still more, an individual effect, presents itself for observation in an isolated form, in such a manner as enables us to view the cause in immediate connection with its effects for the time being, or the effect with its causes.

A more comprehensive sketch of the field of study on which we have entered, and its peculiarities, can only be given in connection with the necessary methods of investigation. What has been already said, may be enough to show that while the investigator of what we call inanimate nature may be said to be comparatively master of his field, in ours we may rather be regarded as slaves, necessitated, for the most part, to struggle on as best we may, along the course of the ever-moving and almost unfathomable stream of life. The physicist and chemist can dive more easily into the depths of the great stream of *their* inquiry—they may even divide it into smaller streams—they can put their questions in a more definite form, and extract, by observation or experiment, clearer answers from each separate fact which they have already seized. And where they have been least able to effect this as, for instance, in meteorological and many chemical processes, or throughout the whole field of climatology and geognosy, or in the study of heat and of the electric and magnetic phenomena, they have been unable to demonstrate any constant connection between causes and effects, any positive regularity or so-called laws. Yet even in this field of study they have advanced to the permanent occupation of positions of importance from whence they may safely advance further.

The department of medicine rather approaches that of the psychologist, the moralist, the teacher, the historian, or the statesman, who, like us, have to deal with the most complicated phenomena and processes, with an aggregate of causes and effects, without being able to produce these phenomena, artificially, or to take to pieces and manipulate what they meet with in nature and reality. And thus these inquirers have, like ourselves, been unable to arrive at a clear insight into the causal connection of the phenomena before them, or at the establishment of any fixed laws, though they have adopted many views, hypotheses, and theories, concerning which, like ourselves, they are daily at issue.

Like ourselves too, they are liable to be led astray in their artificial processes and rules, both by calculation and by conjecture, because their whole system of observation and investigation contains, like our own, but too many sources of error.

SECTION III.

ON THE PROCESS OF INDUCTION PROPERLY SO CALLED, OR THE SEARCH, BY IMMEDIATE OBSERVATION, INTO THE CAUSAL CONNECTION AND LAWS OF THE PHENOMENA AND PROCESSES WITH WHICH WE HAVE TO DO. AN INQUIRY INTO THE SIMPLE METHOD OF EXPERIMENT AND INTO THE INDIVIDUAL STEPS AND MEANS BY WHICH IT MAY BE FACILITATED.

In the foregoing section we have considered in their more general aspect the possibilities and difficulties of investigation and the claims of the facts with which it has to deal. Our present object is a far more important one, viz., to examine in detail the several aids to investigation, more particularly those which offer themselves as it were spontaneously, and which, since their claims to attention received so powerful an impulse from Bacon, have generally been first employed in the investigation of nature.

Our scientific task consists, as has been already stated, in endeavouring, as the physicist and the chemist have done in their peculiar departments, to demonstrate the causal connection, or the conditions and conformity to fixed laws, of everything which occurs in the living body, whether in a state of health or disease. It is incumbent on us not only to seek to determine everything which occurs in the functions and processes of the body, and to show wherein these processes consist, but also to discover their mutual affinities and relations to each other and to the various external influences, and to ascertain how far such known relations bear us out in regarding certain of them as dependent upon certain others. It is evident, however, that we must here confine ourselves to what we are able to learn through exact observation and examination of things as they really exist; an existence and reality which must, whether it be in the case of the living body, or in the external world, (in which, and by the co-operation of which, the body lives), alike be established by experiment.

We have already learnt enough to know that we can only attain our object—

I. By confining ourselves to individual phenomena and groups of phenomena in the living body, *i. e.*, to its functions in health and disease, and—-

II. By observing all those phenomena and circumstances of the external world with which the processes in question, in our body, are in constant natural relation, and under the influence of which we observe them to occur.

III. It must also be our aim to determine by observation which of these phenomena and processes in the living body constantly occur simultaneously or consecutively, and in what constant order—in short, under what external and internal influences and circumstances a given occurrence or phenomenon invariably presents itself to our observation. Having once succeeded in the latter attempt, we might, under favorable circumstances, and with the reservation of certain precautionary measures, regard what had been demonstrated as the invariable forerunner or concomitant of the phenomenon or process in question, as its condition or cause.

And through this discovery of the causal connection and conformity of these phenomena and processes we might look upon our problem as solved. Yet if we proceed to the observation of those phenomena, &c., with this intention we shall soon find that we cannot possibly hope to attain our object by such means alone. We find before us a chaos of all possible kinds of phenomena, all presenting themselves simultaneously, and offering, at the outset, an inextricable confusion. Every one who observes a sick person for the first time, feels confused by the mass of phenomena, of so-called symptoms of disease, which offer themselves to his observation simultaneously and, apparently at least, without order and internal connection, sometimes in one form, sometimes in another. Every attempt to arrive by simple observation at a correct and methodical conception of all those phenomena, and still more at a knowledge of their relations to each other, or to understand which of them are to be classed together, and which constantly occur together, appears vain. To prepare the way for such an insight we must, first of all, group, and be able to place side by side those which belong to a certain series—to bring them into connection with each other—in short, to effect a certain regularity in this apparent confusion. Fortunately, nature herself renders us this first service, for we actually find in this stream of phenomena and events, a certain arrangement which we are able to recognize without much difficulty. Many of them are connected with each other, and always occur simultaneously because they aid, and in fact are necessary to, each other in producing some process or event which could not be developed without their co-operation.

They thus present a certain ensemble, or assemblage of phenomena, always—with comparatively slight deviations—to be met with, each of which may be recognized and distinguished from its fellows, with which, as does each individual wheel in the works of a watch, it is used to work separately yet in concert. Among such, we may place the individual functions or processes of the living body, (as they result from the co-operation of its respective organs), such as digestion, respiration, circulation, the various processes of excretion, motion, sensation, &c. We establish a certain order in all the phenomena (symptoms) in the sick under our observations so soon as we are able to refer them to these different functions, organs, &c., and view them as what they really are, namely, simple change or derangements of those functions. In a similar sense, everything which we observe in external nature, *e. g.*, the atmosphere and the weather, heat, light, and electric phenomena, earth and sea, solid and liquid food, presents certain connected unities or collections of phenomena.

By keeping our attention, in the first instance, steadily fixed upon these, and by considering them, in their various stages, just as they present themselves to view, we at once put ourselves in possession of important data, and obtain materials which the mind can further employ in its investigation of cause and effect, and in tracing the conformity of these phenomena and influences. We should therefore observe more closely, in all their details, the phenomena and processes in a given function, or disease, &c., and compare them with such as we recognize in other cases more or less similar; and thus seek to determine the circumstances under which a given phenomenon or process occurs.

Up to a certain point, all this is facilitated by the circumstance that many of those series of phenomena present a regular and more or less constant course from beginning to end,—as in the development of an organism, the formation of its individual parts, as well as many acute, so-called epidemic, or contagious diseases. Here, as in the former case, groups of certain peculiar phenomena present themselves, in a more or less striking form, to our observation, enabling us to distinguish and recognize them with readiness. They also present themselves in a certain cycle, which remains essentially the same in all cases of the same kind. In the same patient, the same series of phenomena often recurs with regular periodicity,—as, for instance, the exacerbation of continued, and, still more strikingly, of intermittent, fever. We may quote, as analogous facts, the recurrence of sleep or waking; a certain state of the weather in the different seasons; the ebb and flow of the ocean; and the strictly periodical occurrence of all the meteorological processes in the tropics. And since the latter have excited much more attention in tropical countries than with us, so also have those countries been the nursery of meteorological studies, just as those fevers, and other acute diseases, have been the nursery of the doctrines and theories we have arrived at concerning them.

But, with all this, we gain no insight into the proper causal connection of these phenomena and processes, or into the conditions of their development; in other words, we have yet to understand them. Certain causal connections are, it is true, so obvious, that the simplest experience may guide us to them: we are not slow in learning that our respiration is dependent upon atmospheric air, our nutrition upon the supply of certain substances, our capability of seeing upon light, &c. In a similar manner, simple experience has shown certain circumstances and influences to be the causes of many diseases,—as, *e. g.*, exposure to cold, errors of diet, miasmata, poison, the virus of syphilis or smallpox, and the like. Simple experience has also enabled us to assert, that certain drugs furnish the conditions of numerous cures,—as, *e. g.*, cinchona and iodine,—just as we have learned to recognize the frost of winter as the cause of the formation of snow and ice, the heat of summer as the cause of thunderstorms, or of the development of vegetable life. But we are as far as ever from being able to explain the origin of these effects. Here, as elsewhere, observation only tells us that certain phenomena or processes occur simultaneously with a certain event, or immediately follow upon some other event or influence.

It cannot tell us how these phenomena actually occur, nor yet how they are internally connected; neither does it enable us to bring all the phenomena and processes which we have observed, whether as occurring simultaneously or consecutively, into their exact relations to each other, and to say which of them has played the part of cause, which of effect. The most dissimilar things are here occurring at the same moment, even in each individual function, as well as in each of the agents which affect it from without, and perhaps help to modify its condition. Many other influences, both from without and from within, affect those processes; which influences are connected with, and perhaps mutually modify, each other.

Suppose we wished to investigate the process of respiration, and to determine, by simple observation, what really occurs in it, and what are the circumstances and influences upon which it may depend. We should find that respiration is the effect, or result, of numerous processes acting in concert (*e.g.*, respiratory movements, inspiration and expiration of certain materials in a gaseous form, certain changes in the blood), and, also, that many other processes in the living body, being more or less closely connected with these processes (*e.g.*, circulation through bodies like the lungs, the process of excretion by the skin, kidneys, &c.), help to modify their general result in the act of respiration.

And, among the external influences which obviously affect respiration, we may remark, that the atmosphere, with its composition, density, &c., is intimately connected with many entirely different processes,—*e.g.*, with the circulation, with excretion by skin and kidneys, and with innervation. The same truth applies to every form of disease, and to each event or symptom that occurs in it, as well as to its origin and cure. Phenomena and processes the most dissimilar, yet, at the same time, connected with and modifying each other, present themselves simultaneously to our observation; and these, in their turn, have been preceded by the most varied circumstances and influences, which have themselves possibly helped to modify the origin and cure of such disease.

It is thus clear, at the first glance, that we cannot hope, by the simple observation of these phenomena and occurrences, of these causes and effects so constantly connected with each other, to obtain an insight into the actual processes in them, or to ascertain which of them are modified by, and which modify others. All this would be a comparatively easy matter, if we could—as has been already explained—observe each process in the natural course of things, separately, and under various circumstances; if we were able to test each phenomenon, each effect, as well as each active and modifying influence which more particularly interests us, in isolation from the rest; to produce this or that modification in them, and then to observe all the occurrences resulting from it. Thus, the chemist can separate his compounds into their individual component parts, and examine these as to their properties and their behaviour towards other substances. He can even reconstruct from these elements many of his most complicated compounds; and approach, by direct investigation, nearer to the comprehension of his processes, &c., and their conformity to fixed laws, than is possible in the investigation of animated nature.

If we cannot place our phenomena and processes separately before us, and examine each of them by itself; if we must (as was assumed in the induction, properly so called, from our observation of the natural state of things) confine ourselves to the observation of these under their obvious aspect, but one method remains to us for the proximate attainment of our end. We must first of all picture to our mental eye those groups of phenomena, &c. as separated into their individual component parts, and form a notion of the latter in an isolated state. In other words, our first task consists in seeing, in the aggregate of vital phenomena, &c., not so much the whole, as its individual parts and elements, *e. g.* the individual phenomena and processes which constitute a given function, or an entire disease. We must further consider the individual circumstances of the great and complex agencies of the external world (*e. g.* the atmosphere, climate, food), by which they may be affected. And since these elements never present themselves to our observation under a single and individual aspect, we must the more earnestly endeavour to form an abstract notion of them, and represent them to our minds in a possible and probable existence. To seek for them in earnest is to demonstrate their existence from the establishment, on each occasion, of their phenomena, properties, effects, &c. And before we can ascertain and demonstrate certain circumstances to be the determining causes of certain other processes and phenomena of the living body, we must assume the fact of their possible connection and mutual relationship, and then proceed with our investigation in that direction.

The true importance of hypothesis and surmise, in fact, of our whole subjective position and interference, is to be found in this first necessary step of investigation. We are apt to err in the analysis of our phenomena, processes, and agencies, and still more in our appreciation of the importance, action, or conditions, of the individual elements, from our acknowledged inability to isolate anything, and to submit it to direct examination, without the intervention of our *ego*. The more genius and scientific speculation any one brings to this task, the easier he will find this first step, and the more he is supported by positive knowledge, and acquaintance with the subject, the better he will succeed in it. For his surmises or views will the more readily coincide with the natural state of things, and he will be able to choose more consistently appropriate methods of investigation, and to seize upon the points of his subject best calculated to lead to the discovery of truth.

Having watched over our phenomena and processes until, through surmise, observation, and analogy, we have been able to gather them together, and build them up into a certain unity, we have next to investigate the secret of their connection with each other. The question is, therefore, how, amongst all those elements and individual phenomena, or processes, our observation may best discover which of them depend upon, and which of them modify, each other. We must seek, amongst the mass of phenomena and processes, to ascertain the effects of an individual determining circumstance, or the conditions and causes of an individual occurrence, or effect, and then demonstrate from experience this connection between them.

There are evidently two modes of proceeding in our investigations. We may either confine ourselves to the phenomena and occurrences in question as they present themselves to our observation, as the complicated results of certain others (*i. e.* of certain conditions and agents), and direct our investigation to a so-called function, or to a disease, and to the individual phenomena or processes which accompany it; or we may seek to acquire a clear comprehension of the circumstances and influences which we regard as the probable conditions of a certain occurrence or phenomenon. Our investigation is, according to the latter method, directed to the determination of the individual relations, or circumstances, by the influence and co-operation of which we see a function, a disease, or a cure commence and proceed. In the former case we examine by what circumstances and influences a given event, or process (*e. g.* respiration, diarrhœa, narcosis) is effected; in the latter, on the contrary, we examine what phenomena, what effects, a given circumstance, or influence (*e. g.* atmospheric air, an error in diet, poison) may produce. While, in the former case, the subject is attacked as it were *a posteriori*, in relation to effects, in the latter it is attacked *a priori*, in relation to causes.

The former method leads us, first of all, to observe the manifold circumstances under which the phenomenon, or occurrence in question, presents itself, *e. g.* respiration, or the excretion of carbonic acid, the concomitant generation of heat, the various phenomena of sensation and muscular irritability; in like manner, that which we call inflammation, or its individual phenomena and events, *e. g.* increase of temperature in the part itself, the occurrence of exudations, or the formation of a crust, &c., with their spontaneous changes in the course of the disease, or after the employment of certain remedies. We have further to observe these phenomena and occurrences under other circumstances, to submit them to certain influences, and to determine the changes or modifications produced by these, that we may ascertain to which of them they stand in a constant dependent relation. We may thus hope to ascertain, under favorable circumstances, the conditions upon which we are justified in considering them dependent.

In the latter method we have to observe, first of all, the phenomena and occurrences, with their possible changes, as they arise during and after the operation of a certain circumstance, or influence; we have to observe, *e. g.* all the occurrences and changes produced by the inspiration of atmospheric air and of various other gases in the lungs themselves, in the composition and circulation of the blood, in the generation of heat, or in certain excretions. We must also observe all the changes or modifications which take place in vital processes and phenomena through the operation of cold and heat, or through the ingestion of a known aliment, or drug. We must submit the various processes or functions of the living body to the influence of one identical agent, under manifold circumstances, and observe the modifications produced by it. Under favorable circumstances, we may now bring the incident, or influence in question into causal connection with some or other of those phenomena and occurrences, and may regard it, to a certain extent, as the cause or condition of the latter, and thus ascertain the effects which it is capable of producing.

A few examples may serve to make our mode of advance by either of these methods more intelligible.

It had been observed that the blood which returns from the lungs to the heart has a brighter colour, and is in a different state from that which passed from the heart to the lungs. It was also in course of time conjectured that such change in the blood must depend upon certain processes in the lungs, especially upon the inspiration of oxygen. For the determination of the connection between these two processes we had, first of all, to ascertain in what these changes in the blood itself consist. We had further to observe the dark venous blood under the most varied circumstances, to submit it to the operation of various gases, &c., and to test whether the changes it underwent under such influences were kindred to those which occur in the lungs in respiration. Should these changes occur only after the action of oxygen, we might regard that gas as being the most important agent in effecting those changes in the blood in respiration. We might, however, have attained the same knowledge if, in our experiment with oxygen, we had—whether by a fortunate accident, or by a happy surmise, as to the peculiar conditions of those phenomena—caused it to be inspired, or submitted venous blood, drawn from the body, to its action. In like manner the fact that lead may produce certain diseases, as colic, paralysis, &c., might have been discovered during an investigation into the state of health of certain workmen in manufactories, foundries, &c., or from an examination into lead compounds and their action upon men or animals.

Each method would thus lead us to the same point, just as the discoverer of gunpowder might have ascertained its composition and effects during an examination of the phenomena of explosion and combustion, and their conditions, or during experiments with certain explosive or combustible compounds. In both cases the same circumstance would have enabled us to recognize the conditions and necessary occurrence of a certain phenomenon, or effect, namely, the circumstance that we constantly saw a certain something precede or accompany the latter, and that in such a manner as to justify us in regarding it as its cause. And we might do this the more safely as we had the earlier succeeded in observing the effect of a given circumstance distinct from other phenomena or effects, and the cause in question distinct from other modifying circumstances and conditions. Our main object would thus be to discover such cases in nature, or to produce them artificially.

We must also admit that we have only two means of ascertaining the causal connection of natural phenomena and processes.

1. The observation of everything which we meet with in nature, both within the living body and external to it; with a careful review of all the varying circumstances and incidents in the case before us.

2. Experiment,—*i. e.*, the artificial production of those circumstances under which we can best investigate the effect or cause in question, in relation to their conditions, operation, and causal connection.

We shall sometimes, according to circumstances, have occasion to employ the one, sometimes the other, of these methods. In many cases, the choice does not lie with us; and, in all cases, the two methods have practically a very unequal value, since it is to experiment, almost exclusively, that we are indebted for the means of observing our phenomena,—*e. g.*, a function, or a disease,—or the manifold influences which affect them, under such varied and changing circumstances, that we can draw any conclusion concerning their connection with each other, or concerning the conditions of their development and operation. We seldom, if ever, find in nature such a multiplicity of circumstances, or such a degree of clearness in them, and, especially, such an isolation of the occurrence or influence in question from others, as would enable us to draw from the simple observation of the natural course of things a definite conclusion concerning cause and effect.

It depends, in like manner, upon the peculiarities of a given case, whether we have to proceed to the investigation of the occurrence or phenomenon in question, and of its possible conditions, or, inversely, to that of the influence or condition in question, and of its possible effects. Here, also, we frequently have no choice. But, since it is infinitely more simple, and at the same time more safe, to ascertain the possible effects of a cause, or of a given circumstance, than to proceed inversely and seek for the possible conditions and causes of an occurrence, or of a given effect,—especially under such complicated circumstances as we have to deal with,—we must always begin with the former whenever it is possible for us to do so.

In the employment of both methods, we receive important assistance in our investigations from certain auxiliary processes. And difficult, or even impossible, as it may be to arrive, by the observation of groups of phenomena, as such, at a comprehension of their causal connection and conformity, we may frequently, however, obtain thereby certain formulæ, and a knowledge of certain facts; and thus, at the worst, acquire some compensation for our failure in obtaining a more complete insight into them.

These auxiliary processes in our investigations are the following:

1. The observation of the phenomena in question under the greatest possible variety of circumstances and relations.

Varying the circumstances.—Though we are not in a position to investigate separately the processes in the living body, and the aggregate of all the influences of the external world which affect them, we can yet frequently observe certain of them under circumstances which afford us approximately correct information concerning their causal connection. Our acquaintance with a previously unknown phenomenon will approach the more nearly to a comprehension of it, the more manifold the circumstances have, in the course of time, become, under which we have been able to observe such phenomenon, &c.

It is experiment, as has been already stated, which is here indispensably necessary to us.

Do we wish to obtain a clearer notion of the influence of a given agent of the external world upon certain processes in the living body, by the aid of experiment, we can modify it in this or that manner; we can increase or decrease the intensity of its action,—its quantity,—and can observe whether the corresponding changes do or do not occur in those processes and functions which we conjectured would be influenced by the agent in question. If the former is constantly the case, the probability of such a relation increases; and we may now regard such process in the living body as dependent upon that influence,—as directly or indirectly produced by it. If we wish, *e.g.*, to investigate the connection between the temperature of the atmosphere and that of our own bodies, or a certain excretory process, or the origin of a disease, we may raise or lower the temperature in a closed space, and allow it to act for a longer or shorter time, and observe the possible changes in the temperature of our own bodies, or in the excretions of the skin, lungs, kidneys, &c.

If we are restricted, as is often the case, to the observation of that which presents itself in nature, similar variations not unfrequently occur in the natural course of things; and while we see what occurs under these altered circumstances, what changes, *e.g.*, have taken place in the processes of the living body, we may infer a certain connection between them. In a study like ours, in which we cannot produce the individual phenomena and processes, and are equally unable to isolate them, or to bring them into contact with entirely distinct external influences, one of the most important objects in the investigation of nature is to observe them under the greatest possible variety of circumstances, before we venture upon a conjecture as to their causal connection.

This is facilitated by a comparative examination of the functions in the various series of living organisms, and at different periods of development and age in the same organism. In like manner, the influence of the atmosphere and of its temperature, moisture, composition, &c., will become more intelligible in proportion as we investigate and understand better the influence exerted by such agents over the human body and over the vital processes connected with its well being. We shall observe how these are affected by climate, situation, seasons, hours of the day or night, &c. Diseases themselves are but similar "variations of the circumstances" which nature offers us, *i. e.*, definite changes in the natural processes of the body, occurring and arranging themselves in this or that manner under certain circumstances. While we make it our study to compare with each other the greatest possible number of similar cases, with all the concomitant circumstances, in relation to the similarity of their occurrence and general appearance; while we examine in what points they do or do not agree, we acquire a certain notion of the probable conditions of some or other of these phenomena. If, in doing this, we discover that certain events (*e. g.*, a disease, or an individual derangement during its course) proceed in the degree of their development, intensity, or frequency, in direct progression with some particular occurrence or circumstance, external or internal, and observe that it guides their increase or diminution, we may conclude that these stand in a certain relation to each other and, if not in that of cause and effect, we may assume that they are each the common effect of another deeper seated cause.

Simple experience has taught us that the life and health of man are always, in a certain degree, regulated according to the circumstances of his outward condition, such as the quality of his food, the scale of his comforts, and the climate and cultivation of the country he lives in. We have observed this in the natural variation of the circumstances with which the course of the world presents us, *i. e.*, in a comparison of these events under the different modes of life of certain classes of men. We do not yet know, it is true, what the precise cause of those effects may be and what part each individual influence may play, but we know that those circumstances conjointly have conduced to the development of such effects. The same thing holds good with respect to the discovery of the relations between structural changes in certain organs of the body, or chemical changes in the blood, or between certain excretory processes, &c., and the given disease considered as a whole.

We can, however, much more positively bring certain phenomena, occurrences, and circumstances, into causal connection with each other, if we observe the same phenomenon, the same modification of an occurrence to take place whenever a certain circumstance, or a definite modification of any influence, or of an external or internal agent, occurs—whenever, in short, a certain sequent always follows a certain antecedent in all the cases observed by us. Thus we find coloured races only in hot, never in temperate climates, and when the inhabitants of the latter migrate to the former they gradually approximate more and more to the condition of the natives. We infer from this, with tolerable certainty, the causal connection between these climates and the peculiar colour of their inhabitants. We might make this inference with much more certainty, if we could prove that the influences to which the inhabitants of those different climates are subjected are precisely similar in all other respects, and that situation and temperature are the only points in which they essentially differ. In a comparison of all the countries and neighbourhoods in which intermittent fever has been observed as an endemic disease it has been found that, however much they may differ in other respects, they all agree in the circumstance that stagnant waters and marshes, or organic matter in a state of conversion or fermentation, detritus of various kinds, and the like, are met with in them. We are therefore justified in the inference that intermittent fever and the above-named circumstance stand in some relation to each other. And because it has been found further, that the number of persons attacked by that disease, and the mortality, even in the same neighbourhood, correspond more or less in different seasons and years, and even in different parts of the same city, (*e. g.*, of Rome), to the presence and extent of such marsh land, &c., the probablity of such a relation is increased. For, so far as we have been able to ascertain, these seasons, years, and different parts of the same city, differ in no other essential circumstance or influence from each other, except in the presence or absence, the greater or less extent and propinquity of such marsh land, &c.

This last method of observation, namely, that of "exclusion," is of the greatest importance in our department of science. We compare in it numerous cases, or series of cases, with each other, which, being alike in other respects, differ only in this—that the phenomenon, or event whose conditions we are seeking, occurs in certain of them and not in the others. If we succeed in ascertaining the constant existence of a circumstance (*e. g.* malaria), in one series, which is as constantly absent in the other series, we may infer with certainty that this particular circumstance is the condition, or at least an essential element amongst the conditions, of that event (*e. g.* of intermittent fever). If we wished to ascertain the functions of an organ such as the spleen, or liver, in the living body, the simple removal of it would be a direct application of the method of "exclusion." If animals recovered from the immediate effects of the operation, and we afterwards observed certain constant alterations in some of the functions, &c., we might regard such changes as resulting from the absence and non-action of those organs. When we see several persons taken ill in the same manner, after the same meal, we justly suspect some particular dish to have been the cause, so soon as we hear that one of the persons affected ate of that dish only, or that another person escaped who had eaten of all the dishes except the one in question. In an investigation of the circumstances which might produce the unusual liability of the inhabitants of certain neighbourhoods to bronchocele, cretinism, scrofula, &c., the large amount of earthy salts in the water of some of those neighbourhoods was suspected of being the essential condition of these phenomena. But since it was found, upon accurate collation of evidence, that those diseases are often absent in neighbourhoods where the water contains the salts in question in the same quantity, and, also, that in neighbourhoods whose water contains them, those diseases are not endemic, this hypothesis was shown to be false.

By the same method of "exclusion" we might elucidate the influence of our remedies upon the course and cure of a disease. Experience has long since taught us that persons suffering from essentially the same disease, recover with the same frequency and rapidity after the employment of the most various remedies, which, so far as we know, do not possess the slightest similarity in their properties, so that the positive influence of those remedies upon the recovery of such persons may well appear doubtful. But if it further appeared that, under similar circumstances, they recovered just as rapidly and frequently if no such remedies had been employed, the correctness of this conjecture would be demonstrated. Several drugs have obtained the reputation of substitutes for cinchona and quinine in the treatment of intermittent fever, because many persons have been observed to recover under the employment of them; but further observation has shown that persons so affected often recover with the same certainty and rapidity with no other aid than that of good food and care in healthy hospitals, so that the drugs in question may possibly have had no share whatever in the cure.

II. Comparison of similar phenomena, &c., with each other; employment of analogies.

Whenever we see any phenomenon, or object, and reflect upon its nature, origin, &c., other more or less similar phenomena and objects, of which we already possess some knowledge, occur to our minds in connection with it. Those phenomena, however, will occur to us most readily, which exhibit at the first glance the greatest resemblance to those in question, whether in their sensibly appreciable properties, or in relation to the circumstances of their occurrence and manner of operation. When we see the effect of nitrate of silver upon the skin, or narcosis produced by opium, we are, perhaps, reminded, in the former case, of the effects of caustic potash, sulphuric acid, or fire; in the latter, of intoxication, or sleep. If we see an individual suffering from pain in the chest, cough, expectoration, other cases occur to us in which we had observed essentially similar symptoms, or we recall descriptions of those diseases which we had met with at some former period. We now compare more minutely with each other the phenomena in both series, and surmise the possibility that because they correspond in certain points, they may also do so in several others. This surmise will gain in probability as the points of correspondence are proved to be more numerous and important, and the more we are able to trace back these relations to a common origin and nature, and to prove that the differences between them may be referred to comparatively subordinate, or accidental circumstances and influences; in short, the analogy must gain in strength, in proportion to the constancy with which the resemblances it has to deal with exhibit themselves, and will be further enhanced by such appearances being strictly confined to the objects in question.

The foregoing instances will thus lead us to infer with added certainty that there is a correspondence between corrosive, or narcotic substances, both in their whole mode of action, and in all their essential properties, so soon as we know that not only the individual phenomena of their operation (in the former case the visible changes in the corroded parts, in the latter, stupefaction, tendency to sleep) are similar, but also that the same similarity may be traced in the chemical character of their components, and in the action of these on the tissues and processes of the living body. The so-called anæsthetic agents may now be regarded as analogous with greater fitness than ever before, since it has been shown that they essentially resemble each other, not only in their definite mode of action upon men and animals, but also in their chemical composition. For they all contain, so far as we know, carbon, and are more or less complex combinations of that substance with hydrogen. In like manner we might, from the analogy between so-called acidity of the stomach, in many derangements of digestion, and other acids, more justly infer that alkalies, &c., which neutralize the latter, would by virtue of their neutralizing properties, also prove beneficial in that disease, so soon as we could prove that the production of such acidity was a constant and essential circumstance in those derangements, or, at least, was intimately concerned in them. Something similar occurs in the diagnosis of obscure diseases, *e.g.*, of secondary syphilis; for the probability that, in a given patient, who presents, perhaps, several contradictory symptoms, we have to deal precisely with this, and not with any other disease, becomes greater the more the number of those derangements and phenomena which have been observed in other syphilitic patients increases in the course of the disease. Thus, we can more readily make out a writing in cypher or a difficult hand in proportion to the number of signs or letters it may contain with which we are already familiar.

The comparison of more or less analogous phenomena and processes has facilitated our comprehension of individual functions, as well as given us an insight into the nature of certain diseases. Thus, we compared, in the former case, the processes of respiration, of secretion or excretion by the skin, liver, kidneys, &c., both with each other and with analogous processes in plants; and acquired thereby an approximate notion, not only of the actual occurrences in an individual function, but also of their mutual action upon each other. In the latter case, we compared with each other the phenomena and certain anatomical or chemical changes in sick persons, and constructed, from those which were common to certain individuals, the so-called forms or species of disease, and certain groups of diseases, as perfectly as our existing knowledge of those phenomena and processes permitted, or our idea of them at the moment enabled us to do. The same thing will, in the main point, hold good with respect to the employment of various remedies.

Far more important is the question, how the use of such analogies may be made to further our investigation into the causal connection and conditions of our phenomena and processes, or into the laws of the operation of various influences.

The practical value of such inquiries will be in proportion to the fulness and exactness of our previous knowledge of the phenomena and processes which analogy has to assist us in comparing with each other. In the most unfavorable case, however, even if the modes of origin and operation of those things which we have placed side by side with certain others are equally unknown to us with the latter, the mere act of comparing them with each other may throw a light, if but in the way of surmise, upon both, and enable us to pursue our investigations at least in the track of probability.

In this, also, we have often in some degree succeeded. We have long known, *e.g.*, that sulphuric acid chars wood, linen, &c., and corrodes the living tissues; but since the great affinity of sulphuric acid for water, and the formation of the latter from the hydrogen and oxygen of those substances, has been proved to be the essential circumstance or cause of the former effect, the conclusion that its corrosive action upon organic tissues depended upon the same cause lay near at hand. Early experience had shown, that, in persons who had been treated with corrosive sublimate, this substance had entered, in common with other preparations of mercury, deeply into the system; for the so-called constitutional, or general, effects were the same; and the persons in question could even form an amalgam upon the surface of gold coins, by bringing them into contact with their skin. At a later period, it was found that certain albuminous ingredients of the gastric juice are actively concerned in the solution and digestion of food in the stomach; and, further, that corrosive sublimate enters into certain combinations with those albuminous matters in the stomach without being decomposed by them, and that this, its combination with a small quantity of albumen, is insoluble, but on the addition of more albumen, becomes soluble, and finally passes into the system. The observation of this fact led to the conclusion, that the salts of silver, zinc, lead, &c., might behave essentially in the same manner; and this has been confirmed by investigation. We had long known that bronchocele disappeared under the employment of sponge, seaweed, wrack, and certain mineral waters, before we could explain the cause. The only analogy which could be discovered between such different remedies was precisely this, their unexplained effect in that disease. When iodine was found, at a later period, to be present in sponge, &c., Coindet, in the comparison of those effects, came to the conclusion, that this substance was the cause common to them all; and, from that moment, iodine was employed with great success in the treatment of bronchocele.

It has already been intimated, that the comparison of several series or kinds of phenomena, processes, &c., and the consequent appreciation and employment of all their resemblances, forms an indispensable starting-point for our whole process of induction in the investigation of nature. The correct estimation of analogies, giving rise, as it does, to surmises in relation to the connection and conformity of various phenomena and objects in certain essential points, has always been a powerful stimulus to discovery. Discoveries have been seldom, if ever, made in one attempt; but it has usually happened, that an occasional resemblance between previously obscure and unintelligible phenomena, chancing to strike forcibly on some individual mind, has led to a deeper and more diligent investigation of such resemblance, until it has become easy to trace them up to a root by which they hold in common, and to show, through their various exhibitions, the same essential circumstances, the same causal connection and conformity.

Throughout the study of living nature, this attempt has hitherto been less successful than in any other department of science. Our employment of analogies may have led to a certain approximate grouping together of such vital phenomena, &c., as appear to agree in certain points, and perhaps to the conjecture of a similarity in their mode of origin; but we have never obtained sure proof of their agreement in their most essential circumstances and conditions, or a clear insight into their causal connection. Nor could this well be otherwise, because, in the application of such resemblances, or analogies, to the determination of the essential agreement of the things compared together, everything depends upon the exact resemblance of their analogous points, and, consequently, of the things themselves. We must, therefore, first of all, be able to demonstrate that the circumstances, or points, which are common to the phenomena compared and grouped together by us, are, in fact, the essential and determining ones: *i. e.*, those upon which the other circumstances, phenomena, or effects, of the objects compared together by us depend. But this pre-supposes a comprehension of the latter,—at least of some of them,—which we do not, so far as concerns the study of living nature, or of medicine, yet possess. For this reason, no comparisons and analogies can, in themselves, render us any essential service in the comprehension of our objects of study.

The simple confirmation, *e. g.*, of the points in which two or more phenomena and processes in a healthy or sick person agree, is difficult enough where, as our actual case stands, we are not in a position to observe them directly. In addition to this, the things compared with each other by us (*e. g.*, various diseases or functions, the effects of certain hygienic influences, or of remedies and poisons) never agree in all points with each other. They may do so in several, and perhaps, in essential points, but not in others. The correctness of our comparison thus depends upon the relative value of the properties and phenomena, upon which we have based it. We must have been very fortunate if we have selected precisely the most important and influential amongst them, and all guarantee for this is generally wanting so long as the latter are themselves partly or altogether unknown to us. We cannot even be certain that we have observed all the phenomena, properties, effects, &c., of the objects of our investigation. We are thus constantly in danger of mistaking what is comparatively accidental and contingent, for what is essential and determining, and *vice versâ*. There is always a possibility that we may, from the agreement of the things compared together by us in some external, accessible phenomena and properties, have falsely inferred their agreement generally and in essential points.

We must here further take into consideration that even our notions of any given disease and of the so-called types and unities of disease in general (*e. g.*, fever, inflammation, gout, rheumatism, &c.), are more or less obscure and fluctuating, because they have arisen, in a great measure, from a very superficial comparison of certain phenomena and processes, which are themselves fluctuating and vary greatly in their simultaneous or consecutive occurrence on each occasion, their real connection being more or less unknown to us (*e. g.*, certain phenomena in the sick, as those of temperature, circulation, innervation, or of certain excretions, or anatomical and chemical changes). The same holds good essentially of ætiological and hygienic influences and of our remedies. We find these also in groups, the arrangement of which is founded, not upon a comprehension of their more essential points of agreement, but upon their remote and often doubtful resemblance in relation to certain phenomena or processes. The comparison of a number of them with each other is, on this account, little calculated to facilitate our comprehension of them. In our peculiar department of science, and in the present state of our knowledge, analogy and comparison too often, from their indefinite and arbitrary character, deserve rather the name of allegory or simile. The very words which we employ to designate our objects (*e. g.*, fever, inflammation, rheumatism, hemorrhoids, or alteratives, corroborants, purgatives, astringents, counter-irritation, &c.) appear infinitely better adapted to give rise to and propagate indistinct, allegorical notions, than to promote the investigation and better comprehension of the objects to which they are applied. They may suit the poet, but not the scientific investigator.

When we compare our objects with each other, for the purpose of drawing conclusions concerning their general and essential agreement in certain points, it is often much as if a mineralogist were to infer the identity of his substances and minerals from the similarity of their crystalline form, for, in reality, very different substances may assume the same form, and the same substances appear in very different forms. In like manner, sweet and bitter almonds would appear essentially similar if we could compare only their external characters, and had learnt nothing concerning their chemical constituents, and the properties and effects of these. Our comparison or classification of them according to their former similarity would have been, as we now know, in many respects a false one. The analogy of the chemist, on the contrary, is a perfectly correct one, when he associates fermentation with putrefaction, or soap with plaster, and further, their mode of origin; for he has demonstrated that they agree in certain essential points. The chemist and the physicist can compare with each other many of their objects and processes and infer their innate resemblance from their analogies in certain points. They may thus attain to the comprehension of a wider field, presenting numerous series of objects, phenomena, and processes. With us, on the contrary, most of the phenomena and processes in the living body have remained too obscure for us to deduce from their similarity in certain points the fact of their essential agreement, *i. e.*, of the similarity or identity of their causal connection, conditions, or effects.

Analogy and comparison will be most useful to us when we endeavour by their aid to illustrate still obscure phenomena by means of others, the causal connection, condition, and laws of which we have already succeeded in determining; for it can aid us little to compare two or more partly or entirely unknown phenomena, &c. And however advantageous it may be to be able to draw conclusions from the actual steps and established conditions of a process, concerning others which are analogous in several points, it is, for the most part, equally dangerous, or at least useless to do this in those of which the causal connection is doubtful. Our deduction of one from the other cannot possibly further our insight into their nature, any more than it would facilitate the comprehension of a savage, to tell him that the mode of origin and the conditions of lightning are the same as those of the electric spark, or that Paris resembles Berlin in certain points, both being equally unknown to him.

It can thus assist us little, in the comparison of the doubtful phenomena and processes with which we have to do, to draw a parallel between the still unexplored processes of respiration in the lower orders of animals, and respiration in the mammalia, or between the latter and excretion by the liver, or skin; or to compare inflammation with fever, fever with fermentation or dysentery, and cholera with ague and the so-called miasmatic diseases in general. It is the natural consequence of our imperfect insight into the essential, and, therefore, constant processes, or states in our patients, that we are unable, *e. g.* to draw conclusions from typhus and its phenomena in one country, which may hold equally good concerning its nature in another, or to infer from the symptoms of intermittent fever and the efficacy of quinine in one place, that the same symptoms will appear, and the same thing prove efficacious in another. The frequent inference that a medicine which is beneficial in one disease will be so in another, even if both agree in certain circumstances, or phenomena, is still more hazardous. We can by no means infer from the fact that an astringent is useful in certain external diseases, that it would be so in more or less analogous internal diseases, as diarrhœa and dysentery, blenorrhœa and suppuration of internal organs. We cannot even prove that those astringent substances come into contact with the internal tissues in the same manner as with the skin, or the surface of ulcers, still less can we prove that they act in the same manner upon both. The fact that the local employment of cold, mercurial ointment, &c., is often beneficial in simple cutaneous inflammation, and that a certain cutaneous inflammation is present in scarlet fever, led many to the conclusion that the same applications might also be found useful in that disease. This conclusion was, however, a very hazardous one, because the two diseased states differ in essential points, and before we could be warranted in employing remedies found useful in other cases of cutaneous inflammation, we must first determine the exact relation such inflammation holds to scarlet fever.

The foregoing pages have shown that in our whole field of study we cannot draw from analogy and comparison alone, conclusions whereon safely to ground the essential similarity of our objects, or to establish their agreement, whether in causal connection, conditions, or modes of operation. It is otherwise in chemistry and physics, because in them the causal connection and conformity of many phenomena and processes have already been determined, and because it has been found possible to show that many of the latter, which had at first merely been grouped together on account of supposed resemblances, were in reality similar, and essentially dependent upon the same circumstance and process. By the further discovery of points of analogy in certain substances, phenomena, and processes, it became possible to refer new series of these to the same mode of origin, or progress, and thus gradually to enlarge the field of view. With us, on the contrary, and with the objects with which we have to do, all deductions from resemblances and analogies have continued to be a more or less uncertain calculation of probabilities.

In spite of all this, the employment of analogies is an indispensable auxiliary in our investigations, even more so than in other departments of the study of nature, and the difficulty of their application proves nothing against the necessity of their use, any more than a false application of them would militate against a correct one. It is the office of comparison and classification to bring things hitherto widely separated and isolated nearer to each other, and thus to enable us to understand them all more practically. Jenner, for instance, arrived by this method of analogy at his discovery of vaccination. If the foregoing observations have in any way attained their end, they will have pointed out what we are yet in need of, in order to turn this method of investigation to the fullest advantage. Much, however, remains to be explained connectedly hereafter. Our first object must ever be, to compare together such series and kinds of phenomena, such effects, or influences as agree with each other in essential rather than in accidental points. By associating certain functions and processes in the sick, or certain effects of various influences of the external world, or by seeking analogous points in various remedies, or poisons, we sometimes succeed in bringing them into the same point of view, and therewith demonstrating their essential similarity. In our scrutiny of their discovered resemblances, we may meet with points which lead to the further investigation of their causal connection. Thus we have already succeeded in placing side by side the processes in digestion, and in the conversion of substances within the body, with other processes of conversion in organic substances; those of respiration, with other processes of excretion, and with oxidation and combustion generally; and the employment of analogies has already been successful in the accomplishment of something similar in our theories of diseases and remedies.

III. Application of the numerical method, or statistics.

This, as is well known, has become a most important aid wherever it has been necessary to establish facts with great precision. So, also, in our department of study; for it is self-evident, that our notion of any event or process will always be more distinct in proportion to the number of cases in which we have observed it and demonstrated its occurrence in a given manner and under given circumstances. From a greater number of cases, or instances, of a certain phenomenon or event (*e. g.*, a disease), we can much better draw a conclusion concerning their probable general behaviour, *i. e.*, concerning all cases of the same kind, than if we had observed that event in a few cases only. In short, a number of correct and careful observations will always possess a value nearly equivalent to that of experience, which is, in fact, but a name for such observations in their full result.

Statistics here deserve our especial consideration, inasmuch as they may become the means of investigating the objects with which we have to deal, in relation to their causal connection and conformity. The question therefore is, whether, and how far,—from the observation of a certain vital phenomenon, or disease, or of an influence of the external world upon man, &c., in as great a number of cases as possible, under certain variations of circumstances, and by the aid of careful comparison with each other of all the cases observed by us,—we can deduce the conditions and laws of such phenomena, *e. g.*, those of disease. We have further to ask how statistics are to be employed for the most perfect attainment of our end,—*i. e.*, the determination of the causal connection of certain events and effects, or of the effects of certain causes and influences, in our department of science. Experience teaches us, indeed, that on the one hand many important results have been attained by this method, but that, on the other, the exact value of it in the question at issue,—*i. e.*, for the discovery of causal connection,—has not always been correctly and impartially estimated.

By the observation of any phenomenon or event in numerous cases, and by the comparison of them with each other, we may, it is true, not only demonstrate their relative frequency, but also—by taking strictly into account all the concomitant circumstances—the definite mode of their simultaneous or consecutive occurrence, *i. e.*, a certain connection, or series, of various circumstances or events in a given number of cases. We have thus shown a certain constancy of their occurrence under given circumstances; and have even been able to determine, by the observation of the occurrence in question under a sufficient variety of circumstances, that this event, or effect, has occurred only under a certain combination of circumstances and influences, or has at least occurred much more frequently under these than under any others. We are now, therefore, justified in assuming a certain causal connection, the dependence of a given event upon certain circumstances and influences, with more probability than its dependence upon others. And we infer such a relation and causal connection with much more right than if we had met with them in a few cases only. It is precisely the large number of cases which approaches to the value of a mathematical demonstration.

If we know, for instance, that under certain circumstances (in a given county, neighbourhood, or town, or amongst persons whose habits of life are similar), a thousand persons have been attacked in a similar manner in a comparatively short time, or that, in a thousand persons suffering from the same disease, certain organs and processes have exhibited certain definite changes, or that, of a thousand persons similarly affected, nine hundred have recovered after a certain mode of treatment, we can assume, with much greater probability, a certain causal connection, than if we had observed the same event as a consequence of the same influence, and under the same circumstances, in a small number of individuals only.

This probability becomes much greater if, in the enumeration of cases, we also compare them with each other, more especially if we duly vary the circumstances. We may have found, for instance, in the above-named examples, that of a thousand persons in other countries, or in different parts of a town, particularly under different and varying social circumstances, much fewer, or more, fell sick and died; that, of a thousand persons similarly affected, and submitted to a certain mode of treatment, a much smaller, or much greater number died than in the other case; or that the average duration of the disease, if left to itself, was thirty days, while under the use of one remedy it was fifteen, of another twenty days. For here, as everywhere, we have better grounds for regarding a certain circumstance, or influence, as the direct or indirect condition of a given event, if we observe the latter to follow that particular circumstance much more frequently than it follows any others; and we may do this the more safely, the more constantly both occur simultaneously or consecutively.

It is certain, that by the mere collection of numerous cases and examples of a similar kind,—under circumstances which admit of our so doing,—we may establish certain highly important facts and empirical laws. Thus, we may determine, for instance, the constant proportion of male and female children born; the average duration of human life, in relation to age, circumstances, &c.; the relative frequency of disease and mortality, in respect to age, climate, or certain internal and external social conditions; the relative frequency of certain derangements in the same disease, and its duration when left to itself, or treated in a certain manner; the beneficial or deleterious influence of certain sanitary conditions,—*e. g.*, of a neighbourhood, or town, or of hospitals and other public institutions,—upon health and the duration of life, &c. It is only by the extended and methodical adoption of this statistical method that we can hope to acquire, in our department, correct numbers, and facts founded upon experience, in the place of general and vague surmises.

On the other hand, we must not overrate, or falsely estimate, as some have done, the scientific value of these numbers, for they are, at best, nothing more than facts founded upon experience, and can, as such, give us no certain indication of their causal connection, *i. e.* no explanation of them. Statistics tell us only that certain relations exist, that a certain number of human beings have been born, have fallen sick, and recovered, or died, but they do not give us the reasons. Our object would be, to demonstrate with approximate certainty by this method, that certain events and circumstances or influences, which we have observed simultaneously, or consecutively in a certain number of instances, did not coincide accidentally, but really modified and produced each other. The collection of the greatest possible number of cases and examples cannot, however, any more than simple observation, do anything for us here in the way of explanation.

If, like Louis and his disciples, we note, in hundreds of persons suffering from the same disease, the anatomical changes in individual organs, or observe, as is now frequently done, the alterations in the composition of the blood, urine, &c., we may learn thereby, it is true, the relative frequency of certain events and effects under certain circumstances, and in certain forms of disease, but nothing concerning the part they may have played therein, or concerning their causal connection. Here, as elsewhere, the mass of circumstances occurring and operating at the same moment is, in reality, so great, that we cannot, by the mere notation of the frequency of a certain event, any more than by the observation of it generally, draw any certain conclusion concerning its conditions and connection, or concerning the part which any individual circumstance may have played in its production.

If we had seen a thousand men seized with typhus after eating asparagus, &c., and recover after taking herb-tea, water, sulphuret of antimony, and the like; or if we had observed that a greater number of children were born at the change, than at the full of the moon, we should not infer therefrom any causal connection between those events and circumstances. Such a conclusion would be rendered absurd, not only by the known inadequacy of such agencies to produce such effects, but by our consciousness of the operation of other influences far more numerous and important.

If we learn, on the contrary, that only certain persons, who had been exposed to hunger and misery, were seized with typhus, or that in certain patients, after the administration of opium, pain or sleeplessness had ceased, we should be scarcely less disposed to infer causal connection than if we saw a local evil disappear by the use of the knife or caustics. A single case is also frequently sufficient, especially if we have reason to believe the efficiency of those influences from our previous experience and general knowledge of the subject.

Hence it follows that high numbers, taken alone, can by no means conclusively establish a certain causal connection in the things we may have observed. For, in the last-mentioned instances, the observation of a thousand cases would teach us nothing further concerning the actual mode of operation of those influences, than could a constant relation between a given event, or result, and a given circumstance—*i. e.* a causal connection—be demonstrated by the greatest possible number of solitary instances. Our inference of such a connection cannot be implicitly trusted until it has been confirmed by further experience, and by proofs of an entirely different nature.

That many naturalists and physicians should have attached so much value to numbers in themselves may be regarded as a complete misapprehension of the value of statistics; and as a kindred error must we consider their belief that the causal connection and conformity of certain things could be established by the mere observation of their simultaneous or consecutive occurrence, however great the number of cases might be. Thus Boudin, for example, does not hesitate, on the strength of certain computations, to bring the motion of the earth about its axis, and about the sun, into a certain relation to the weight and excreting processes of the human body, and even to the varying numbers of births and deaths, of mental diseases and crimes. This example shows clearly why statistical calculations and results, in themselves, are little to be relied upon as proofs of the dependence of a given event upon a given influence, and the less in proportion to the greater complexity of objects and processes, of possible causes, or effects, and to our own imperfect acquaintance with the essential conditions which govern the operation of these processes, effects, and influences. For it is precisely such a study as ours which leaves the greatest room for errors and arbitrary modes of observation, and still more are we open to these in our estimation of the value of the things which we have observed, and in our conclusions concerning their causal connection. In all our attempts under such complicated and more or less strange, obscure circumstances, we run a risk which statistics, taken singly, can do little to diminish.

Statistics are, indeed, an important auxiliary in our search after experimental truth, but in determining their precise value we must bear in mind the rules which we allowed to hold good in our observation of natural occurrences. For here, as in other methods of investigation, the mode of their employment must be adapted to the nature of the objects, or questions to be investigated; and it will depend upon the manner in which we apply them, and upon the arrangement of the questions which we wish to answer by their aid, whether they further comprehension in our department of science, or merely bring us to that goal to which statistical numbers, taken singly, can so easily attain, *viz.* the jumping, by their help, to the desired conclusion, no matter in how arbitrary a fashion.

The first requirement here is, that the events and cases collected be of the same nature. To admit of any comparison or classification, they must be similar in their main points and in all the circumstances essential to the question at issue, or to the event to be explained. But this is seldom the case in our computations, *e. g.*, in most diseases (with the exception, perhaps of many acute and epidemic diseases, and still more of the chemico-anatomical changes which we can see and appreciate), any more than in the circumstances of their origin or cure. But if the objects and individual cases which we collect are not clearly defined, and if they are susceptible of more than one mode of interpretation, (*e. g.*, dropsy, gout, rheumatism, or even scrofula, pulmonary consumption, &c.), things may be brought under the same denominator, and classified as being essentially of the same kind, which have a very unequal value. We are hence in danger, on the strength of our statistics, of making assertions concerning them all which do not apply to all in an equal degree. Thus Louis has taken blood, almost indiscriminately, from many hundreds of sick persons, and inferred, from the results thus obtained, the inefficiency of bleeding in general.

In the questions with which we have to do, one chief disadvantage lies in the great complexity of circumstances, and in our comparative ignorance of the actual conditions and causal connection of the phenomena and processes under observation. Until we become better acqainted with the conditions of an individual process, and with the effects of each individual influence, we cannot say, in all the instances of any event observed by us, how far it may have depended upon any particular circumstance which preceded or accompanied it. We shall not be able to form clearer notions from the collection of many individual cases, and, however often we may find these in company, we cannot determine whether this may be any other than an accidental coincidence. To make the numerical method really useful in our investigations, our computations must not refer to the aggregate of such intricate complications of phenomena, causes, and effects, as are connected with the duration of life, with mortality, or with diseases and their origin, or cure under complex circumstances, whose separate influence at the moment of observation is unknown to us, and which we are only then attempting to determine. It must be our aim to collect cases which are as similar as possible in their nature, (*e. g.*, cases of inflammation of the lungs, or of tubercular disease in persons of the same age, rank, constitution, &c.), and even these under the most simple circumstances possible.. Statistics must enable us to determine the precise influence of any individual agency in the production of a disease, or in a cure, before we can demonstrate anything numerically, concerning the share to be attributed to such agencies in the aggregate (*e. g.*, the seasons, the weather, climate, or situation). And since the effect of an individual influence or circumstance may not only be complicated, but also modified, or even cancelled, by the effects of various others, while other final results, or so-called exceptions and accidental effects are thereby produced, we must, first of all, determine by the aid of statistics, the effects and influences of the more constant and important individual agencies.

It may be regarded as the established result of experience, that scrofula, rachitis, the formation of tubercles, and similar affections, occur most frequently in the children of the poor. Statistical indications of the same circumstance are also not wanting, but our insight into its immediate or remote causes is not furthered by them because they merely give us a certain total result, and do not enlighten us, for instance, as to whether those children fell sick from bad food and air, or from want of care, from hereditary predisposition, from bodily malformations, or a syphilitic taint derived from their parents. All this we may hope to learn with more certainty as statistical comparison becomes more accurate and is better able to isolate some particular influence and determine its action upon any given case. If a practical physician has, in a large number of cases of inflammation of the lungs, employed bleeding, suitable diet, rest, and perhaps also tartarized antimony, nitre, and mercury, and a certain percentage recover, he cannot infer anything from that large number concerning the influence of the bleeding, for instance, any more than from a few cases only. To render our comprehension of the causal connection in such an instance more clear, it would be necessary to exclude, as far as possible, all other influences which affected the recovery or death of the patients, and to determine, as definitely as possible, the part played by each of them. This would be more easily done if but one remedy had been employed, whether it might be bleeding, or tartarized antimony, or diet and rest; and we might thus, upon an examination into the results of these various remedies, and a consideration of the cures which each, taken separately from the others, had effected, arrive at a decision as to which among them most merited approval.

In like manner, we are not much wiser as to either the cause or cure of any given disease by being simply told that in a certain hospital, or in a certain town, so many persons have fallen ill and died of the malady in question; but we come nearer our mark when we begin to compute the cases of disease and death which have been met with in different parts of a town, amongst the different classes of the inhabitants, and the more so if these have been drawn up separately with a reference to age, sex, social station, business, and mode of life.

The influence of the individual momentum, or agent, upon such diseases and deaths would be more clearly ascertained by this method; and, by carefully excluding other influences, we might bring cases of a complicated nature into the category of comparatively uniform and simple ones, or unities.

It is evident from these examples, that, in a study such as ours, comprehension can only be furthered by statistics where they are specially employed to test the simplest possible cases of a similar kind, in the smallest possible field, and the same kind of effects or influences, under the greatest possible variety of circumstances, and, finally, to collect and compare with each other, separately, the results obtained on each occasion.

Applied in this manner, statistics will help us to obtain more exact and safe demonstration in our scientific medical investigations, and thus assist us in tracing our phenomena and processes to a causal conformity. As in all other sciences, we should deduce from statistical computations this causal connection and conformity with the more certainty, just as, by the aid of statistical investigation in detail, we became more conversant with the office and relative importance of individual agencies, and with the conditions of individual effects. And precisely because the latter has so seldom been carried out adequately in questions of ætiology and therapeutics, and in the general departments of physiology, anthropology, and hygiène, statistics have hitherto been far from effecting what they can and must yet effect.

So long, however, as we remain but partially acquainted with the actual causal connection and laws of our processes and influences, statistics give us, at least, the best possible substitute,—viz., the established frequency of the coincidence of certain effects with certain influences or circumstances; for the more frequently such coincidences occur simultaneously, or consecutively, the sooner will their merely accidental character be excluded. And if, in a study like ours, we can, for the numerous reasons already mentioned, draw no conclusion concerning their causal connection itself, it is still very important to know that certain events have occurred a certain number of times, under certain established circumstances. We have thus, at least, acquired another fact, another law, based upon experience.

The mean numerical values of statistics are important, not only for our provisional comprehension and for future investigation, but also for our art and practice. Up to the present time, for instance, many remedies have the reputation of preventing or curing disease; but, if it were proved statistically, that, amongst persons similarly affected, an equal per centage recovered as quickly, or certainly, whether a given drug had been administered or not, such proof would put us in possession of a useful fact. Many medicines and processes have been said, for instance, to be capable of preventing the outbreak of hydrophobia in persons bitten by mad dogs; and it is certain that, after the employment of such means, the disease has in many cases failed to appear. By the exact observation of hundreds of dogs which had been inoculated with the poison, or bitten by others which were mad, it has, however, been ascertained, that even under the circumstances most favorable for the production of the disease,—*i. e.*, where no means, or so-called preservatives of any kind had been employed, scarcely 60 to 70 per cent. of the animals became rabid. This fact has necessarily gone far to lessen the positive value of such remedial means.

SECTION IV.

CHARACTERISTICS AND VALUE OF THE KNOWLEDGE WHICH MAY BE ACQUIRED BY THE OBSERVATION OF NATURAL OBJECTS, AND BY IMMEDIATE DEDUCTION FROM THEM. EXPERIENCE, EMPIRICAL LAWS, AND THEIR SCIENTIFIC IMPORTANCE.

The last section explained the method after which, in the department of medicine, we are accustomed to observe and investigate phenomena as they present themselves to us; and to draw from what we have observed, certain conclusions concerning their causal connection and conformity to fixed laws. Our next step is, to ascertain more exactly, how far, by the aid of experience, the inductive process is capable of bringing us to sound conclusions concerning the things submitted to our investigation.

The several processes and means peculiar to that method of investigation, have hitherto been employed more than any others, in science in general, and almost exclusively, in medicine proper. The present question is very nearly synonymous with the other, viz., what do we as yet know on the general subject of life—whether healthy, or diseased—of its causal connection, and of the laws by which its course is regulated?

It is not necessary here to prove that our knowledge is, after all, imperfect and uncertain, and to explain why it is so, for this has already been done. What we do know with certainty is, that so many thousand forms of living beings exist, with certain conformations, and with certain combinations of their integral parts; that many of them develop themselves after a certain manner, and exhibit during their lives certain phenomena, or functions and changes, which have all, no doubt, their definite causes, and occur in accordance with their own fixed laws; but we do not know these their conditions and laws, *i. e.*, we are unable to point out the peculiar internal connection and subserviency of the phenomena and processes in the living body, either in a state of health, or of disease. We know tolerably well, it is true, what occurs in many of them (*e. g.*, in the more simple organic phenomena of sensation, and of the contraction of muscles, in digestion, in the circulation of the humours, in absorption, and in various processes of excretion). In many cases we are familiar with the results of certain processes and laws, but neither with the laws themselves, nor with the internal and necessary mechanism of the processes. At best, we have only learnt to conjecture their nature with more or less probability.

It follows from this that we cannot, by the observation of those phenomena and processes, as they actually present themselves to us in the aggregate, nor by the aid of induction, in its more limited signification, attain a clear insight into their causal connection, or conformity. All that we have thus discovered, or can hope to discover under the most favorable circumstances is, that certain things (*e. g.*, certain phenomena and processes in the performance of a function, or in the progress of a disease) constantly occur simultaneously, or consecutively, and have been always observed under constant, definite circumstances, when certain internal and external influences are found in co-operation. We are unable, however, to say with certainty, whether certain processes (*e. g.*, those attendant upon a function, a disease, or a cure) are really dependent upon certain influences, or circumstances, and in how far they are so, and what is their peculiar mechanism. Our comprehension, or positive knowledge, in no case exceeds what we have been able to observe directly, and this observation has seldom gone to any considerable depth below the surface.

We now know, with tolerable certainty, that in respiration carbonic acid and watery vapour are excreted; that heat is generated, and that the character of the blood is changed in such a manner as may enable it to perform its office in relation to the processes going on in the body. In like manner, we know that in persons suffering from pneumonia, the lungs, the blood and circulation, the generation of heat, certain excretory processes, &c., exhibit changes, which occur in a definite and constant order. In neither case, however, do we know what is the link which connects these phenomena and processes together, or unites them with others; neither do we know what are their conditions and adequate causes. If, in any given disease, certain chemical changes of the blood, or urine, or certain anatomical changes of various organs (*e. g.*, hepatization of the lungs in pneumonia, certain heart affections in the asthmatic, enlargement of the spleen in ague, ulcerations of the intestines in typhus, &c., or sugar in diabetic urine, albumen in dropsy, buffing of the blood in inflammatory diseases, &c.), constantly occurred, even then, the mere fact of their occurrence would teach us nothing concerning their relations to the whole disease. But, in reality, they are *not* constant, and do *not* always present themselves in the same form, especially in the earlier periods of the disease. They are, moreover, frequently present when other phenomena, indicative of the disease, are absent, or the latter present themselves without the occurrence of the former. They cannot, therefore, be regarded as standing to each other in the relation of cause and effect, but must both be considered as the common effect of a third agency, yet unknown to us.

On the other hand, it certainly often appears as if some individual organ or function had lost its normal character, and the disease so far seems to be a local affection. If this were really the primary condition, and if certain changes, or derangements of other organs and functions constantly followed in a fixed order, they might perhaps, under certain circumstances, be regarded as dependent upon those which had preceded them. We might then draw an approximate conclusion concerning the part played by any individual organ, or apparatus, or concerning the office of such an individual function, and its signification in relation to others. Every physician knows the influence upon medicine exercised by this view; *e. g.*, in the so-called anatomical theory, or doctrine of lesions; and how local, and especially visible and sensible changes became the starting-point for all attempts to interpret and explain diseases in their general character. But in fact, we never see a disease in its first commencement, except perhaps in cases where we have ourselves produced it; and we cannot know all that occurs in it, or why it occurs. It is therefore impossible even to form an opinion whether all the phenomena which we observe have started from any given point, or to determine what that point is; *e. g.*, how far an apparently strictly local derangement may be the consequence of other earlier derangements and processes. And if, in the further course of a disease, we observe entirely different processes to occur, and other organs to become deranged, we have no right to conclude, from the observation of such a series of changes, &c., by itself, that the sequence has been real, and not apparent only. Still less are we in a position to conclude which among these is cause, and which effect; *i. e.*, whether any, and which of those derangements are dependent upon others, and whether they, as well as others, do not rather result from those influences which first gave origin to the entire disease.

We are equally in the dark concerning the influence which all these circumstances exert in the production of any disease, except only in the case of direct seizures, *e. g.*, of injuries, or poisons, and of the derangement or disease following immediately upon them. Many of these influences may have been in operation years before the present attack, and may have affected the body and its processes in a certain manner, by virtue of which they may give the disease, just as a previous disease might do, a peculiar character and tendency. The same doubts must present themselves whenever we attempt to judge of the actual influence of remedies from the simple observation of the condition of the patient and the progress of his recovery. We may observe various ailments to diminish, nay, even in a number of cases, to disappear, after the employment of certain remedies, as constantly as we see day follow night, or good weather follow bad, yet without being justified in assuming that a given remedy has been the cause of such improvement, or cure, any more than that night is the cause of day, or bad weather the cause of good. Physicians would be equally justified in drawing from the fact that all their patients affected with pulmonary phthisis, or cancer, had died after the employment of cod-liver oil, or iodine, &c., the inference that these drugs had been the cause of their death. In short, even under the most favorable circumstances, *i. e.*, when we see certain phenomena, or events, and certain influences, or circumstances coincide, or follow each other constantly, or almost constantly—we do not even then know how far this is accidental, or how far they are essentially connected together—whether or not they really stand to each other in the relation of cause and effect.

It is true, that simple experience has, in several instances, placed this beyond doubt. Thus it is established that certain diseases arise, not only after the operation of animal poisons, or of contagions, but also in consequence of this operation (*e. g.*, syphilis, smallpox, hydrophobia); that intermittent fevers develop themselves chiefly in miasmatic neighbourhoods, and by the agency of the miasm which prevails; that bronchocele, or ague, not only disappear after the employment of iodine, or quinine, respectively, but also as the consequence of their action. Even in these cases, however, we do not understand the causal connection of the several events, or the peculiar mechanism by which the effects are produced. In these, as in other phenomena and processes, we know nothing of the peculiar conditions attendant on their origin and progress, or of their connection with each other. We cannot say, for instance, by what exact agency miasmatic, or marshy neighbourhoods produce intermittent fever; or why certain changes, or derangements occur in persons thus affected. Neither can we say why these changes, &c., occur in a certain definite order, nor for what reason, and by what properties and modes of operation quinine cures them. Hence, we seldom, if ever, know with certainty, how far, in such cases, the effects and influences which we see constantly occurring together, or following each other, are, in reality, interdependent or have been caused by each other; in short, we cannot say whether their coincidence has been other than accidental.

We might with more certainty infer their essential connection as cause and effect, if we could, in any given case, demonstrate, not only that the event adduced as an effect (*e. g.*, a disease, or cure), always occurs only with, or after the circumstance adduced as its cause, but also, that this circumstance is never present without the occurrence of its alleged effect. But this we have never been able to demonstrate of any one occurrence or influence throughout our whole department of science. The very fact that we do not know, with respect to any given function of the human body, in what form, and under what circumstances it occurs in other animals, renders this impossible; still less, in the case of any given disease, can we predicate under what circumstances it may arise, or under what form it may appear in different individuals and nations, or what mode of treatment may prove most efficacious in these varying cases. With regard to intermittent fever, for instance, we are led to believe that it does not appear in all seasons of the year, even in miasmatic districts, and that it does appear in one year more than in another. The inhabitants of those districts are not all attacked by intermittent fever, although all may be more or less exposed to malaria; and we cannot deny, on the other hand, that intermittent fever not unfrequently occurs in places where we cannot demonstrate the presence of marsh land, or miasm.

We further find, that a great number of persons suffering from that disease are not cured by quinine, especially in miasmatic neighbourhoods. Many recover without having had recourse to it at all, or after the employment of entirely different remedies; just as we find numerous and entirely different remedies employed for the cure of almost every disease. We must therefore conclude that, in the production, as well as in the cure of intermittent fever, circumstances and influences entirely different from "malaria," or "quinine," play a very important, perhaps the chief part, and that thus neither the one nor the other of these influences can, in itself, be regarded as the adequate cause of the effect attributed to it.

Even if we take it for granted that both always occur together, *i. e.* that a disease (*e. g.* intermittent fever) never occurs without the circumstance assumed as its cause (*e. g.* malaria), or a cure without the employment of a certain remedy (*e. g.* quinine), it would not even then be demonstrated with certainty that these precise circumstances and influences had produced those effects. For many influences and circumstances have, in reality, been here in operation at the same time,—differing, perhaps, entirely from those of whose existence and effects we know anything—in every case influences and agents enough whose possible office, or signification in the production of a given effect (*e. g.* a certain disease, or cure) is equally unknown to us with those of the others. It is, therefore, always possible that, besides the circumstances which we perhaps with due probability assume to be the cause of a certain effect, other and entirely different influences may have had their share in its production. An influence which we have perhaps overlooked, or undervalued, or concerning which we were unable to ascertain anything satisfactory, may have played the chief part. At all events, we are not able to demonstrate that such is not the case.

To obtain a further insight into the mode of operation and influence of an individual circumstance, or agent, our next step is to compare the cases in which the event in question (*e. g.* a disease, or a cure) occurs, and to observe when and under what circumstances it occurs, when constantly, when with certain variations, and when not at all. To determine, for instance, the influence of quinine upon the cure of ague, we may employ it in some cases and not in others, or administer it under the greatest possible variety of circumstances, in patients of different ages, sexes, &c., in larger or smaller doses, or in combination with various dietetic precautions. We may now compare all these cases and series of observations with each other, and observe in what instances and under what circumstances a cure has been effected most quickly and certainly, and where most slowly. Still it must be remembered, that for the reasons assigned above, the importance of any circumstance, or influence, as essential to any event, would by no means be accurately defined. It could only be so, if the given circumstance, or influence had come into operation entirely alone, and if the effect attributed to it, *i. e.* the result in question, were not only to follow it invariably, but also to follow none other than one of this particular kind. We never have, however, in reality, the opportunity of observing the effect of any influence, or agent by itself. Man is never, any more than any other living being, for a moment under the influence of but a few, and still less under that of one only of the agencies of the external world, whether it be *e. g.* the operation of malaria, or of a remedy.

We might hope to attain our end rather by isolating a given influence, and allowing it to operate under an endless variety of circumstances, *i. e.* by experimenting with it. The former mode of proceeding is, however, almost impossible, and the latter, at best, very circuitous. Those influences, for instance, which we perhaps suspect to be the remote, or proximate causes of a disease, or the conditions essential to the performance of a given function, and which we consequently wish to investigate, can seldom be brought to act separately upon man, or even upon animals. With some this is indeed possible to a certain extent, *e. g.* with medicines and poisons, and attempts are frequently made to investigate their so-called physiological effects in this manner. But here we must allow for the difference in their mode of operation upon the healthy man, or animal, and upon the sick, and we may conclude from the observation of their effects in the latter, that the peculiar state of the patient and the spontaneous changes occurring in disease have had far more influence in their production than the operation of the substance itself. Thus, vomiting may follow the administration of ipecacuanha, or tartarized antimony, or of sulphate of zinc, or copper; apoplexy may follow upon exposure to great heat, or cold, or the use of spirits or narcotics, each being produced by a variety of causes, just as the body may be supported and nourished by very various kinds of food. In the former of the two examples, the substances named evidently only give the first impulse, in the latter they only aid those internal processes and operations in the body, which then act, as at other times, according to their own laws and by their own essential mechanism. Thus a building is burnt down in the same manner, and in conformity to the same laws, whether it has been set on fire by lightning, or by combustibles.

To ascertain by experiment the causal connection and conditions of any vital process, or of any disease and its cure, we should be able to bring into operation artificially all the circumstances and influences which we imagine to be causes, or at least important agents in the production of the phenomenon; observing what changes take place in man, or animals, under their operation, and whether they agree or not with the effects, or results (*e. g.* of a disease), as they occur in nature. We should therefore vary those combinations of circumstances and influences, and bring them into operation in different ways, until at last this particular kind of effect (*e. g.* a certain disease, or an individual change, or given state in the disease), occurs. This would evidently be a very tedious process in seeking to ascertain the unknown causes of a given effect, especially amongst such complicated operations and influences as those with which we have to deal.

It seems more than doubtful whether we should attain our object by such means, *i. e.* whether we should finally succeed in ascertaining, by all possible variations of the circumstances, the precise kind and combination of circumstances and influences which have been followed by the effect in question. Thus Bacon doubts whether any one would ever have discovered the composition of gun-powder, if he had taken its effects as the starting-point of his investigations, *e. g.* the fact that rocks are split and bullets thrown long distances by it. It is no less doubtful whether we could ever produce by this method a genuine intermittent fever, and enlargement of the spleen, or an actual typhus, or tubercles in the lungs. An experimenter does not make the possible causes of a given effect the first object of his search, but rather the possible, still unknown effects of a given cause—of some modifying circumstance or influence. It therefore appears at once easier and safer to limit our experiments to the investigation of the possible effects, or modes of operation of those influences and agencies. If we always succeeded in producing by the agencies we employed, results corresponding exactly to those which occur in reality (*e. g.* intermittent fever, typhus), we should then have discovered, with tolerable certainty, the conditions upon which they depend.

If, however, our experiments are to stand the test of rigid exactitude, and if we take into consideration all the requirements they must fulfil before we can draw from them any conclusion concerning the causal connection and the conditions of a disease, it will be evident how great are the uncertainty and comparative inconclusiveness of this mode of investigation. If the chemist and physicist wish to bring similar points in their peculiar department to the test of experiment, they set out, in the present day, with a knowledge of the subject, an understanding of its phenomena and processes, of the individual agencies at work within it, and of the laws which govern their operation, which we, in our branch of science, do not yet possess. They know beforehand upon what substances and compounds they have to act in concert with the agent to be investigated ; they can, moreover, be certain that no other agents are acting upon them at the same time, and, in order to ensure this certainty, it is possible to determine beforehand the effect of any such agents, so as, if necessary, to exclude them from the operation, and in any case to be able to appreciate them at their true value.

The physiologist already understands approximately the normal action of the organs and functions which he intends to put to the test of further experiment. He can, therefore, the better draw conclusions respecting their action and internal connection from changes which he produces in these processes by artificial manipulations, and he is thus the better able to bring the effect observed into connection with its causes.

We, on the contrary, are not only in a great measure still unacquainted with the properties and modes of operation of the complicated influences and agents with which we have to deal (*e. g.*, marsh-land, weather, bad food, &c.); but, moreover, we find them working, not within a circle of processes already known to us, but within the complicated mechanism of the living body—a chain of processes whose conditions and laws of operation are still more or less obscure. This is much the same as if the chemist were to endeavour to ascertain the properties and effects of a substance by experimenting upon a complex mineral which contained, besides the substance in question, many others with which he was not acquainted, and as if, in his experiments, he were to employ impure preparations containing many foreign and unknown substances.

Simple observation will guide us to determine the effects of certain influences and agents with more ease and safety, if we see them constantly produce such intense changes in the living body that we can no longer doubt the dependence of the one upon the other. This is the case with various remedies and poisons, with some forms of contagions, with the effect of heat, or cold, of injuries and surgical operations. In these cases the connection in question can be all the more readily ascertained from the isolated character of the phenomena; and from the fact that their effects appear directly and rapidly, and without being spread over considerable periods of time, as is the case with many other influences and agencies. But the living body acts and reacts in these instances, just as it does in others, in obedience to its constant laws, and the changes observed in it, and in its individual processes and phenomena, have occurred under definite conditions which we do not yet understand, but which we can only produce in a fixed and definite manner. In no experiment upon the living body is it possible to exclude all other influences, especially those which tend to maintain life, and for this reason we can seldom determine in what degree, if at all, the changes produced (*e. g.*, a disease, or a cure) depend upon the agencies, or influences intentionally brought into operation. And even although we were to take it for granted, that a certain disease, or definite changes in certain processes, or functions, always followed in such cases, the mechanism of their production would not thereby be rendered more intelligible to us, because we have not yet arrived at an understanding of the whole mechanism of life,—we do not yet see clearly all the intermediate physiological links which exist between the operation of any given agent and the final result (*e. g.*, an attack of disease, the effect of a remedy, or the progress of recovery).

The complexity of the phenomena and processes going on in the living body is that which necessarily prevents our acquiring a clear comprehension of their essential conditions, and conformity to fixed laws, by simple observation, and by direct experiment;—it is that intimate co-operation of so many processes to produce one result, together with the mass of circumstances and influences which modify it from without—in other words, that dependence of all processes upon every other which we can possibly conceive. Whatever, therefore, presents itself to our observation is only in appearance a homogeneous, simple whole; it is, in reality, rather the final result, or effect of very many processes and influences, whose tendencies frequently come into collision, and whose effects oftentimes cancel each other. The physicist would find it very difficult to discover the conditions and laws of the phenomena, or "forces" which he investigates, from cases in which the tendency of one agency, or force was constantly modified and deranged, or even cancelled by another. As if, for example, he were required to ascertain the laws of motion solely from bodies kept in a state of rest by opposing forces; or if, from cases in which motion results from a combination of forces (*e. g.*, steam, atmospheric pressure, horse-power, &c.), he were required to calculate the products and laws of operation of any one of them separately, *e. g.*, of steam.

If the process of induction is frequently accompanied by great difficulties in the chemico-physical department of study, where an individual effect, or an individual influence can so often be investigated in a state of isolation—if it is often difficult even there to separate the various phenomena and processes, and to determine their conditions as well as their action (*e. g.*, the electric, magnetic, thermal, and chemical phenomena of the same substance, or in the same process)—it becomes a positive impossibility in the study of the phenomena of life. As already mentioned, the mere observation of the aggregates of effects and causes in the living body, and the application of the process of induction, properly so called, are insufficient to enable us to ascertain the conditions of the vital phenomena, and the effects of all the various influences upon them. We may, therefore, after a calm and close examination, abandon all hope of attaining, by this method, to a comprehension of the laws by which the phenomena and processes are regulated, which are submitted to us for investigation. So many circumstances of a modifying, or of a modified and secondary kind, always enter into and complicate the problem to be solved, that we cannot deduce any true view of their causal connection, their fixed order, or the laws by which they are regulated, from the mere observation of vital phenomena and influences, whether in the healthy or in the sick.

This circumstance accounts for the many obstacles and discouragements which beset us in the present state of our knowledge, and especially in our attempts to unravel or to comprehend the objects of our investigation. Thus it is self-evident that whenever an effect, or a definite result may depend upon a variety of circumstances and influences, or upon the co-operation of several of them at the same time, as must ever be the case in medical science, it may happen that some one individual condition, or circumstance amongst them has in any given case a preponderating share in the production of the supposed effect. This may help us to explain:

I. Why, in the living body, in a state of health, or disease, *the same event, or the same effect may occur in very different manners, under varying and dissimilar circumstances*—a fact which greatly complicates the investigation of their causal connection.

Taking, for example, any one of the functions of the living body, we know that in the long chain of the animal and vegetable world, although their organs, so variously constructed and combined, act in various media and under the manifold influences of the external world, yet in all, perhaps, the function occurs essentially in the same manner, or at least with a similar final result, *e. g.* respiration and other processes of excretion, absorption and the circulation of fluids, nutrition and conversion of tissues, &c. In like manner, the human body maintains its integrity although supplied with very various kinds of food, in some cases, perhaps, only with fruits and milk, in others with the most abundant and compound aliments. It has been ascertained by frequent observation, that a disease may occur under very various circumstances, and yet shall be essentially the same in all, and that it affects sometimes one, sometimes another set of processes and organs, and these in a varying connection and order. Experience also teaches that the same disease may run the same course, and disappear with the same rapidity under the most various modes of treatment and remedies, or even in spite of them. Typhus, for instance, occurs sometimes in overfilled hospitals and prisons, sometimes epidemically in times of dearth, or other misery, and sometimes sporadically in individuals, without any evidence of the presence of any of these deleterious agents. We also see paralysis occurring, sometimes as the consequence of some affection of the brain, sometimes as the result of chronic lead-poisoning, and often without any such palpable cause; in the same way as asthmatic attacks may sometimes be traced to defects in the heart, or its valves, yet they frequently occur where no such defects exist.

If functions essentially similar may be performed under very various circumstances; if the same disease may arise with and after the operation of the most various influences, and cease frequently and rapidly under the most various modes of treatment, it follows that the circumstances and influences in question cannot possibly have determined the development of those results, but rather that they have been chiefly brought about by the vital processes, occurring in conformity to their own laws. It is an error, therefore, though one of daily occurrence, to regard any individual circumstance, or influence, as the cause, or even as the essential condition of such an event, although it may have been observed by different persons to precede it, and even, apparently, to aid in its production. For we have shown that the same result, the same final effect, may be produced under the operation of widely differing circumstances. In the imperfect state of medical knowledge, we must allow largely for merely accidental coincidences, and as such we may safely, in a great number of cases, set down many of the so-called causes of and remedies for diseases.

It is further evident how much this occurrence of the same event, under such various circumstances and influences, must impede the discovery of its causal connection and peculiar conditions. No single investigator can well hope, from his observations of physiological processes, or of the changes produced by disease in any number of persons, to arrive at any positive conclusion regarding the causal connection of even one of all the functions and diseases. He may establish facts, and important ones, but he will not easily explain them, because he cannot ascertain, experimentally, all the processes associated with the function he is examining, and all the conditions under which it may possibly go on, nor will he have the opportunity of observing the origin and termination of the disease, and the progress of its cure, under circumstances sufficiently varied to enable him to draw a conclusion even approximately correct regarding its true causes, and the laws to which it is subject. The field and range of his experience are too confined, and the special manner in which he interprets and frames his deductions from what he has observed is not sufficiently comprehensive and correct, to enable him to advance general truths on this important subject. It can only be accomplished by the combined exertions of numerous observers and investigators.

II. The fact that circumstances and influences so varied co-operate in the development of the processes or phenomena in question, will explain the further fact, "*that in the living body no occurrence or effect ever takes place with perfect constancy and certainty, and always in the same manner; and that we cannot, for this reason, with perfect certainty predict or expect any occurrence or effect whatever.*"

For supposing any one of those numerous influences and circumstances not to have come into its ordinary operation, to have been in some degree modified, or to have been controlled by some foreign influence, the result or final effect cannot fail to be different from what it would otherwise have been. This will the more certainly be the case the more important the part which the agent in question has played in any such process; in other words, the more the development of a process, or of a disease, has depended upon it. The comparatively weaker, and more dependent influence of other conditions and circumstances will be so far modified and interrupted, perhaps in some cases altogether cancelled, in consequence of some such change having occurred, that the ordinary effect may either not be produced at all, or not in the usual manner. The same thing may happen in consequence of the accession of some additional influence, or circumstance by which the operation of others has been in an unusual manner strengthened, weakened, interrupted, or even altogether neutralized. And this shows us how, among the mutable conditions of life, and the varying circumstances which affect it, sometimes one, sometimes another of its processes, will exhibit the most manifold variations and changes. Hence it follows that the results which we are able to obtain by the observation of the vital processes, &c., oscillate within very wide limits, and that from the causes already so frequently alluded to, they do this to a much greater extent in disease than in health.

Thus, one person is attacked by typhus or inflammation of the lungs, under circumstances in which another is attacked by diarrhœa or a cold in the head, while a hundred others escape altogether. While in one individual a disease assumes a mild form and runs a particular course, in others, the same disease, appearing at the same time, may be very severe and run a course entirely different. In one dropsical patient the urine contains a large quantity of albumen, in another, not a trace of it; in one country fever is accompanied by extensive lesions of certain tissues, *e. g.*, ulceration of the intestines, while in another this is not the case. One of the most constant events recorded in medicine is the fact that intermittent fever occurs through the operation of some poison generated in miasmatic neighbourhoods, and that it is cured by quinine. We formerly alluded to this fact as an instance of the highest degree of certainty which can be attained by simple experience in medicine. But in reality, men are attacked by intermittent fever in situations where, besides marsh-miasmata and the like, many other deleterious influences are in operation; not only those of climate and weather, but also unhealthy dwellings, want of cleanliness, various kinds of exhalations, imperfect tillage, insufficient food, &c. We know further that after the removal of these influences, the disease has much diminished in frequency and severity among the inhabitants: we know, also, that in the same neighbourhood and place, all the inhabitants are not attacked to the same extent, but that the disease is in great measure confined to the lower and ill-fed classes, to people of dissipated habits, to the inhabitants of unhealthy and thickly-populated places, and to badly managed hospitals, barracks, &c. We must therefore regard it as highly probable that marsh-miasmata, taken alone, are not the adequate cause of intermittent fever.

We may thus the better understand why the neighbourhoods in which intermittent fever occurs endemically (such as Rome and its Campagna), are not by any means invariably situated in the vicinity of marshes; and we may also learn why intermittent fevers are not most frequent and severe just where the greatest extent of marsh land exists; and, finally, why, in the neighbourhoods rarely visited by these fevers, we are so little able to trace their exemption to any common vantage ground in the way of situation.

To take another example, most of the persons suffering from intermittent fever (those, for instance, who are treated in our hospitals) are not only put under the influence of quinine, but also of better food and care, and enjoy a comparatively more healthy existence than they had done previously. This also partly explains why, among the persons cured, some have recovered under quinine, and some without its having been employed at all.

To this we must add the generally acknowledged impossibility of concluding with certainty, that because a given person attacked in a certain manner under certain circumstances, has recovered under a certain mode of treatment, the same thing will invariably occur in other individuals. And we cannot, from the physiological effects of any substance, *e. g.*, of common salt, fatty matters, salts of lime, &c., even of one with which we are best acquainted, draw the conclusion that it would be of use in a given disease, *e. g.*, in indigestion, tubercular deposits, disease of bone, &c. For it is impossible to determine *a priori*, in what degree such substances will produce similar effects to those in health, if they are capable of doing so at all under circumstances so entirely new and, as yet, so obscure in their causal connection and conditions. We can only infer their use in such diseases with probability, when it has been confirmed by special clinical experience.

In short, in medical science, every one must feel it to be impossible to predict anything whatever with certainty, so long as we remain unacquainted with all the conditions of a given event, and ignorant as to how these conditions or influences may act in any particular instance. It is only in cases of direct injuries and of certain physico-chemical agencies and absolutely indispensable vital conditions, that we can pretty certainly predict the consequences of their operation on the one hand, or of the interruption of it on the other. But we have said enough to show how the continual fluctuations and variations affecting the phenomena, effects, and influences under discussion must impede that knowledge of their causal connection which observation and experiment would otherwise unfold.

III. With this acknowledgment is intimately connected the circumstance *that such frequent allusion is made to accident and exceptions, to unexpected effects and events.*

Even the physiologist has frequent occasion to do so; the physician is compelled to admit it at every step. Thus a disease or an epidemic frequently breaks out under circumstances in which we had not anticipated the probability of its development, or is absent under circumstances where our previous experience would have led us to expect it. In an epidemic of typhus, dysentery, &c., many persons are attacked whom we had regarded as more or less likely to escape, while others, apparently under exactly the same circumstances, with the same habits, or who seemed to be particularly exposed and threatened, are not attacked at all. A disease may at any time run a course, and events and complications may occur in it, or effects of remedies be seen in it which the most experienced and circumspect physician had never thought probable. Remedies and modes of treatment under which, perhaps, thousands have recovered, prove inefficient in a particular patient without our being able to give even a remote explanation of the cause.

And yet all these events which, in common language, we call *accidents,* must, under the given circumstances, have occurred as much by way of necessary consequence as any other event or effect whatever, and that from the co-operation of various influences and conditions. We are accustomed to designate as "accidental," every event and influence occurring under circumstances in which we had not expected it. There are cases of daily occurrence in which the circumstances and conditions that would otherwise have sufficed for the production or prevention of a given event (*e. g.*, a disease or a cure), have been modified, interrupted, or even altogether neutralized in their action by the accession and operation of others. But this has happened unexpectedly because we have not been sufficiently acquainted either with all the conditions necessary to it, or with all the influences and agencies which have co-operated to produce it, and have thus failed to take into account many things for which we should have seen the necessity of making allowance, in a more advanced state of knowledge. In other words, such an event happened unexpectedly, and is now regarded as "accidental," because the connection and laws of operation of its causes were more or less unknown to us. We have possessed, perhaps, a certain experience respecting it, inasmuch as we had observed it to occur a certain number of times, together with, or after certain influences and circumstances, and yet we may not have recognized its causal connection with, and actual dependence upon them. The circumstances under which we had observed any event (*e. g.*, an attack of disease, a fracture, the operation of a remedy), as well as our knowledge of the influences which co-operated to produce it, have been of such a kind that we could not deduce from them a certainty of its occurrence under the same circumstances, or conversely of its non-occurrence under certain others. We are, perhaps, aware that some of the latter are likely to have a material influence upon the effect in question, but are so far ignorant of its general conditions as to be unable to judge which among these many circumstances and influences has really, in any given case, produced it; or to predicate how far they would operate in another case. It is rather to be concluded then, that this event or effect stands in no causal connection with those influences and circumstances under which we observed it to occur; it may either accompany or follow them, or fail to occur at all.

It is just this kind of simultaneous and consecutive occurrence of certain circumstances and processes which we call accidental, *i. e.* when they simply accompany, or follow each other, without being, in reality, mutually dependent, or standing to each other in the relation of cause and effect. Their mutual dependence and causal connection is indeed possible, and in many cases, even highly probable, but we ought nevertheless to bear in mind the possibility of their occurrence being merely accidental, simultaneous, or consecutive, so long as we have not established their dependence upon each other, *i. e.* their causal connection, and cannot, until this is accomplished, consider the event in question as an instance of such a law. Such a conformity, such a causal connection has, however, scarcely anywhere been established in medical phenomena and processes. Up to the present time any real knowledge of them is almost entirely wanting, and it is just on this account that we so often speak of accidental occurrences.

In former ages men regarded almost everything as accidental, because, so long as they were unacquainted with the conformity of natural phenomena to fixed laws, they were accustomed to refer to fate, or necessity, all that we now regard as resulting from the laws of nature. To physicists and chemists many things formerly appeared accidental which they no longer regard as such, now that they have become better acquainted with the causal connection and laws of the effects and processes observed in their departments of science. But in the living body, as elsewhere in nature, everything occurs in accordance with laws, *i. e.* is the effect of fixed causes. Deviations from natural laws, or events opposed to them, can nowhere exist, they are either only antagonistic effects, or modifications resulting from the intervention of other influences. It is therefore an error to say that any event or effect whatever has arisen accidentally. All that we can say is, that certain circumstances and events have occurred at the same time, or followed each other, without our having known their causal connection, and, therefore, in the absence of any known probability, contrary to our expectations. If, however, we were familiar with all the laws and conditions to which those effects and counter-effects are subject, we should be able to predict what would occur under every given circumstance, and our calculations would exclude the very idea of accident.

It follows from this that we ought to be careful how we speak of "exceptions," simply because there are no exceptions to natural laws; for in the living body, as elsewhere, no effects, or processes can occur otherwise than as the consequences of their fixed causes, and in their definite order and dependence. We speak of exceptions more particularly in those departments of human knowledge in which we have hitherto acquired least insight into this subordination, and have therefore not been able to establish any general laws, or safe rules for practice. What we have already said respecting accident, will explain why, in medical science, so-called "exceptions" so much abound.

All that simple observation and experience can determine, and nearly all that has yet been established in medical science, amounts to this—that certain things occur (*e. g.* in a vital process), in a definite manner and order. Our whole empirical knowledge consists, in other words, in the fact that phenomena observed by us in a function, or a disease, &c., very frequently, or perhaps constantly accompany, or follow each other under certain circumstances. We may, indeed, be convinced, that those phenomena, &c. occur in conformity to fixed laws, and that all the circumstances and processes which affect them from within and from without, have acted in their definite order and manner, but we have not ascertained those laws in their actuality. We may, it is true, form certain hypotheses concerning them; we may even, in the case of individual processes and influences, be able to predicate their probable existence; but we can in no case clearly demonstrate their causal connection.

We give the name "empirical law" to this simple fact, learnt from experience, that certain phenomena and circumstances, certain effects and influences occur together without our being able to point out clearly their actual mutual dependence, or their general causal relation to each other. This means, therefore, nothing more or less than that, so far as we have observed, the simultaneous, or consecutive occurrence of any series of phenomena has invariably happened in the same manner; it conveys no information whether, and to what extent, any one of them is the precise effect of any other. We cannot even be sure that we have surmised or recognized all the circumstances and influences upon which a given event, &c. may depend. We have no guarantee that the only possible mode and order of their occurrence, is that which we have observed, or that some other, perhaps an entirely opposite one, might not have been equally possible. We are, finally, unable to determine what circumstances, or influences are necessary to produce the process, or result in question (*e. g.* a function, a disease, or a cure). At best we can only say, that in all the cases, or instances in which it has been observed, or in which a comparison has been made with reference to it, certain circumstances, phenomena, or events have accompanied or followed each other. But since we do not know with certainty their connection and mutual dependence, we cannot deduce from this observation what would occur in other cases—*e. g.* on the accession of other circumstances not previously present—until we have observed such cases with equal care, and have determined with equal precision in them, each simultaneous or consecutive occurrence. Hence we cannot safely rely upon all that simple experience has led us to regard as fact, or upon what are called empirical laws and axioms. They furnish no conclusion concerning the whole, because they have not made us acquainted with this whole; for no one can be certain that the circumstances which have been observed by himself or by others to occur simultaneously, or consecutively, will invariably be present, and will always bear the same relation to each other. For this reason empirical rules hold good only for the place and time at which the event or the phenomenon in question has been observed, and only for the particular combination or kind of circumstances, or influences under which our experience of it has been acquired. Thus it would be asserting much more than we can do with safety, to say that a disease (*e. g.* dysentery, typhus, intermittent fever) will appear again under circumstances similar to those which we had observed, in previous epidemics, to precede or accompany its outbreak, whensoever these should arise, or that because in a given place, or at a given time, a certain per centage of the population, or of a regiment of soldiers, &c., was attacked by a certain disease and died, the same thing would happen in the following year, or in other places of a similar character. Again, although a certain mode of treatment, or remedy, may have been successful in ninety-nine cases of ague, or syphilis, we cannot therefore conclude with certainty that they would succeed in the hundredth case, notwithstanding the exact resemblance it appears to our best judgment to bear to the others. In short, every physician knows from experience how little he can rely upon any empirical rules. Every new case, every contradictory experience, is enough to show their uncertainty, and in some instances to overthrow them altogether, and that, just in proportion to their generality and comprehensiveness. And we cannot, from any empirical law, draw conclusions with respect to an individual case, any more than with respect to all cases of the same nature, because that precise case may contradict the law and belong to the so-called exceptions.

The necessarily limited range of individual experience is enough to explain why so many errors and illusions exist in a science like ours. They will always occur the more readily, just as the things observed are the more vague and fluctuating in their nature, and our experience concerning them the less to be depended upon, in consequence; and just as the deductions which an individual has attempted to make from his experience, are the more comprehensive; *e. g.* in relation to the constancy and causal connection of an event. It has been shown that mere experience, as such, does not enable us to draw correct conclusions; our interpretation of the things observed, must remain more or less uncertain and arbitrary, our surmise of a connection between them, a "mera palpatio." A person, for instance, who had observed chlorosis only in young girls, ill fed and of sedentary habits, would form a different opinion of its conditions and nature, from one who had seen it rapidly developed as a consequence of fright, sorrow, or love. The former would be led to seek its origin in some change in the blood, or in some fault of nutrition; the latter, in a peculiar state of the mind, or of innervation, &c.; and while the former would endeavour to affect the composition of the blood, the conversion of tissues, or the process of nutrition, the latter would direct his remedies against derangements of the nervous system, &c. Both, however, would be equally wrong in attempting to draw from their respective experiences, however correct in themselves, any general conclusions concerning the state of chlorotic subjects, especially in relation to the origin of the disease.

Of all purely empirical axioms and propositions it is true, that even under the most favorable circumstances, *i. e.* where such conclusions are valid for all the cases observed (*e. g.* in a given place, or at a given time), they may prove, in other cases, altogether false, or at least one-sided. If, for instance, any one had observed during the last decennia, that in thousands of cases of fever in Germany, or France, the well-known changes in the alimentary canal, and in the mesenteric glands, were constant, and if he knew, perhaps, that all the other physicians of those countries had made the same observation, he might be justified in concluding that these are constant, and even essential occurrences, in that disease. And yet he would certainly be wrong if he attempted, from this, to form a general law, and to say that such changes, or lesions, occur in all cases of fever, and are essential to the nature of the disease. For in Ireland, Livonia, and many tropical countries, they seldom if ever occur in connection with it, and the same observer would, perhaps, fail to meet with them in the epidemics of fever which occur in campaigns, or if he were to see the disease under other circumstances, at other times, and in other places. If again in every case of ague which we had observed, certain and rapid recovery followed any particular remedy, *e. g.* quinine, the conclusion that this was a certain cure for the disease, would be so far perfectly correct. But in true miasmatic and aguish neighbourhoods, especially in hot countries, the fallacy of such a conclusion, valid as it is for certain circumstances and persons, or for a certain place, &c., would soon become evident, and we should find that we had established only a simple empirical law, not a positive and general one.

If what has been already said shows the uncertainty of empirical formulæ, and the impossibility of founding upon them, as such, an explanation of the phenomena and processes connected with medicine, it would, on the other hand, be far from reasonable to declare their importance in this science to be but slight and dubious. This would be tantamount to distrusting all human knowledge and capability, and to slighting all the acquisitions hitherto made. Although we have found ourselves unable, by the method of simple observation and experience, to ascertain any positive laws, or the causal connection and the conditions essential to the development of these phenomena and processes, we have nevertheless reached a point much nearer to this knowledge than would have been possible without the aid of experience, &c. In point of actual knowledge, even a Celsus or Hippocrates, a Boerhaave or a Sydenham, would be a smatterer in comparison with any practical physician or physiologist of the present day. And, although we have, as yet, arrived at no certain or very correct comprehension of the conditions essential to the development of the processes to be investigated, as well in the living body as in the external world, or of their conformity to fixed laws, we can still, in every case, surmise them with much greater probability of being correct than our predecessors. It is true that we can never say, from mere experience, upon what a given effect, or event, *e.g.*, a vital process, an attack of disease, or a recovery, really depends; nor can we, for this very reason, be certain that the same event will inevitably happen in any given case; neither can we, by the aid of experience, propose any plan of treatment with full certainty of success. With sufficient experience and circumspection, however, reasons enough will almost always exist, to render the occurrence of one event much more probable than that of another. Little, therefore, as we can rely upon mere empirical laws, they are, nevertheless, as such, of the highest importance.

This is especially true with reference to the whole mode of proceeding in the practical purposes of science; not only because such empirical knowledge is the sole knowledge which we possess, but also because it affords us what may be the most weighty reasons—the firmest basis for practice. We have not, it is true, learnt from it why such a comparatively large number of persons fall sick and die, in various ways, in what are called miasmatic districts, in miserable and dirty parts of towns, in over-filled hospitals, &c. But we could no doubt save the health and lives of thousands, if we could defend them from all those influences which our experience has pointed out. No one knows exactly what smallpox is, or in what the preservative power of vaccination consists, yet millions have already been saved by this precaution. We cannot say why mercury does more good in syphilis than so many other remedies; nor can we be certain that a given patient will be cured by it, yet, we are fully justified in employing it in his case, as soon as it has been ascertained that in such patients, under similar circumstances, it almost always effects a cure. Experience, indeed, teaches us that it frequently does no good, but rather harm, and that this may possibly be the case with the patient in question. But, on the other hand, a survey and scrutiny of all the particular circumstances of the case, may show that it differs in these respects from those in which mercury was followed by bad effects, and that we may here anticipate good results with great probability. In the same manner, the skilful practitioner always knows how to apply his experience of the effects and uses of his remedies in the most serviceable manner, even when he has no exact knowledge of their nature. Moreover, he would not be justified in waiting until such a knowledge, tardily perhaps, followed his empirical mode of proceeding. The patient in question, requires our aid, and we must decide for, or against the employment of mercury, and upon the manner in which to employ it. If we possess sufficient experience, knowledge of the subject, and practical tact, we shall no doubt be able to do all for that patient which circumstances permit of.

Experience has long taught the seaman that in certain states of the atmosphere and of the surface of the sea, when the wind blows from a certain quarter, when the sea-gulls approach the vessel, and the fishes swim in a particular manner, a storm is at hand; he is thence enabled to adopt precautionary measures. He is perhaps as ignorant as is the physician in his science of the exact connection between these phenomena and the conditions and influences in operation, and knows nothing of the laws by which such effects may have been produced. But even if we were acquainted with all these circumstances, our practice would scarcely be much facilitated, for they can never be so simple as those of a mechanical apparatus, or of a physical or chemical process; on the contrary, they must, in all cases, be extremely complex and modified by many particular circumstances. We should then, in any particular instance, have to weigh manifold influences and circumstances, and to make allowance for their reciprocal effects; and we should have to determine, by detailed investigations, all the states and changes occurring in the patient. Such a proceeding, however, would generally be impossible in practice, just as it would be impossible for the seaman, in the short time at his disposal, to make exact barometrical thermometrical, or eudiometrical observations, or even to refer to his tables on the subject.

What the practitioner especially needs, is the capability of ascertaining the chief points for decision, and of choosing what may best further his object in the case before him. It is this rapid and correct *coup d'œil* which is the most essential element in what we call "practical tact;" and this is not acquired by mere knowledge,—it is not to be learnt in this way. Scientific and erudite men are often least fitted to acquire it, partly, perhaps, because they are not, when practising medicine, in a position to form an opinion, or make a choice, from a clear comprehension, and upon adequate grounds, such as their habits and inclination, acquired in scientific investigation, would lead them to demand. In practice, every one must make the most of the knowledge he possesses, and strive to turn it to the best account; and, since there will always be gaps enough in purely empirical knowledge, he instinctively regulates his proceedings as if such gaps did not exist. The simple experiences which generally serve as his guides are of scarcely less value to him, even in a scientific point of view. The correctness, not only of his practice, but also of his knowledge, increases with their extent; for the greater the number of cases which we have observed and compared with each other, and the more varied the circumstances under which we have observed a given event to occur, the more correct will be the conclusions and empirical rules which we can deduce from them, and the more extensive will be their application. They will go far, too, to establish the probable correctness of our inferences respecting certain more intimate, though, perhaps, not precisely causal relations which we have assumed to exist between certain effects and influences.

We cannot, indeed—from the fact that we have observed a function, a disease, or a recovery, to occur under certain circumstances and influences—infer that the latter were the essential conditions of the former. That some connection exists between them appears the more probable the oftener we have observed the same event under the same circumstances, when it has not been limited to a given place and a particular time. If it has been established, for instance, that the presence of oxygen is indispensable for the performance of the function of respiration in all animals possessing a respiratory apparatus, the dependence of that function upon oxygen,—*i. e.*, the important part which it plays therein,—becomes highly probable. If experience has shown that intermittent fever predominates in marshy districts, not only in the northern, but also in the southern, hemisphere, or that it generally ceases after the employment of quinine in both, these facts may more safely be regarded as the conditions of those events than if the same observation had been made in a small number of places, and in a few cases only. In the same manner, the scientific value, and the correctness, of our empirical rules will be increased by all those auxiliary methods which were mentioned in connection with the process of induction.

Since that which experience teaches us always furnishes the starting-point for our further investigations, and the first, though imperfect materials for a better comprehension of the phenomena we are studying, it must be regarded as an indispensable auxiliary in true science. All our experiences, in themselves, give us, it is true, no knowledge of these phenomena, but they prepare the way for what they cannot give. From the rough masses of information which experience affords, some fortunate investigator, some speculative mind obtains more scientific and available materials.

Our surmise of a causal connection between certain processes is confirmed by the circumstance that further facts and knowledge of one kind or other come to our aid; that we learn something more concerning those processes, or phenomena, than their mere occurrence under certain circumstances. This is more especially the case where we know something of the properties and mode of operation of a given influence, or of the manner in which the event in question occurs somewhere else, even though it may occur under entirely different circumstances. Thus we can better infer the frequent and serious attacks of disease, and the great mortality amongst the poorer classes to be due to their bad and insufficient food, and their general mode of life, than to want of proper clothing or cleanliness; for we know, from other sources, that the former play an infinitely more important part in the maintenance of health than the latter. In like manner, when patients recover after the administration of preparations of iron, or of bitters, the so-called tonic medicines, in combination with nutritious food, we may attribute to the latter the greater influence in effecting a cure, because the materials contained in it are of infinitely more importance in maintaining the integrity of the human body than iron, bitter extractive matters, &c.

We have gradually succeeded in establishing at least approximately correct empirical laws for certain processes and phenomena in the living body, though not in ascertaining their causal connection, their essential conditions and laws. This holds good especially with respect to many of the physical and chemical processes in the body—such as digestion and nutrition, the conversion of tissues, the processes of respiration and excretion, and the circulation; also with respect to the so-called law of diffusion of gases and liquids, in endosmose and exosmose, in absorption, &c. In the nervous system, also, we have acquired some knowledge of the state of the nerve-fibres which produce sensation and motion, partly in its peripheral, partly in its central portions, and have even established certain empirical laws according to which they act; and however imperfect this knowledge may be, we have still gained much in the way of a better comprehension of the whole apparatus. We can now, better than heretofore, surmise, if not explain, the origin and course of many diseases, by tracing them to a deranged state of the internal mechanism; and we can better ascertain the mode in which certain external agents and influences act upon the various processes or functions in the living body. In short, physiology, as well as physics and chemistry, have already furnished us with many empirical truths, by the application of which to the problems and phenomena of our own department, we gain a readier insight into them, or at least the prospect of acquiring such knowledge at some future time. We owe much, therefore, to the other natural sciences, and must continue to do so, not only because man is merely a link in the whole chain of nature, but also because investigation pursues a more exact routine in them than in the science of medicine.

Our knowledge of the intrinsic connection between the vital processes, the laws to which they are subject, and the influence of external agents upon them, is at present so imperfect, that it does not enable us to deduce from these circumstances, or to give any clear explanation of, the phenomena and occurrences which daily present themselves to us. Bearing in mind the fluctuating and fallacious nature of empirical laws, we must always remember the possibility of entirely different relations, of an entirely different causal connection subsisting between the objects of our study, from that which we had previously imagined. We may be right, or we may be wrong, and thus it must be until we have penetrated beyond mere experience, or empirical laws, to a true comprehension of the phenomena—to actual laws of nature.

We shall now proceed to speak of the means and processes which may, more fully than simple experience and direct induction, enable us to attain our end.

SECTION V.

OF THE METHODS OF INVESTIGATION AND THE AUXILIARY PROCESSES THROUGH WHICH ALONE WE CAN ATTAIN TO HIGHER AND MORE EXACT SCIENTIFIC KNOWLEDGE.—ANALYSIS AND THE SYNTHETICAL (DEDUCTIVE) METHOD.

PROGRESSIVE RESOLUTION OF MANY AND COMPLICATED, INTO FEWER AND MORE GENERAL, LAWS. PROCESS OF REDUCTION OR GENERALIZATION.

IT is hoped that the two foregoing sections will have sufficiently explained the impossibility of arriving at a clear comprehension of the causal connection and conformity of the phenomena and occurrences in medical science, whether in their development, or operation ; an impossibility which, when such complicated processes and influences are in question, must exist in spite of both direct observation and experiment, however careful. We have also stated the reasons which render it impossible, either by the direct method of observation or induction, to arrive at anything more certain than our so-called empirical facts, or laws. It is in vain therefore to attempt, by the direct observation of vital phenomena, as such, to discover the actual connection and conditions of their development and course—in short, their conformity to fixed laws. This is no more possible than for the chemist, from simple observation, whether of the atmosphere, water, or any other complex compounds, to understand their real nature, and gain an insight into the conformity of his chemical combinations and processes. No more would the physicist, from observations of the weather and its complex and variable phenomena or processes, have arrived at the discovery of its causal connection and the laws of its meteorological phenomena.

We cannot say what a so-called function, a disease, or its cure, really is, *i. e.*, in what it may consist and upon what it may depend, unless we first know what individual phenomena have produced it, and upon what these, in their turn, depend. We must acquaint ourselves with the laws and learn the order according to which each of its elements has been developed and has acted. There is therefore only one method, which has already been alluded to, of attaining a clear insight into the conditions and laws of our complex phenomena and processes. We must, first of all, endeavour to understand all the individual occurrences, or elements which constitute a so-called function, a given process, or a disease; and further, we must ascertain the conditions and laws which produce and govern these individual elements, or occurrences. Thus, to understand the mode of operation and the office of the complex external influences which affect the living body, we must first endeavour to ascertain the properties and action of each of the individual circumstances and agents in the atmosphere, in our food, or in a certain neighbourhood, with a given mode of life, &c., which act upon the body in various combinations. And from the laws once established by us of all these individual occurrences and influences, of these individual causes and effects—at least of the most important and essential of them—we must finally deduce the causal connection and conformity of those complicated occurrences, or effects, in the living body, as well as the laws of operation of those complicated agents of the external world.

In other words, before we can explain the development of a given function, a disease, a cure, or any other complicated process, we must first have ascertained the conditions of each of the individual elementary processes, effects, &c., in it, and traced their general conformity to fixed laws; and we must go on to deduce the conditions and laws of those aggregates from the conditions and laws of these their individual elements. In short, while we adopt what we are accustomed to call the deductive method of investigation, and inasmuch as we seek to explain a complicated whole from the laws and conditions of its individual elements, we may also term our method the constructive, or synthetical. Since it has been shown to be impossible to ascertain the conformity and connection of effects and influences under the aggregates in which they appear to us in nature, we must in some degree understand the connections of each individual effect and occurrence, before we are able to determine its precise influence and mode of working; and having once established laws, both for the individual occurrences and effects in question and for the determining influences which affect them, we might, under favorable circumstances, find ourselves in a position to deduce the laws, that is to say, the development or operation of these very aggregates, and thus be able to explain them from the former. Instead of taking the complicated whole and attempting thus to understand and to explain its nature and occurrences, we must rather hope to understand and explain it from the separate consideration of its component parts and elements. Thus the anatomist, in order to comprehend the complicated organs submitted to his investigation, must first isolate and recognize their individual integral tissues. The direct experimental method of investigation, or induction, as it was described in Section III, would thus be replaced by the constructive, or synthetical method, *i. e.*, by deduction.

Thus the chemist is first obliged to isolate the individual elements of the compounds which he meets with in nature, whether in the atmosphere, water, minerals, or certain organic substances. He must first study separately the peculiar properties of those elements, and observe the changes which they exhibit when they come into contact with others, and the laws which regulate their operation, before he can deduce, with more or less certainty, the conformity in the formation and operation, and the behaviour of those complex combinations from those of their component parts and elements. Modern animal chemistry especially has no longer been content to observe the ingesta and egesta in the living body merely as a whole, but has separated them into their individual parts, or constituents, and examined each of them severally and in detail. And while it has become more intimately acquainted with the behaviour of the various constituents of our food, while it knows more than it ever did before of the nature of the air we breathe, more about the blood, the organs and products of excretion, and the constitution of the fluids necessary to digestion, it can, of course, better than ever before, surmise in what manner, and according to what laws, the chemical processes in the body are likely to occur. Just so has the natural philosopher been able better to ascertain the conformity of his complicated phenomena and processes (*e. g.*, of the air-currents, the weather, &c.), from having become better acquainted with the laws of operation of the individual agents upon which they depend (*e. g.*, of gravitation and atmospheric pressure, of temperature, moisture in the air, &c.), and with the conditions of the individual phenomena, or occurrences, in which those complicated processes themselves consist. This method is no doubt an extremely circuitous one, for instead of directly attacking the objects of our investigation,—the phenomena and processes in the living body, as they present themselves in nature,—we must endeavour to approach them indirectly—as it were *a posteriori* and from within. But however this may be, and whether we are able and willing to carry out all these methods of investigation, or not, they must, in every case, be carried out before we can either understand or explain the conformity of our phenomena, or construct an even approximately correct theory of them. And in so doing we shall be in the less danger of yielding to illusions, of exhausting our powers on barren soil, of expecting to acquire knowledge from unsafe auxiliaries, or of accepting fallacies for certainties.

Just because we have not recognized this as the only possible method, and failed to appreciate why it is so, we have not made so much progress as we might have done, whether in physiology, or in the theory of diseases, and their cure.

The mere fact of observing a broad stream flowing past us would not acquaint us with the place whence all the water came. To discover this, we must first seek out all its tributary streams, and trace each of these to its source. We cannot say upon what a complicated process (*e. g.*, a function or a disease), which is compounded of a certain number of individual processes, depends, and in what it consists, unless we first know in what each individual process in it consists, and upon what it depends. Equally little can we judge how a complex influence, composed of certain individual elements (*e. g.*, weather, climate, food, mode of life, a certain mode of treating a patient) may act in the living body, unless we first understand the operation of each individual agent, or circumstance in it. If we once know that a given process or effect in the living body consists in certain individual processes or effects, we must go on to investigate these, and endeavour to determine their causal connection and conditions. And if we know that a given effect can be produced by a certain aggregate of causes or influences, we have no alternative but to ascertain separately the mode of operation of each.

There need also be no question as to where we are to seek for the conditions and conformity of the vital phenomena and processes; for if we ask how the allied natural sciences, which have advanced so much further than our own—chemistry and physics—have already ascertained, more or less precisely, the laws of *their* phenomena and processes, we find that it is through examination of the substances, elements, and compounds, which present those phenomena and processes; and the same holds good in the case of all the influences and circumstances upon which the production of the latter depends, and by which they are modified in various ways. The conditions and laws of the vital processes, whether in health or disease, are therefore to be sought for only in the individual processes which constitute "life" as a whole; and the conditions and laws of the latter are to be sought for only in the organs and materials of which the body is composed. And since life itself is only possible under the constant influence and by the constant co-operation of the external world, we must consider as its ultimate source, or condition, the medium in which the living body has its origin and end, in connection with the aggregate of the external agents and influences which are indispensable for its maintenance and integrity, viz., food, warmth, light, &c. We are thus in a position to understand the changes which, in the case of disease, take place in the living body, and to trace out their producing cause. Some change, in consequence of certain external and internal circumstances and conditions, has taken place in its organs, and interfered with the mechanism of their operation, so that what occurs in them ceases to occur in the usual normal manner; they no longer conform to the order of their healthy activity and the result of their working, therefore, is not health.

The method to be adopted in this investigation may therefore be divided into the following stages.

I. We must, from what we observe in nature, looking at it as a complex whole (*e. g.*, a certain function, or disease, the atmosphere, food, &c.), become acquainted with the individual parts of which it consists,—its various elementary processes and agents, or materials, &c. We must take this whole to pieces, or at least, through a process of artificial analysis, imagine it to be taken to pieces (a process which has been alluded to when on the subject of induction). We must first of all ascertain the individual parts or elements of which these complex objects are made up, and then determine the respective causes or conditions of each individual process or effect, and the respective effects of each individual influence and determining circumstance in them.

II. Having once become acquainted with the conditions of the individual processes, or effects, and with the modes of operation, or laws by which the individual influences and agents act, we have now, from the consideration of all this, to determine what have been the causes of a given aggregate of those processes, or effects, and what have been the effects of a given aggregate of those influences, or agents, under given circumstances. In other words, we have to seek to deduce from the special laws or theories of those elementary processes and influences the theory of these aggregates, *i. e.*, the conditions of the complicated series of phenomena and processes which occur in medical science, and the effects which follow upon these complicated influences and causes.

Could we bring all these to the test of exact calculation, our problem would be solved as completely as the great problem of astronomy—the comprehension and explanation of the motions of the heavenly bodies—was solved when Newton and others succeeded in deducing with certainty those motions from the laws of gravitation. We might, for instance, explain in a similar manner what occurs in respiration, *i. e.*, we could point out its causal connection, were we able to demonstrate so clearly the conditions which attend upon the excretion of carbonic acid and watery vapour, and to follow the conversion of certain components of the blood, and the production of heat which accompany that process, as to be able to deduce from the conditions and laws of these individual processes those of the whole process of respiration. Essentially the same thing holds good in the case of every disease, and of the various influences of the external world. We should be able to understand all the so-called functions and diseases and all the influences of the external world upon them if we only knew in what causal connection each of their individual processes stands, and in what manner each individual influence acts upon them; for we could then calculate what share each has in the production of the whole, how and why, for instance, all those individual processes in a function, or in a disease, occur simultaneously or consecutively in this given order and manner, or why the complicated agents of the external world affect them precisely in a certain manner, and not otherwise. The total result or effect of a whole series of influences and determining circumstances amounts, in fact, to nothing more than the effects of all the individual influences and circumstances taken together.

Thus experience has long since shown that persons who ascend high mountains suffer from difficulty of breathing, a sense of constriction, a feeling of weakness, &c., and a question arose as to how these derangements were produced. While some traced them to the relatively small amount of oxygen in the air, others with more reason declared the rarefaction of the air and diminished pressure to be the conditions of those phenomena. This question, however, is not to be answered certainly until we know exactly in what manner and degree each individual agent, or circumstance concerned may act. We must, to this end, enquire into the composition and temperature of the air at such elevations, into diminished atmospheric pressure, and consequent disturbance of the equilibrium between the external pressure and the tension or counter-pressure of the fluids and gases contained in the body. For had we observed and compared these effects in the case of each individual agent by the aid of an adequate variation of the circumstances, and above all by the method of exclusion (*e. g.*, the effects of diminished pressure separately in the air-pump), we might, from all this, calculate with certainty what would occur under given circumstances, *e. g.*, at a certain elevation, and explain why it so occurred. We should understand a disease just in the same way were we in possession of all its occurrences, and the causes whence they arise. Thus, in inflammation of the lungs, we should be able to determine the connection between its individual phenomena and processes were we once able to ascertain the conditions that affect the increased heat of the body, that accelerate the pulse, that work changes in the excretory processes of the lungs, in the composition of the blood, and in the general nervous system of a patient affected with pneumonia.

The foregoing pages show not only the importance of this so-called deductive or synthetical method, but also its difficulty, and, in a relative sense, impossibility in our science; and to this subject more special allusion will be made at a later period. We find, however, on a closer examination of all our theories, and more especially of all the *a priori* constructions and hypotheses by the aid of which we have hitherto attempted to explain any vital process, any disease or its cure, that this method and the expedients in it have already been adopted, more or less, though often almost instinctively and in a confused manner. Yet through a careful examination even of these expedients the individual steps will become more clear to us which we must take in those more exact theories, or attempts at explanation, but which we have not hitherto taken with the necessary insight either into their requirements or real signification.

Demonstration of the causal connection of individual phenomena and processes, and of the laws of operation of individual determining circumstances and influences. Deduction of the causal connection of the complicated processes and influences of medical science.

That first step—the demonstration of the conformity to fixed laws of each of the individual processes and influences—assumes another step as having been already taken. We must, namely, first of all, have become acquainted with the individual processes, agents, or influences, which are concerned in the development of a function, or of a given disease. In most cases, this is not very difficult. We must observe all the phenomena as they occur in nature, whether in a function or in a disease, either as nature presents them to us in her great agents,—*i. e.*, in the atmosphere, food, &c.,—or as we have been able to produce them in an artificial or modified form by the aid of experiment. Above all, we must observe *in what individual processes, phenomena, or circumstances*, those aggregates themselves consist; and if we are unable to do this directly, by the help of our unaided senses, we must at least endeavour to take them to pieces mentally, and to ascertain of what parts or elements they are composed. We must go through that mental analysis of the aggregates of effects and causes presented to us by nature, of which mention has already been made in reference to the inductive process.

Having effected such an analysis as completely as possible, the next important step is, to ascertain the causes and effects of each of these individual links in the chain,—*e. g.*, the conditions of an individual process in the living body, and its ultimate action upon other processes in it; also we must determine the manner in which any individual influence of the external world acts upon the body. It is self-evident that, in order to do this, we must observe each of those processes and influences, each individual cause and effect, separately, and isolated, as far as possible, from the others, and then ascertain everything which may affect and modify in various ways an individual process,—*e. g.*, in respiration, in digestion, or in fever. Thus, we might determine, with tolerable facility, the influence and laws of operation of the seasons or the weather, and observe how food acts upon the healthy, or how a given mode of treatment affects our patients, could we but cause each individual agent or circumstance, in those aggregates of causes, to act separately, and watch, in the former case, the various degrees of temperature and moisture in the atmosphere, the individual materials for the maintenance of the body; in the latter, the action of any individual remedy, or the natural progress of disease, apart and detached from all co-operating influences. Could we once determine the circumstances and influences upon which each individual process depends, and what changes will inevitably follow each of those circumstances and influences, we could then bring each effect and change in the living body with their concomitant conditions, each influence, each cause with the tendency of its operation in the living body, into their real connection. Hence, also, we might deduce consistently, on the one hand, the conformity and causal connection of all the vital processes, and, with them, the conformity and causal connection of every disease; and, on the other, the conformity of the influences they receive from the complicated agencies of the external world.

To proceed, however, in this direct and simple manner is, as has already been explained, impossible in the case of the objects with which our science has to deal. Our phenomena and processes are never, as is well known, to be met with in an isolated form; they occur rather in the closest connection with many others, as more or less complex aggregates of causes and effects, and we cannot, even by an artificial process, separate them from each other. We cannot, for instance, as the chemist can frequently do with his substances, or the physicist with many of his processes and machines, arrest all the other processes in the living body, for the purpose of investigating a single one, without arresting the mechanism of the whole apparatus.

In seeking to ascertain, for instance, the individual processes in respiration, we cannot isolate the lungs with their circulation, the blood in their arteries and veins, or the absorption and excretion of certain gases by them, and then examine what changes the constituents of the blood always undergo under the single influence of respiration, or of the absorption of oxygen into the blood; nor can we determine what influence the individual processes in the body itself may exert upon them (*e. g.*, the capacity of the lungs,—*i. e.*, the quantity of air inspired, the rapidity of the circulation, the pressure of the column of blood, or various processes of excretion, &c.). We are equally unable to investigate directly the modifications of the individual processes in respiration, as they may occur in consequence of the separate operation of the individual agents in the atmosphere (*e. g.*, its pressure, composition, moisture, or temperature), or how the particular mode of the conversion of tissues and the changes of individual constituents of the blood in respiration, may react upon the individual processes in the living body (*e. g.*, upon the production of heat in it, upon various processes of excretion, upon conversion of tissues and nutrition).

In the same manner, it would be desirable for us to be able to observe and separately investigate, at any rate, the more important and influential of the elementary processes in a disease, in their conditions and action. It is, however, unnecessary to point out the impediments to such a mode of proceeding. If we were called upon to investigate a given derangement of the process of respiration, we should, first of all, have to determine which of its parts had undergone a change, and what the change was which might produce certain alterations in the whole mechanism of respiration, with their consequences for certain processes in the living body, and, therewith, for certain diseases. If we assume the most favorable case, viz., that we can produce a disease artificially, as an inflammation of the skin, or of the parenchyma of the lung, we perhaps find the organ in question gorged with blood, or loaded with so-called exudations which were not present before; further, perhaps we find certain changes in the composition of the blood and temperature of the body, in the circulation, in certain processes of excretion and of innervation, pain, spasm, &c. But to understand in what these derangements essentially consist, how they are connected with each other, and upon what they depend,—in short, to recognize their mechanism, conformity, and fixed order,—we must first have learnt the causal connection, for the time being, of the individual processes in such inflammation. We must also have learnt the influence of the peculiar composition of the blood in it, of the circulation and its various stimuli, of exosmosis and the various processes of excretion, of the conversion of tissues within the body, of the production of heat, and of the tendency of the nervous action. We must have made ourselves acquainted with each of these modifications separately, under given circumstances, and as they are influenced, whether by other processes in the living body, or by external agencies, —such as temperature, composition of the atmosphere, ingesta, &c.

All this holds good in our investigation of the numerous influences and agents of the external world, whether they are termed hygienic influences, or remedies. To understand the part which they may have played, as well in the normal course of our lives, as in the development of the derangements in a disease, or in the cure of that disease, we must, first of all, have studied them separately, and determined the laws of their operation. Before we can lay claim to even an approximately clear insight into the mode of operation and influence of a given state of the weather, of the climate of a certain district, or of any particular season of the year as a whole, upon our body as a whole, we must first have become acquainted with their individual parts,—temperature, moisture, atmospheric pressure, winds, &c., —and learnt to understand their action upon the individual processes of the body,—*e. g.*, upon its temperature, upon exhalation, the circulation, conversion of tissues, &c. We must not only be informed as to how each of these acts separately, but also as to the result of their various combinations with each other; we must know, further, how their effects, through mutual modifications, are increased or diminished, or perhaps altogether cancelled. In the same manner, we must not only determine the effects of our remedies, but watch over the less direct influences exerted by the individual elements of our food, by the occupations to which the patient has been accustomed, and by other hygienic agents more or less directly active. For, so long as we remain ignorant of the office and effects of the individual circumstances and influences, we shall also be ignorant whether, or in what manner they may have conduced to the development of a disease, or to its cure; and much less are we in a condition to form a correct estimate of the influence exerted by whole groups of such agents. It is doubtless difficult,especially in the case of man, to investigate these agencies and influences in their separate operation upon the living body and its individual processes; and it is certain that they have not hitherto been so investigated.

Here, as elsewhere in our department of study, we are met at the first step of our investigation into the laws of operation of individual influences, and into the conditions of individual occurrences or effects, by difficulties which have already been alluded to, and which the physicist and chemist have not to contend with, either in the same form or to the same extent. But although they have many advantages over us in the shape of a considerable certainty of *a priori* knowledge, and a basis of many firmly established laws to rest on, they would scarcely venture from these known laws to infer what, in the case of certain individual processes, effects, or agents, might occur under a complication of circumstances the details of which are still unknown to them, or on the accession of entirely new and obscure elements. We, however, are often less scrupulous, and it is only the great difficulties of our subject, and the necessity which the calling of a physician entails, which can in any degree justify our ventures in the eyes of the allied sciences more advanced than ours in the investigation of nature.

But the greatest difficulty which we meet with is, in the end, the great confusion and multiplicity of the things with which we have to deal, and the impossibility of submitting them to a direct and separate investigation. Each individual phenomenon, each individual process in the living body, is, in consequence of this multiplicity of causes and effects, bound within a knot, the single threads of which we can scarcely trace with certainty, and in the attempt to unravel which many have succumbed. But we have already gained much if we have acquired a clear insight into the peculiarities of the things submitted to us for investigation, and a knowledge of the problems which we must solve in the first instance. Here, as elsewhere, this may be regarded as the first step towards the understanding of our whole subject, for where the human mind has once reached this point, it has always found the means of attaining its end. We can now, at least, guess what food can alone satisfy us, and shall either reject what is inferior, or, at best, regard it but as a succedaneum.

If we cannot easily isolate our phenomena and influences, and investigate them separately; if we are obliged rather to confine ourselves to those variable aggregates which nature offers us, we should accustom ourselves more and more to regard them as what they are, and not, as so often happens, as things in themselves simple and uniform. The first step is, therefore, to divide those various aggregates of effects and processes, of influences and causes, into the individual processes and circumstances of which they are composed; not, indeed, by fire and steel, but in our minds and thoughts. The necessity of this analysis has long been acknowledged, or at least felt instinctively, and has already been carried out in certain points and in many quarters.

A different state of things prevailed of old, and naturally so, in consequence of the then imperfect comprehension of nature. Our predecessors necessarily confined themselves to those great appellatives as they presented themselves, for instance, as "life," "health," "disease," "death," &c., *i. e.*, to the abstract notions which, in the first stage of their intellectual activity, they had been able to form of all these things. They also imagined equally great appellatives, or collective notions, as the causes of these, whether gods or God, nature or vital force, soul, or mind, &c. In the course of time, however, men gradually learnt to distinguish the individual parts of things, to detect them in the aggregates, and to bring them into connection with their respective causes. This analysis has already acquired a greater extension, which the necessity for it will continue to increase. If the anatomist, on the one hand, has long succeeded in dividing his complicated organs more and more into their finer parts, the physiologist, on the other hand, has learnt to distinguish the individual parts in the aggregates of his functions, or processes, and to investigate, in detail, the manifold phenomena of which they are composed. Even as regards the act of dying, men are no longer content to perceive something uniform and constantly identical—*i. e.*, death—but have rather learnt to distinguish those chief powers in the animal economy, the suspension of which can arrest the whole process of life. In like manner, men have ceased, in the investigation of their complex agents and substances, to consider them merely as wholes, known only through their properties or effects, but have rather striven to ascertain upon which of their individual elements and agents, and, again, upon which of the properties and modes of operation of these very agents, their total effect upon the living body depends. And the influence of this necessity soon made itself felt in the theory of disease. Men soon began to separate the old unities in diseases, or "Entia," into the individual elements of which they are composed. Thus the anatomical changes in form and texture in an entire disease were distinguished and examined separately; and the same method was extended to changes of composition, and to individual processes and phenomena in general, so far as they appeared to occur according to chemico-physical laws, and according to those which regulate nervous action.

But so certainly as this division or analysis must never be regarded as our final aim, but only as the first step towards it, *i. e.*, towards the comprehension of the living whole; just as certainly must our chief object now be, to determine the causal connection and conformity of all these individual processes and phenomena, that we may finally deduce from them the causal connection and conformity of the whole, *e. g.*, of a function, a disease, or a cure. And here, again, we meet with precisely the same impediments which have been already so often mentioned. But however complicated the processes in the living body, whether in health or disease, may be, and however closely they may be connected with each other, we know, at least, that they arise from what takes place in all the organs and substances of which the body is composed—from their properties, the tendencies of their operation, or their reciprocal action, just as, in fact, every effect whatsoever must spring from its individual causes. It appears no less certain that the complexity and multiplicity of the phenomena which everywhere present themselves to us, arise only from the great number of the individual processes, effects, and influences *i. e.*, of the elements which are concerned in the development of those complex phenomena. However obscure, therefore, the connection and conformity of the vital processes may appear to us at present, and however difficult it may be to establish the fact of their connection, we know, at least, that their conditions and conformity are, in fact, only the result of the laws by which all those individual constituent processes, or agents, act, where the tendency of operation of the latter is not modified, or perhaps cancelled by their own simultaneous occurrence. Whether, therefore, those individual occurrences and processes in the living body be of a chemico-physical nature, and their laws, consequently, to be termed chemico-physical; or whether they diverge in their whole character, at least in certain respects, from the occurrences which have hitherto been regarded as chemico-physical, and, consequently, their laws are to be termed "vital" laws, is not important here. Having divided our aggregates—a function, a disease, or the external influences which affect them (*e. g.*, the atmosphere, weather, or food)—into their individual processes, or agents, it remains for us to investigate the causal connection, the causes and effects, in short, the conformity of each of these separately. This investigation must be conducted in the manner already laid down for the general process of induction. Our chief means are, here and elsewhere, observation and experiment.

We must, however, choose the starting-point of our investigation, according to the particular circumstances of the case in question, *i. e.*, we must first strive to ascertain the effects of a given cause, or influence, or the causes and individual modifying circumstances of a given effect, or process, as well in the living body as in the external world. We shall also generally have to investigate both, either together, or in close connection with each other. In the former case, we must first separate the great agents of the external world, *e. g.*, the atmosphere, climate, food, occupation, and mode of life, &c., into their individual elements, and examine them singly in relation to their effects upon the living body. The same holds good as regards the manifold agencies of our remedies. Our aim must then be to ascertain and determine the mode in which each active element works upon the individual processes in the living body under the various modifications which their effects undergo, whether by age, sex, and other peculiarities in individuals. In the latter case, on the contrary, we confine ourselves to the investigation of effects and processes in the living body itself, whether in health or disease. Having, as far as possible, isolated and divided these aggregates into their elements (*e. g.*, into the individual processes in digestion, respiration, the circulation, certain processes of excretion, the conversion of tissues, or innervation), we shall then have to investigate what occurs in these individual processes; which of them appear changed in a given disease, and in what manner, and according to what order and constant sequence. We must further seek to ascertain in what manner these individual processes are changed under the most various internal and external influences, under what circumstances these their various changes occur, and how, in consequence of such changes, they may reciprocally affect each other.

Just as it is infinitely more simple and more safe to seek for the possible effects of a given cause, or influence, than for the possible conditions of a process, or effect, which is still obscure, especially under such complex circumstances as those with which we have to deal, it is always better, if we have the choice, to begin with the former than with the latter. We shall, consequently, have first to direct our investigations to hygienic agencies and influences, and to the possible causes of the diseases with which we have to do, and then to pass on to our numerous remedies, and their mode of operation.

A few examples may serve to render the mode of our advance more intelligible.

If, for instance, the problem to be solved were the final determination of the influence which a marshy district exerts upon its inhabitants (*e. g.*, upon their liability to intermittent fever), we must first endeavour to ascertain all the individual circumstances, or agents, which play any part therein, we must consider the composition and other peculiar characteristics of the soil and atmosphere, the temperature and its oscillations at different periods of the day or year, the composition of the water which those people drink, &c., and it will then be necessary to determine the precise effects of these various agents upon the living body. We must, therefore, study the changes which the individual processes in the living body, such as its nutrition and the composition of the blood, the various processes of excretion by the skin, kidneys, lungs, &c., its temperature, the circulation of the fluids, and the conversion of tissues within it, and the various modifications of innervation, undergo by the operation of each of these agents, and to this end we must acquire a more exact knowledge of the influence, in each case, of these external agents upon the vital processes, by a fitting variation of the circumstances, by observations and experiments of various kinds, especially by the aid of the "method of exclusion," and by comparison with the more or less analogous effects of other influences—in short, by all the auxiliary methods of investigation already alluded to. Under favorable circumstances, we should now, from the consideration of these established effects, or laws, in so far as they have been proved to be actively concerned in the result in question, be able to deduce the effect of marshy districts, and bring it into conformity with fixed laws; and now we should also understand, and, consequently, be able to explain, the development of intermittent fever, the so-called miasmatic cachexia, the enlargement of the spleen, &c. We should no longer be compelled, as heretofore, to include all the active determining circumstances, or agents of those diseases, in the abstract notion of a miasmatic poison, or malaria, by which, after all, everything and nothing is explained. Even if, in the most unfavorable, and, unfortunately, most probable case, all those investigations in detail should fail to bring us to our precise object, and we should remain unable to determine, with certainty, the conditions of that miasmatic influence, and of those diseases, we should none the less surely have gained much important information, which might essentially facilitate our comprehension not only of this, but of many additional points. This would be the case even if we had obtained comparatively negative results only from all these toilsome investigations, if, *e.g.*, we could demonstrate, as is very possible, that neither gases, the water, nor poisonous matter of any kind, any more than the considerable reduction of temperature of the body, and the like, were essentially concerned therein. And we should now find ourselves justified in drawing the conclusion which was, *a priori*, the most probable one, that in the inhabitants of those marshy districts, the material substratum of their bodies, and certain of the processes in them, experience, equally with the soil itself, or some of its component parts, certain peculiar changes; and that they do not fall sick in consequence of these peculiar states of the soil, but rather only concomitantly with them.

Again, let us suppose that experience has shown a definite state of health, whether good or bad, or an unusual frequency and malignancy of certain diseases, to exist among certain classes of the population, in individuals whose occupations and mode of life we know. Here, also, we are unable to form any precise notion of the part which any individual agent may play therein, *e.g.*, their work, and the exertion necessary for it, unless we also know whether, and in what manner, the other agents or influences act, and what is the tendency of their operation, *e. g.*, the state of their dwellings and workshops, cleanliness, temperature, and degree of moisture of the atmosphere, and, still more, the quality and quantity of their food. To acquire any comprehension of such liability to disease, and its external causes, we must, first of all, seek to isolate as completely as possible, and to establish the mode of operation and the importance of each of those individual influences.

The same thing holds good as regards remedies, whose real importance lies in the ends for which we employ them. Every reflecting physician will confess that we know very little, for instance, of the peculiar mode of operation of what are called sudorifics and diuretics, or tonics, not even in the healthy, much less in the sick. To gain more knowledge on this subject, we must no longer confine ourselves to the investigation of the effect of those remedies as a whole, whether it be real, or only apparent, and arbitrarily interpreted; we must no longer speak superficially of their sudorific, or diuretic effects, of their tonic, stimulating or depressing, solvent, alterant effects, &c. We must rather first direct our investigations to all the individual processes and circumstances in the living body, by the co-operation of which excretion by the skin and kidneys, absorption, nutrition, &c., are effected. We must first ascertain, as precisely as possible, the internal and external circumstances and influences upon which those individual processes, *e. g.*, excretion by the skin, kidneys, &c., may depend, and by the operation of which they undergo certain modifications, such as an increased consumption of water, variations in the composition of the blood (in relation to the amount of salts contained in it), the consistence, circulation, and pressure of the column of the blood, the relative temperature of the body, and of the surrounding atmosphere, the relative inactivity of other processes of excretion, &c. Or if we would acquire a clearer notion of the effects of bleeding as a whole, we must first investigate, in a similar manner, how it may modify the individual processes in the living body, *e. g.*, how the abstraction of a given quantity of blood may influence the motion and pressure of the rest, the rapidity of its circulation, and perhaps its composition, &c.; further how it may perhaps, in consequence of all this, influence the processes of endosmose and exosmose, the intensity of the respiration, and of other processes of excretion, the generation of heat, &c. And it will always remain necessary, in every such investigation of the changes produced in an individual process in the living body by a given influence, or agent, not only to exclude, as completely as possible, all other important influences and agencies, but also to keep all the other processes and functions, as far as possible, in their ordinary equilibrium. We shall the sooner attain our present object—the determination of causal connection—the better we succeed, at least approximately, in causing the influence in question alone to act, and this in such a manner that the particular process in the living body which we wish to investigate in relation to its conditions, *e. g.*, to its dependence upon that influence, may alone undergo a change. In short we must, so far as it is possible to do so, exclude everything which could render the question more complicated, and our conclusions even less valid than would otherwise be the case. If, for instance, after the long-continued use of a substance such as iodine, mercury, common salt, and the like, a certain change, *e. g.*, emaciation, had been observed more or less constantly, we must try to ascertain whether those substances had in themselves been positively concerned in the production of that effect, and it would be impossible to decide this point with certainty if the appetite had at the same time decreased, digestion been deranged, or the supply of food been in any way diminished. But if these, and other complicating circumstances, had been sufficiently excluded, and it had been demonstrated that the emaciation essentially depended upon those substances, we should next have to investigate by what individual intermediate operations, or physiological steps, the emaciation itself had been produced, *i. e.*, what processes in the living body had been influenced and changed by them in such a manner as finally to produce the result in question. It would then be our office to determine whether certain processes of excretion—in short, the egestion of materials from the body—had in some manner been increased, or whether the usual supply had simply been diminished, or interrupted, whether it had occurred from want of appetite, and from the small quantity of food taken, or in consequence of some derangement of the processes of digestion, assimilation, &c.

To judge whether, and how far, a disease has been removed by a certain remedy, we must first ascertain by a sufficient variation of the circumstances, and by the method of exclusion, &c., what part entirely different agents, or influences, such as diet, rest, and, above all, the natural course of the disease itself, have played in the cure. We must, therefore, in many series of cases, have seen sometimes the one, sometimes the other of these agents in operation, and have determined, by a comparison of the results, under which of those circumstances the patient has most frequently and rapidly been cured. In the most favorable case, however, the positive value of our remedy would thereby only be established empirically; the fact, namely, that the disease had been cured by it, *e. g.*, syphilis by mercury, or intermittent fever by quinine. To understand further why such remedies act beneficially, we must ascertain all the modifying or essential changes and processes in the disease, and the way in which it is wont to be acted upon by our remedies.

If, on the other hand, we direct our investigations to the phenomena and processes of the living body, in health or disease, we must, first of all, divide them into the individual agents, or elements of which they are in each case composed —into their physiological links—and then investigate these separately in relation to their general behaviour, conditions, and mode of operation upon the other processes in the body.

Let us take one of the comparatively more simple functions —the circulation. Having first distinguished its individual mechanisms or agents as a whole, we must examine these separately, and ascertain the circumstances upon which they depend and their reciprocal action upon each other. Thus we must ascertain the pressure which the heart's contraction communicates to a given column of blood, and the resistance offered to it in the bloodvessels and capillaries, &c.; the consequent rapidity of its motion in vessels of various calibre; the pressure which the column of blood exerts upon the walls of the vessels, and other analogous points. Having once determined the conditions, effects, and various modifications of the comparatively simple and, for the most part, mechanical forces, or processes in the circulation, we might then, with more prospect of success, proceed to the investigation of their action under the complex relations and influences of the living body, and to the discovery of the part which its other functions and processes, in operation at the same time, may play therein, *e. g.*, the respiratory movements, and the intensity, in each case, of the process of respiration, and of various other processes of excretion, as well as certain tendencies of the nervous activity. We should finally be in a position to deduce the causal connection and conformity of the circulation as a whole, from these laws of the individual agents or processes, the causes and effects going on within it.

Essentially of the same kind would be our advance in the investigation of a given disease. In the case of each patient, we must divide the aggregate of his deviations from the normal state into its individual processes or elements, into all its connecting physiological links, and endeavour to determine for each its causal connection, conditions, and action. In fever, for instance, we must first observe its more constant phenomena, its more constant and influential processes from the commencement to the termination of the disease, and ascertain their behaviour during its whole course, their modifications, in each case by certain external and internal circumstances, and their probable conditions and reciprocal action upon each other. This holds good, for instance, for the increase and variations of the temperature of the body; for the circulation, and the various processes of excretion by the skin, lungs, kidneys, internal mucous membranes, &c.; for the composition of the excretions, and of the whole mass of the blood, and finally for the varying functional states of the nervous system in its different offices. If then an increase of temperature in the body, an acceleration of the circulation, a certain change in the composition of the blood, in the conversion of materials within the body, and in the state of the nervous system had been shown to be the most constant and most important phenomena in a case of fever, we should next have to separate each of those phenomena into its individual elements, and to investigate their causal connection in the manner already pointed out. We might then be able to deduce the conditions and conformity of the whole process of fever, *i. e.*, to construct a theory of "fever" as a whole, so soon as we had once ascertained the conditions and laws of its essential integral processes, or changes, and the causes of the modifications in them resulting from their simultaneous or consecutive occurrence in that disease.

We must always fix the starting-point of our investigations in the most familiar and accessible portion of our subject, for the path of perception leads from the known to the unknown, from what is single and comparatively simple to what is complex. In the investigation of a function, or of a disease, therefore, we shall more frequently have to commence with its phenomena, or effects, than with its causes or modifying circumstances. Those phenomena and processes in a given function, or disease, are always more familiar to us, and also infinitely more accessible than their possible conditions and causes. If we have once fixed upon a given circumstance, or agent, as probably determining and essential in a certain function, we can then the better investigate it in its possible effects upon various processes in the living body. In considering, on the contrary, the agents of the external world, whether it be the so-called hygienic influences, or remedies, and poisons, we must commence with the investigation of their active elements, and with the properties and mode of operation of these, and not with those complex and variously dependent processes in the living body themselves, which have either been imagined to be the effects of the former, or proved to be such by experiment. Frequently, however, it is the latter which first becomes known to us; we learn the facts, that men are attacked by intermittent fever in certain districts, and that they are cured by quinine; that men experience dyspnœa and syncope at great elevations, or that the workmen in certain manufactories are attacked by colic and paralysis; that men who have swallowed a certain substance, or who, like the Turks, have smoked opium, exhibit certain peculiar symptoms of poisoning, *e. g.*, intoxication or narcosis. It is here from these empirically established phenomena, or effects alone, that we can acquire a knowledge of their causes, and ascertain their mode of operation.

Whether we first direct our investigations to the effects of a given influence and circumstance, or to the conditions of a given process, or event, we shall always, in medical science, have to deal with more or less complex and unknown objects. To facilitate our investigation as much as possible, and what is more important still, to render its results more trustworthy, we must everywhere strive to place our problem *in the simplest circumstances possible.* Our object, therefore, must be to observe the process or influence in question, under circumstances (whether we may find such in nature, or be able to produce them artificially) in which its character is not only sufficiently distinct, but also in which it is least mixed up with and disguised by other processes, or influences. Thus we may ascertain what occurs in respiration *ceteris paribus* more easily in any animal in proportion to the simplicity of its organization, and of its respiratory apparatus, and especially of the process of respiration in it; in like manner, we should recognize the occurrences in inflammation more easily in a simple tissue than in a very complex one. So certainly as an anatomy, physiology, or chemistry of the human body pre-supposes that of the more simple organisms, and even of plants, it is no less certain that we shall never investigate the diseases of man with complete success, or hope to arrive at a clearer comprehension of them, until we have first investigated, and become familiar with the more or less analogous processes, or derangements, in the former, under the simplest possible circumstances. In like manner the effects of a simple substance, *e. g.*, of an alkaloid such as morphia, or quinine, are more easily determined than the mode of operation of their complex combinations with other substances, as in opium or cinchona, unless the accessory elements are absolutely inert. Our comprehension of the influence of a remedy upon the course, cure, or termination of a disease, would also be rendered more easy if it only had been employed in a large number of cases, than if several other remedies and influences had been in operation at the same time. If we judiciously selected a number of sick persons, and left them entirely to nature, or at least refrained from the administration of any medicines, and if it was observed that those persons recovered as certainly and rapidly as after the employment of those remedies, or the reverse, we should always know more of their influence upon the cure than we do at present.

Our first attention is in all cases due to the investigation of those processes, or influences, which are least dependent upon others, and least variable and accidental, to those, in short, which we have already recognized as the determining, or so-called primary, essential, and most constant ones. Thereby alone can we escape the danger of spending our time and labour upon things the investigation of which is difficult on account of their manifold variations, and unprofitable on account of the comparative insignificance of the results with which they are concerned. To understand the process of respiration it was, for instance, more important first to ascertain the office of the gases inspired, and of the interchange between the blood and the air, than the possible influence of the respiratory movements in themselves; and it would be more important to first determine the office of the oxygen of the air than that of its watery vapour. In like manner, to learn the causal connection and conformity to fixed laws in "inflammation," we should rather investigate the phenomena of increased heat, of the circulation and composition of the blood, and of various processes of excretion, in relation to their conditions, than those of pain or of digestion. To ascertain the causal connection of the change in the composition of the blood in inflammation, we should first direct our attention to one of the most constant phenomena in it, viz., buffing. We should have to investigate in what this phenomenon consists, of what materials the crust is composed, and in what these materials differ from the albuminous components of normal blood. We must further seek for other more or less analogous changes or conversions of the albuminous materials, &c., and examine the circumstances comparatively under which the latter as well as the buffing occur. We might thus, perhaps, succeed, not only in ascertaining the conditions of the buffing itself, but also of all the other more constant processes and changes in inflammation by the co-operation of which the peculiar composition of the blood, and the changes in its albuminous materials, may have been produced. We might even deduce from these established laws and conditions of the most essential processes in inflammation those of the latter process itself, and thus explain "inflammation" as a whole. The same holds good of "fever." If we only knew that in fever some process, or circumstance, such as increased temperature of the body, is a chief phase, perhaps the condition, of many other, or even of all the phenomena, and processes in it, we should have, first of all, to investigate that increase of temperature. We should have to ascertain its action and variations, and the circumstances under which we observe them; further their probable immediate or remote dependence upon certain other processes in the body, and upon various agents of the external world, and finally the influence of that increased temperature itself upon the other processes in fever patients.

To all this we may add that we must always commence with the investigation of those individual phenomena and influences, with those elements and processes in a complex whole (*e. g.*, in a function, or in a disease), *which are comparatively the most simple and accessible, or with whose conditions and effects we were already, perhaps, most familiar from other sources.*

The simpler such a phenomenon or process is, and the simpler and more direct may be its causes in their influence and mode of operation, the sooner are we able to draw conclusions concerning the conditions of the former from the mere fact of their occurrence, and from their modifications under certain circumstances. This applies to wounds, inflammation, and other events resulting from mechanical injuries and similar direct lesions, and to the effects of poisons.

Of infinitely more importance to us here, however, is the long-established fact, that in most, indeed in all the functions of the living body, certain phenomena and processes are included, which commence and run their course in accordance with simple physico-mechanical and chemical laws, and which appear to play a more or less important part in its whole mechanism. This may be observed in all the occurrences in the living body and its processes which depend upon pressure and gravitation, upon density or elasticity, upon adhesion, capillarity, or porosity (*e. g.*, various processes in the circulation, imbibition or absorption, excretion of fluids and gases, &c.); the same thing applies to the generation of heat and electricity, and to their laws. Chemical action occurs chiefly in the processes of digestion and respiration, in the development of the components of the blood, and of the body in general, from the materials supplied by our food, and in the gradual conversion and final excretion of the converted or decomposed materials of the body by the lungs, skin, kidneys, liver, &c.,—in short, in the whole process of nutrition, in the whole renewal and conversion of tissues within the body. From this it follows naturally that certain changes, certain deviations of these chemico-physical processes, from the normal state, will occur in all sick persons, without exception, and will exert a greater or less degree of influence according to the individual disease; this is especially the case in the phenomena of fever and inflammation, therefore in all acute diseases,—in diarrhœa, indigestion, dysentery, cholera, dropsy, gout, in the formation of gravel and calculus, and in all cachectic conditions.

Wherever such physico-mechanical and chemical processes, or agents, are in operation, we must, first of all, apply ourselves to a detailed investigation of them by the aid of experiment, and according to the exact rules of natural science. Mention has already been made of the forces which act in the circulation. Thus, in considering the process of absorption, we must ascertain the laws and conditions of simple imbibition, of endosmose and exosmose, and of the motion in the absorbent vessels of the materials taken up by them. If we wish to investigate, for instance, all the occurrences in inflammation, fever, &c., to learn in what they consist, and what may be their conditions, we must not commence with their more enigmatical and complicated elements, or processes, such as the phenomena of innervation; neither must we take up any given function as a whole. Here, as elsewhere, we must primarily confine ourselves to the investigation of the simplest processes—*e. g.*, the physico-chemical—and endeavour to determine their conformity and conditions in the case before us; that is to say, the processes and circumstances in the living body upon which they depend, by which they are modified in various ways, and upon which they themselves react. Having done this, we may the more successfully investigate other phenomena and processes—*e. g.*, those of the nervous system. What has been said will also apply to our investigation of the individual external influences and agencies, to light, heat, the air, water, to all the solid and fluid materials of our food, and to our remedies, drugs, and poisons. We must first of all ascertain, not only the properties and modes of operation of such of their elements and agents as are most simple and accessible for investigation, but also such of their effects in the living body as commence and go on in accordance with simple chemical and physical laws. If we wish to ascertain, for instance, the effects of morphia in the living body, and especially upon its nervous system, to know in what these effects consist, and how they are produced, we must not attempt to understand them by way of direct investigation, but we must first ascertain what changes morphia may produce in various chemical processes in the body, and then trace its effects upon the circulation, temperature of the body, and various processes of excretion, &c. We may thus hope better to understand its effects upon sensation and muscular contraction.

All this makes it evident that our efforts are insufficient to explain either the aggregate of these effects in the living body, or in a function, or a disease, as a whole, because all these physico-chemical processes are, in the living body, connected with many others; for the same reason, their conditions and laws cannot here be so simple as in inanimate nature. If, however, the conditions and laws of this species of phenomena and processes in the living body were once established, we should, notwithstanding what has just been said, have acquired a firm starting-point and solid basis for our future comprehension of the aggregates of those functions and diseased states, and of the influences of the external world upon them. Were we able to test, in either case, how far certain processes and elements of such aggregates can be deduced from those physico-chemical processes and their laws, we should doubtless acquire important information concerning those aggregates themselves, and much in them, of which we have at present no fixed idea, might be found capable of explanation. In short, it is only by such an investigation of the minutest individual phenomena and processes, and especially of such among them as are simplest and most accessible, that we can hope to acquire a clearer insight into the connection and conditions of the whole. Experience has sufficiently taught us, that men have for centuries gone on regarding a function, a disease, or its cure, as a whole, and observing its occurrence, and the modifications resulting from various special circumstances, without learning anything positive of its peculiar course and mechanism, or of its conditions and conformity to fixed laws.

Instead, therefore, of wearying ourselves by the investigation of those great aggregates of effects and influences as such—*i. e.*, as we meet with them in nature—and seeking in vain for the conditions and laws of their development and action as such, we must first endeavour to discover, on the one hand, the conditions of each individual process, or effect, and, on the other, the properties and mode of operation of each individual influence, or determining circumstance. Such detailed investigations in physiology and in the whole science of medicine have an infinitely greater value than whole compendia, or the most beautiful and comprehensive theories. He who has, for instance, investigated the variations in the weight of the body, and ascertained the amount of the excretions in a healthy or sick person, or the single phenomenon of the temperature of the body, in fever or inflammation, with all its oscillations and relations to other phenomena and processes—*e. g.*, to the circulation, to respiration, and to various processes of excretion—will doubtless have done more real service to medical science than one who has brought forward the most ingenious views of the aggregates of those diseases, or functions, and perhaps met with a larger share of applause. And if the above method is insufficient for the discovery of all the laws, still more of the essential causes of the processes with which we are concerned, our hopes are not disappointed, for they did not lead us so far. We shall be satisfied with having discovered much that is important and instructive, and our advance will, in the course of time, be facilitated, where we have, perhaps, little expected it. On the other hand, all our more direct attempts to fathom the conditions and conformity of our processes may lead to hypotheses, but not to a clear comprehension of them. The great object everywhere in the investigation of nature, and especially in the medical department, is to discover, not the reasons, but the mode, not the essential and ultimate causes, which lie far beyond our reach, but rather the manner in which our phenomena and processes commence and run their course, and all the circumstances under which they constantly occur simultaneously and consecutively.

We have already spoken, when upon the subject of the inductive process, of the assistance which is to be obtained in these investigations both from observation and experiment, under certain given circumstances, and from the employment of analogies and statistics, and hereafter we shall have more to bring forward on this subject. The mode of their employment, their importance, and their services in themselves, are the same in the one process as in the other. Inasmuch, however, as the circumstances under which we employ them here are simpler and clearer than in the investigation of complicated objects upon a mere empirical basis, the results obtained by us will be infinitely more certain and more conducive to a full understanding of our subject by following the deductive rather than the inductive method.

Were we to assume that the conditions and laws of all the elementary processes and effects in the living body and the laws of operation of all the individual agents or influences which affect them, had been demonstrated with as much exactness as has already been attained to in chemistry, physics, mechanics, and astronomy, we could then deduce, or explain from them the conditions and conformity of our most complex processes, and the effects of our most complex influences. This construction or deduction is, in itself, so little difficult as to be a matter of simple calculation.

Thus the physicist can calculate the fall of a given body with mathematical precision from the known laws of gravity or of the earth's attraction, and from the laws of atmospheric pressure and of the density of the atmosphere. The range of a gun, and the path and velocity of a ball discharged from it, can be deduced from the established expansive force of the gases developed by the gunpowder, from the weight of the missile, from the density and consequent resistance of the air, and from the angle of elevation, &c. In like manner the chemist can calculate from the known properties and laws of his simple inorganic substances, the behaviour and conformity of their combinations with each other—*e. g.*, as acids, bases, salts, &c.—unless, in consequence of their combination, substances and compounds are formed with entirely new properties and with new laws of action, as is sometimes the case with organic compounds. We do not aim at accomplishing anything similar to this in the study of the vital processes, nor can we attempt, with even an approximate degree of precision, to deduce from the established laws of their individual agents their development as a whole and the conformity of their origin and course.

Had we once ascertained, for instance, the conditions and laws of all the individual changes which take place in venous blood, in its passage through the lungs, (*e. g.*, the changes of certain of its component parts, the egress of certain gases, and the changes in its colour, temperature, density, &c.)—in short, if we knew all that, on the accession of atmospheric air, occurs in the lungs and in the blood contained in them, and why the components of that air—*e. g.*, the oxygen—produce those changes in the venous blood, we could thence deduce with certainty the conformity of respiration as a whole, and should thus be able to explain the whole process. It is well known, however, that the latter attempt has been made repeatedly before the former investigations had been carried out comprehensively enough and their results confirmed.

The reader is doubtless acquainted with Liebig's ingenious attempt to deduce this complex vital process and several others from certain chemical processes, or agents in them, and from the conditions and laws of the latter—*i. e.*, from the conditions and chemico-physiological links in the absorption of oxygen into the blood, on the one hand, and in the excretion of carbonic acid from it, on the other. Liebig set out from the known property of protoxide of iron to combine, when air and water are present, with a larger amount of oxygen to form the hydrated red peroxide; another fact from which he started is the property of this hydrated peroxide, when in contact with organic substances, to part readily with some of its oxygen, and thus become reconverted into the protoxide. Many of these circumstances appear to be present for the iron in the blood. The oxygen inspired may combine with the iron of the dark venous blood and effect a higher degree of oxidation in it, whence Liebig explains the conversion of the dark venous into the brighter arterial blood. The hydrated peroxide of iron, on the other hand, in its further circulation through the body, comes into contact, in the capillaries, with compounds and substances rich in carbon, resulting from the metamorphosis and conversion of tissues, and from the entrance of bile into the blood, &c. It then partly yields its oxygen to these, and especially to the carbon in them, and is thereby again reduced to the lower degree of oxidation, while the carbon of those organic substances is converted into carbonic acid by the free oxygen which combines with part of the protoxide of iron in the blood. In the lungs, however—where it comes into contact with the newly absorbed oxygen and with the water of the blood—it again yields up its carbonic acid, which is expired, and the protoxide, thus set free, again combines with the oxygen inspired, and with water, to form the hydrated peroxide.

It is also well known how Liebig sought to explain the generation of animal heat by the conversion of iron, or its protoxide, into the peroxide on the one hand, and on the other, by the conversion of the carbon of those organic compounds in the tissues and organs into carbonic acid by this process of combustion, or oxidation. It is not necessary here to examine further into the basis and validity of this attempt at explanation. It serves, in any case, as a good example of an endeavour to deduce the still unknown conditions or laws of an entire complex process from the conditions and laws of certain simpler and better-known processes and agents—an attempt which we shall recognize more or less in every similar theory in medical science.

We are enabled further to explain by Graham's well-known law of the diffusion of gases a purely physical process, but one of the highest importance in respiration, viz., the exchange of gases between the blood in the lungs and the air inspired. By this law a space already filled with any gas nevertheless takes up very rapidly any other gas not previously present, and this occurs also where two spaces are separated by an animal membrane. By this law we can explain the previously enigmatical facts that the entrance of oxygen into the blood in the lungs and the excretion of carbonic acid from it occur so rapidly through the walls of the capillary vessels and cells of the lungs, and in spite of them. We understand at the same time how another circumstance, which appeared at first sight calculated to prevent the entrance of new gases into the cells of the lungs during respiration, viz., their being already full of another gas, by no means produces this effect. This law of diffusion thus explains to us a portion of the whole process of respiration—*i. e.*, a physical process in it, as well as certain chemical processes already mentioned. We can further deduce the exchange of certain fluids between the blood and the parenchyma of the lungs from the known laws of imbibition, or of endosmose and exosmose. Those chemical processes in respiration and their established conformity to fixed laws might also aid us in explaining why the above-mentioned exchange of gases between the blood and the atmosphere takes place in the lungs only, why the oxygen inspired does not penetrate the whole body with the same rapidity, and why, on the other hand, the carbonic acid of the venous blood is not excreted, as well from all the other organs, cavities, and parts of the body, as from the lungs. We should know that it is in the lungs only that the oxygen inspired, immediately upon its entrance into the venous blood in them, meets with a body, viz., iron, with which it enters into direct combination, just as, on the other hand, the carbonic acid formed in the capillaries comes in contact, at the moment of its development, with the protoxide of iron with which it combines in the nascent state.

The above may suffice to show why we must, first of all, become acquainted with the individual parts of each function, or disease, in relation to their conditions, and to the conformity of their development, as well as with the laws of operation of all the individual influences and circumstances, by the co-operation of which they arise and run their course. Were we once familiar with each of these individual laws of development and operation, we might deduce from them the laws according to which a function, as a whole, and a disease, or even life, as a whole, commence and run their course. And this would sufficiently explain why such is their precise nature, and why they commence and go on precisely in this and in no other manner. We might even determine what effects must be produced by a given combination of influences and agents in the living body, and be able to point out by what combination of conditions and circumstances a given effect, or occurrence in the living body, had been produced in the case in question. We could equally predict what effects a given combination of influences and circumstances would produce in a given case.

Unfortunately, however, we are not yet so far advanced in medical science. We do not as yet even know all the individual elements which constitute the functions and processes in the living body, in health and disease, any more than the individual active circumstances and agents of the influences which affect them from without. Much less have we as yet succeeded in demonstrating the conformity and causal connection of such individual occurrences, or effects, and of the individual circumstances which modify them; and this applies even to those which act according to the comparatively simpler chemico-physical laws. For these reasons we cannot yet explain anything scientifically, and can scarcely assert anything positively, while every attempt at explanation is met by doubts, and allusion is made to contradictory facts or experiences, to accident and exceptions. So long as we are unable to determine the conformity of our vital processes, and of the influences which affect them, we cannot speak of a scientifically correct physiology, or of a rational theory of disease, and of therapeutics. Such might, indeed, in many cases already exist, but the true causal connection of our phenomena and processes, and the laws of operation of our influences and agents have nowhere been clearly demonstrated, and our clinical practice is not based upon a clear scientific comprehension, but rather upon simple, more or less uncertain experience.

Enough has already been said of the difficulties which our subject presents at every step. We have no alternative but to take it as we find it; we must not treat the problem it offers us either too partially, or too narrowly, we must not regard it either as too easy, or too difficult. Men must content themselves with the knowledge of what is comprehensible in nature; and, if they do not understand all that they wish to understand, they must remember that Nature, being such as she is, has evidently not been designed with a view to their fully understanding her. But if it is our earnest endeavour to discover, amongst the mass of phenomena and circumstances, of effects and causes which we observe simultaneously or consecutively, just those which stand in a causal connection with each other, we shall—if we keep our object steadily in view—find more and more the means which, approximately at least, will enable us to do so. Numerous as may be the effects of each influence or cause, and numerous as may be the causes each occurrence or effect may have, we shall, in the end, be able to discover them. The main point, however, will be, not only that we should observe and investigate consistently, but that we should, first of all, discover and select what is most fitted for investigation. This choice will, of course, vary according to time and circumstances, and upon it the whole success of an individual will frequently depend.

If we had already investigated nature to the full extent rendered practicable by the auxiliary means which we possess, we should have attained a very different status of knowledge and practice to our present one. This remark applies much more to the study of the vital phenomena in disease than in health. Our present knowledge of diseased conditions, however, is but a small part of what has always been accessible enough to simple experience. Thus we have ascertained, for instance, next to nothing of the diseases of hot and cold climates, we know little of the various classes of animals down to the simplest Mollusca and Polypes, or of the majority of the analogous processes and conditions in plants, although the nature and internal connection of the diseased conditions in these the simplest organisms, would doubtless be much more easily recognized in them than in such as are higher and more complicated. Even in man these phenomena and processes have not been determined to the extent attainable by the simplest observation, and, least of all, in disease. We should have much to do if we sought to investigate more exactly the phenomena of temperature and electricity, of the circulation, of imbibition, or of endosmose and exosmose,—*i. e.*, all that which could be ascertained by a more extensive employment of the thermometer and scales, or of our auxiliary chemical means, in the healthy and in animals, as well as in the sick.

Our whole future prospects are essentially based, as has already been explained, upon the attempt to determine separately, first of all, the conditions and effects of what is most simple and accessible; we may then proceed to what is more complex, and investigate it under the simplest circumstances possible. We cannot, it is true, acquire by our investigations any direct knowledge of the chemical processes within the living body. We might, however, investigate the ingesta and egesta with sufficient exactness to enable us to draw conclusions, by the aid of analogies, and by comparison with other more or less similar changes in organic substances, concerning the occurrences and chemical processes in the animal economy, with at least much greater probability than we can do so long as we have not investigated all that is accessible. Whenever anything important is discovered in the department of physiology, whether in the physico-chemical processes of our economy, or in the nervous system, &c., we have long been accustomed to inquire whether it may further our comprehension of the origin and course of certain diseased conditions, and of their cure. Here, as elsewhere, we gain, by each step that we take in the analysis and investigation of the vital processes, some knowledge in entirely different and often apparently unconnected paths. With every new discovery, however remotely it may be connected with the comprehensive questions of medical science, we gain a wider horizon and new points of view, perhaps even clearer and more definite starting points. We receive at once thereby an impulse to further investigation in entirely new directions, and new means of investigation are afforded us such as would never have occurred to our minds from the mere observation of the whole,—*i. e.*, of the aggregates of our phenomena as such.

Thus the knowledge of those palpable changes in our patients, which pathological anatomy affords us, has not, in itself, essentially furthered our comprehension of the diseases themselves, and of their causal connection; we have often only had new questions and problems presented to us—*e. g.*, the conditions of a given lesion, of an inflammation, of exudations, of the processes of softening, or atrophy, &c. These pathological phenomena have, nevertheless, done us infinite service by affording us visible, tangible, and more definite states for further investigation than our former symptomatic outlines of disease—states and changes which we can frequently produce at discretion. They afford us, as it were, a handle by which we can grasp, and investigate more closely the diseased processes in question. A clearer insight into the chemico-physical processes of the body, in health or disease, and into the part they play in its whole mechanism, would render us very much the same service. It would enable us to undertake the investigation of the latter in a comparatively more simple and exact manner. It is not necessary here to allude more explicitly to the increased knowledge of the vital phenomena which we have acquired by the aid of chemistry and physics, especially in the most recent times, or to the new views and impulse they have communicated to further investigations.

If we are precluded from attaining our object so easily and so directly as it is possible for the physicist and the chemist to do in the case of at least very many of their phenomena, no insuperable impediment exists to our progressive approach to it. If we cannot comprehend directly all our complex aggregates of phenomena, causes, and effects, we must come at them as well as we can indirectly, whether by a more extensive observation of them, as they are met with in nature, or by a skilful examination of them, and an express arrangement and variation of the circumstances—in short, by experiment. A science whose practical object is the welfare of mankind, and which can only fulfil that requirement in the ratio of its own progress, must not spare this labour. Even the physicist was not able to ascertain directly that our earth revolves about its axis; we even know that physicists have been sacrificed on the ground of this fact being opposed to a dogma of the church to which they belonged. It has only been by numerous by-paths, by many indirect proofs, that this fact has been established; it was shown, for instance, that the revolution of the sun, and of the whole vault of heaven, with all the heavenly bodies, around the earth, is only apparent; that the earth is flattened at the poles; and this view was confirmed by the discovery of the laws of gravity, and of the so-called force of attraction and centrifugal force. Centuries of zealous and careful investigation in astronomy and physics were required for the establishment of even these indirect proofs. It is very possible that with all our exertions, we shall attain nothing more than an approximately clear insight into the causal connection and conformity of our most simple phenomena and processes, and that we must content ourselves, for a long time to come, with individual fragments and instalments of the whole. It depends upon ourselves, however, whether other and better knowledge is to follow. It will and must follow if we earnestly seek it, and we shall frequently acquire further information and clearer views from entirely unexpected sources, whether it be from discoveries in other departments of natural science, or from the successful attempts of investigators in our own.

Yet we can scarcely hope with all this to attain to the same degree of precise knowledge as has been acquired in chemistry, physics, and mechanics. Such is the nature of the objects of our investigation, so closely are all the phenomena and processes in the living body connected with each other, and with those of the external world, so varied is the order of their occurrence, that we shall perhaps be always restricted to a calculation of probabilities. The highest point we can hope to attain may be predicted to consist in being able to give an increased degree of certainty to this calculation of probabilities, by the further discovery of laws, and certain constant relations of the vital phenomena and processes to each other, and to external nature. At present, as every one knows, any such calculation is but very vague. We know but a small part of all the influences and agencies which constitute a given function, or which affect the living organism in a given case of disease. And even in those which are more familiar to us, we know but little of their laws, and of the definite manner in which they affect certain processes in the body. We may, therefore, easily form a false estimate of highly important conditions and phases, or possibly overlook them altogether, and, under the most favorable circumstances, we can acquire notions only approximately correct of those most familiar to us. How then can we hope, when our knowledge, even of the comparatively simplest things, continues confessedly so small, and our comprehension of the conditions and connection of our individual elementary processes and influences remains so imperfect, to deduce from their laws, with even an approximate degree of precision, the conformity of the most complicated processes and influences?

Every theory which seeks to explain any vital function, or disease, or the operation of any hygienic influence, or of any remedy in the living body, or the whole cure of any disease, shows that this attempt has been ventured upon at all periods, and continues to be ventured upon at the present time, as well as that, in a certain sense, it is perfectly justifiable. Every such theory, or explanation is, in fact, a species of comprehensive proposition, or attempt at generalization. We attempt to explain by it a whole series, or aggregate of processes, effects, and causes, while we do not clearly understand, and are unable to explain an individual process, the production of an individual effect, or the mode of operation of an individual determining circumstance! From an innate feeling of necessity, instinctively almost, every one deduces the conditions, and real or supposed laws of the individual phenomena and processes with which he has to deal, as well as the rules of his art, from certain general propositions and views. Every one, for instance, who attempts to explain the conformity to fixed laws, and the internal connection of a function, deduces them from his general notions of life, and of the action and conditions of its individual processes, whatever the amount of his knowledge of them may be. The pathologist, for instance, deduces the production and conditions of a so-called dyscrasy, or of an idiopathic affection of the nervous system, in the former case from his notions concerning the formation and composition of the blood, and its influence upon nutrition, the processes of excretion, and the conversion of tissues; in the latter case, from his general notions with regard to innervation, and from the views he may have formed concerning the importance and mode of operation of the central organs, and of the so-called centripetal and centrifugal portions of the nervous system, &c. In short, we choose our remedies and rules of diet, and explain their effects, according to our previous views of health, or of a given disease, and according to our estimate of the influence, properties, and mode of operation of certain hygienic agents, or of medicines generally.

Several examples of this have already been given. A further illustration of it is afforded by a disease which has long attracted the attention, not only of the physician, but also of the chemist, viz., *diabetes mellitus*. The numerous attempts which have been made to explain all the conditions and derangements in it, and especially the peculiar changes in the urine of patients affected by it, in relation to its composition and quantity, are also well known. Some modern theorists, Bouchardat and others, have founded their explanation upon the fact that starch and analogous substances are converted into sugar by the action of diastase, yeast, and all the substances which belong to the albuminous group. From this they have inferred that a similar conversion takes place in the stomachs of such patients in an abnormal manner, and to an unusual extent, and attempted to explain thereby the presence of sugar in their urine. Farther, because alkalies and alkaline earths appeared to be of service for a time in diabetic patients,— *i. e.*, because the sugar in their urine was diminished in quantity, or perhaps disappeared for some time, they inferred that in consequence of their administration, the conversion of starchy matters, &c., into sugar was prevented; and that, on the other hand, their presence in the blood facilitated the conversion and oxidation, &c., of the sugar in it. In his further observation of such patients, Bouchardat remarked that the temperature of their bodies was reduced 2—3°, and deduced from this circumstance a further confirmation of his theory that the disease depended upon a diminished intensity of the process of oxidation or combustion within the body. To facilitate this process by artificial means, he caused his patients to inspire oxygen, and asserts that he observed a diminution, and even the absence of sugar in their urine. What he really observed could, however, at best have been the diminution or disappearance of the sugar during and after the employment of those means, but not in consequence of their use and influence; for neither Bouchardat nor any one else has yet succeeded in demonstrating the accuracy of the whole theory, and therewith the conditions and causal connection of the development and cure of the disease.

This example is also useful in showing us that we cannot so far safely rely upon any theory, upon any single deduction, or explanation in our whole science. None of them have stood the test of time; they have all yielded to our advanced knowledge, and even, in many cases, to the results of simple experience; and the reasons why this could not have been otherwise, may easily be gathered from the foregoing pages.

This very disadvantage is, however, in certain respects an advantage. In our dilemma as to whether or not we may believe in the accuracy of any given theory, we receive a certain support from the very quarter in which all our attempts at explanation have been frustrated,—*i. e.*, from simple experience itself. It is, after all, only the observation of the actual state of things—in short experience—which can afford us the means of testing our various deductions and attempts at explanation. Those aggregates of phenomena or processes, of effects and causes, which we meet with in nature, and the investigation and explanation of which present so many difficulties, and destroy so many illusions, afford us in their actual condition, and as they may be established by every observer, the highly-important aid, of serving, to a certain extent, as infallible tests of the accuracy of the boldest and most ingenious theories and deductions. We always can, and therefore must, by means of such observation, make a kind of counter-test, and note whether, and to what extent, we have calculated correctly, or falsely, in our interpretations and in our deductions of the conformity and causal connection of our processes. "Demonstratio longè optima est experientia," as Bacon correctly observes; and the same thing is expressed by the old saying, "experience is the test of all theories;" not, however, the keystone as so many have believed.

In other words, therefore:

The laws we have deduced, i. e., *all our attempts to explain the aggregates of our complex processes, or influences, from the conditions, or laws of operation of their individual agents, can only lay claim to validity and be relied upon, in so far as they agree with the actual state of things, and in so far as the deductions, or conclusions, to which they lead us in a given case coincide with what we observe and meet with in reality.*

To return to the point from which we started in our investigations, our object is, not merely to ascertain and still less to assert that certain phenomena, certain processes or circumstances may present themselves simultaneously or consecutively, or that certain causes, or influences, might affect them in a given manner, but rather that they in reality always present themselves so and not otherwise, and exist in this constant causal connection with each other. Our problem remains, to test whether and how far they really so occur together, or follow each other in the constant manner which accords with our theories of them, and with the laws which we have advanced, or inferred. Even astronomers, after having ascertained through their calculations the laws by which a heavenly body moves, have found themselves compelled to test the accuracy of these calculations by further observation of the various positions it successively assumed in the firmament. Thus Newton and others have tested and ascertained, in the case of the heavenly bodies, the accuracy of the laws of motion, or gravitation already discovered by Keppler. In other words, it was Newton and some of his contemporaries who first succeeded in demonstrating that the motions of our planets around the sun could not take place in the manner in which experience has shown them to do, *i. e.,*—in an ellipse,—if they were not attracted by the sun in exact accordance with Keppler's laws of gravitation. What Keppler had calculated mathematically, Newton and others have demonstrated by the aid of experience and exact observation.

On the other hand, all our theories and very nearly all our explanations of the causal connection and conformity of the vital processes in health and disease have succumbed to the test of further experience and more exact observations. They have died out, one by one, and will continue to do so until the requirements in our science, which have already so often been mentioned, have been fulfilled, and what lies at least next at hand, —*i. e.*, the conformity of our individual processes,—has been clearly demonstrated. Thus, neither the theory of the formation of sugar in diabetic patients mentioned above, nor any other, has been able to withstand the test of increased experience and clinical and experimental investigation; and whether the same sugar had subsequently been observed to be produced under circumstances, or had been found in parts, fluids, &c., of the body in which, according to that theory, no sugar should have been formed; or whether no sugar had been found where it ought to have been present, the theory would equally have been a false one. The simple fact of the continuance of the disease, and of the presence of sugar in the urine, and the death of such patients proved that alkalies, earths, oxygen, &c., failed to effect what, in accordance with the assumptions, or postulates of that theory, they ought to have effected. No less was the well-known theory of Broussais, that almost all diseases depended upon an irritation, or inflammation of the stomach and intestines, soon refuted by the experience that no such derangement was discovered in the generality of patients, at least not as a primary, or essential element of their disease, and that the development, course, and cure of most diseases, and the mode of operation and success of our remedies, rather contradicted, than agreed with the theory. And any one who has attempted to deduce, in every case, the phenomena of apoplexy and the hemiplegia, &c., in it, from an extravasation of blood into the brain, or from other palpable changes in that organ—*i. e.*, to explain them thereby—will no less certainly have found himself contradicted by further and more exact observation.

It is also evident that we cannot rely upon any attempt to explain either life itself, or any of its functions as a whole, or depend upon any theory of the development, conditions, and nature of a disease, or of the mode in which it may be acted upon either by a given remedy, or by any of the various agents of the external world, simply because such theories appear sufficiently correct and plausible to ourselves, or others: their accuracy is only to be confided in when they have been confirmed by experience, and by the determination of the exact state of things. The fact that all theories of this kind in medical science have been refuted rather than confirmed by more extensive and exact experience, or that at least their partial and confined character has always become evident, serves to explain that distrust of them which seems rather to increase than to diminish in the minds of candid investigators. Hence follows the necessity, so long justly insisted upon by the empirics in relation to all theory, of every where observing attentively, as frequently as possible, and in all its details, the obvious occurrences of nature, and the tangible results of our experiments, with the mode and order of their occurrence, and the circumstances under which they usually occur; and of comparing carefully, in as many instances as possible, the cases which agree in their more important points, and of observing and investigating the same process, the same event, or the same influence under the most various circumstances possible. In other words, we must always, in our science, cleave to simple experience and mere empirical facts, or laws, and must therefore, above all, seek to discover more and more of them by the employment of all the auxiliary means which have already been alluded to in speaking of the process of induction (see Section III).

This simple empirical establishment of certain facts must, however, by no means be regarded as the whole problem before us; it is not the last, but rather the first, preparatory step. We must, at last, ascertain the causal connection and conformity to definite laws—in short, the theory—of the things which we have established empirically, of all the phenomena, processes, and circumstances which we have observed to coexist or follow each other. But since our knowledge of the latter, for the reasons often mentioned already, has hitherto amounted to little more than a calculation of probabilities, and will doubtlessly continue, more or less, to be so, we must test, as frequently as possible, whether our experience, and the actual state of things agree with our conclusions, or abstractions, and with the kind of conformity which we have deduced and constructed for our phenomena and processes, or whether our conclusions and attempts at explanation, our notions of their causal connection, condition, or operation, have been too narrow and partial, or even altogether erroneous.

If in a given case, for instance, the phenomena, effects, and events, which ought to have occurred according to our theory, according to our views of their causes, and of the mode of operation, and importance of those causes under the given circumstances, either fail to occur at all, or do not occur in the manner to be expected, our theory—*i. e.*, our previous deduction of their causal connection and of the laws of operation of all the influences which affect them—can be of little or no value. For although the circumstances and influences which we may have regarded as the causes of the effect in question, and the mode of operation and laws of which we believed ourselves to have demonstrated, may possibly have existed and operated, the effect or event in question (*e. g.*, a certain process in the living body, a disease, or a cure) either does not occur at all, or at least not in the precise manner and order which our theory has led us to expect. We are thus obliged to confess that we have not correctly enough estimated, or comprehensively enough observed several, and perhaps important parts, or elements of the whole process—that we have perhaps overlooked certain causes of it, and the mode of operation and influence of certain determining circumstances in it. To speak more concisely, we have assumed certain causes, certain effects which either do not exist in reality, or not in the exact form and definite connection, and relation to each other which we have imagined.

Unfortunately, however, this correction of our theories and attempts at explanation by experience is infinitely more difficult in medical science than in physics and chemistry, because our experience and observations themselves are in a great measure fluctuating, and not to be relied upon, and especially because they admit of very different interpretations. A case, for instance, which appears to one person to be opposed to a given theory, or to demonstrate the inaccuracy of a given mode of explanation, may appear to another to be a further illustration in its favour. At all events the latter view would be justified if it could be demonstrated why, in any such case, the occurrence in question, a certain effect, &c., has not taken place at all, or not in the manner to be expected in accordance with our theory. We should have no reason, therefore, to doubt its correctness, if we had succeeded in ascertaining, and clearly demonstrating, the existence of other circumstances and influences, by the accession and interference of which the usual effect of the causes and circumstances in question may have been prevented, or in various ways modified. For we should then be in a position to explain satisfactorily this apparent contradiction of our theory; we could adduce valid reasons, founded upon experience, why, in the case in question, and under existing circumstances, the result was and must be different to the usual one. It is self-evident, however, that to give such an explanation at all, and to be able to adduce reasons perfectly consistent with truth, we must assume a knowledge of all the active and antagonistic phases, of the whole causal connection of the event in question, such as we can scarcely be said to possess anywhere in our science.

However all this may be, such a counter-test, or correction of our theories, or attempts at explanation, by experience, and by more comprehensive and penetrating investigation into the exact state of things is, as has already been explained, of the highest importance. It is, in fact, altogether indispensable in our science. If it does not answer, if the further ascertained facts and experiences are opposed to our previous view of the causal connection and conformity of certain phenomena and processes,—in short, to our theory,—our attention has, at least, been directed to the deficiencies in our previous observations and experiences, and at the same time drawn to certain sources of error in our accustomed explanation and deduction of those causes and laws. We are now called upon to observe and more closely investigate the things in question in other directions and under new relations, and to do this under the greatest possible variation of circumstances, through which we shall almost certainly attain a better knowledge of them, and a clearer insight into their conditions and laws, than we have previously possessed. The correction of our theories, &c., by experience, will thus become a powerful aid to our further progress, and if we only knew how to apply it correctly, we need never fear that our time and labour would be thrown away upon it.

After men had succeeded, for instance, as they believed, in determining the rapidity with which the waves of sound travelled, by exact experiments and by calculations founded upon them, it appeared that the real rapidity with which sound travels did not agree exactly with the theory of its communication. This incongruity between reality and theoretical calculation led physicists to further and more exact investigations, and finally to the recognition of a circumstance which had previously been overlooked, viz., to the development of heat in consequence of the condensation of the air, and, therefore, also by the waves of sound. It now became not only possible to explain the difference between the actual velocity of sound and that calculated theoretically, thus confirming the accuracy of the previous theory, but a clearer insight was at the same time acquired into an entirely different phenòmenon, —viz., into the development of heat by compression of the air, under new and hitherto unknown circumstances. Just so, by means of such correction by experience have many similar advantages been obtained towards perfecting our theories and attempts at explanation in physiology, as well as in the general field of the doctrines of disease and therapeutics. They have all, it is true, succumbed to this test, and possibly even those which in our own time enjoy the greatest credit will scarcely escape the same fate. Yet they have, none the less, in their time not only satisfied certain requirements, and thus been positively useful so long as their accuracy could be believed in, but the deficiencies afterwards discovered in them have stimulated us to further investigation, and the refutation of them has, indirectly at least, furthered our more perfect knowledge.

If, for instance, in the case mentioned above, all the theories with regard to the formation of sugar in diabetes have failed in their object; or if we cannot, like our predecessors, deduce gout, hemorrhoids, and the like, from imaginary obstructions, infarction, acrimony of the humours, &c., because, in the present state of our knowledge of the human body and its processes, we are aware that such cannot really occur, our insight into the fallacy of those attempts at explanation has led us to the further investigation of obscure diseased states, and of their possible conditions, or so-called nature. If we can no longer explain, as formerly, all the phenomena and processes in typhus and dysentery by an inflammation and by certain textural changes in the alimentary canal and in its glands, &c., this improved knowledge has imparted a stimulus to investigate entirely different processes and circumstances in such patients—*e. g.*, various processes of excretion, the composition of the blood and chemistry of the conversion of tissues, certain states of the circulation, of the pressure of the column of blood, of innervation, &c. And if further experience shows that we have but imperfectly investigated, and falsely estimated, such conditions and circumstances in those diseases; and that we can by no means yet deduce from the alleged laws of their operation, all the phenomena and processes in the diseases themselves, as aggregates, we shall, on that very account, seek to investigate more exactly the states and changes in them, and search for another causal connection.

Since experience has shown that intermittent fever and other so-called miasmatic diseases do not find their adequate cause in marsh-land and malaria, simply because they frequently occur without these, we have begun to seek diligently for other determining circumstances. Peculiar states of the temperature and changes in it, the rapid cooling of the body, a peculiar geognostic character of the soil, bad dwellings and imperfect tillage, unsuitable food and clothing, &c., came to be recognized as more or less probable causes. Our insight into the part which so many remedies and substitutes for quinine were previously supposed to play in the cure of intermittent fever, was rendered much clearer by our observing that recovery frequently followed without the employment of any of them, by the aid of good food and nursing, in healthy dwellings, &c. And if further experience should once clearly show, and physicians become convinced, that the great majority of our remedies have no higher value in other diseases than they have in intermittent fever, that perhaps our interference in almost all severe diseases can scarcely meet with any positive success, we shall have more cause to seek other aid; and the prevention of those diseases will become, more and more, the special object of our art.

Departments of human knowledge and practice in which, as in ours, most of the facts and experiences are unexplained, and for which no causal connection has been established, are precisely those which offer the honest investigator the widest field for discovery. If he bear in mind existing deficiencies and the improved mode of procedure which is so much called for, he will, at every attack upon the chaotic mass before him, meet with something to aid in the furtherance of his science and, in the end, of his art.

REDUCTION OF NUMEROUS APPARENTLY HETEROGENEOUS PHENOMENA AND PROCESSES TO ONE AND THE SAME ESSENTIAL PROCESS AND CAUSAL CONNECTION. RESOLUTION OF NUMEROUS COMPLEX LAWS INTO SIMPLER, OR MORE GENERAL AND HIGHER LAWS.

(Process of GENERALIZATION or REDUCTION.)

Special mention has been made in the foregoing pages of the requirements which we must fulfil for the attainment of a scientific and clearer insight into the processes and occurrences which the physician meets with in nature as complex aggregates. We have also endeavoured to explain by the aid of what operations and means this can alone be attained, viz., analysis and our deductions from it.

If we have ascertained what occurs in an individual process (*e. g.*, in a given function or disease, or in a given external influence), and if we have discovered, or demonstrated with approximate probability, its conditions, and the laws of its operation—in short, its causal connection and conformity to ascertained laws—we may rest satisfied in relation to this individual case, for we have succeeded in understanding and explaining it as far as we can do with regard to *it* individually. A further scientific want is, however, by no means so easily satisfied. We long to connect this individual process with others; we seek to bring this individual phenomenon, or process, into connection with all the others with which, as we know or surmise from our experience, it stands in a certain relation, or even causal connection. In the former method, we had, as it were, taken to pieces and examined separately those series of phenomena and processes as they were presented to us in the aggregate by the living body, in health or disease, and the whole mass of the influences and agents of the external world. It was our office to separate into its individual elements, as we best might, all that which we met with in nature, as a more or less complex whole, and to investigate separately its causes, or effects, before we could hope to acquire any insight into its causal connection and conformity.

But this very process disconnects the original living whole, which now lies before us in its isolated elements. We see at the first glance, and almost instinctively, that we cannot thereby even fully comprehend, or explain, the causal connection and operation of those individual elements themselves, much less their internal connection and co-operation within the living body as a whole, or in external nature, nor can we trace their mutual relations to each other and to the occurrences, or processes in the latter. A natural impulse to comprehend this whole now urges us to bring back those individual elements into which we had divided it into their proper connection with all the others which appear to us to be allied, or analogous. But what these allied things are we usually learn from the simple observation of Nature from which we started, and from experience,—*e. g.*, from the fact that we have always observed them to occur simultaneously, or consecutively, and from the circumstance that in the whole mass of phenomena, processes, &c., certain of them exhibit greater or less similarities, certain allied relations of some kind or other. Just as we recognize hundreds of individual plants or animals to belong to one and the same natural group, and set them down, so to speak, as the individual integral links of a higher ideal organism, or common type, by the resemblance in their external conformations, so also certain organs of the living body, and certain of its tissues, appear to us to belong essentially to one and the same group. For their external forms and their elementary parts, and the arrangement of these, as well generally as in detail, the mode of their development (*e. g.*, from cells) and their properties, &c., are found to agree in certain main points. In this sense, for instance, the formation of the vertebræ is repeated in the cranium of the Mammalia; and in the organs of the most dissimilar animals and plants we observe the same forms to recur as developments in accordance with the same essential type. And as, in external nature, many individual agents and processes co-operate to produce what we briefly denominate weather, seasons, and climate, so we find, or at least surmise, that certain functions, or processes in the living body are allied and stand in a certain relation to each other.

And the same circumstance which enabled us to recognize an individual function in the aggregate of the vital phenomena in a body, and to distinguish it from the others, now aids us further in ascertaining which of these functions and processes are allied to each other. On a closer examination of them it becomes evident that certain of them are only integral members or agents in one and the same process, and that they assist in producing, in the end, essentially the same result; some perhaps are concerned in the nutrition of the body and the maintenance of its material substratum; others, on the contrary, are connected with its finer and even intellectual relations to the things that surround it; others, again, with the propagation of the species.

In a somewhat similar manner we may recognize in many states of disease such an agreement in certain essential points —*e. g.*, not only in the aggregate of their more important and perhaps determining phenomena and processes, and in the manner in which they run their course, but also in their development and occurrence under various external circumstances, and in certain personal relations, so that nature herself might seem to have arranged them in one group.

In short, we everywhere meet with, or at least surmise, a certain resemblance and agreement between various processes and aggregates of functions in the living body, and the agents of the external world by which they are affected. If they differ from each other in individual points, the possibility furnished by analogy and experience leaves it open for us to suppose that any differences in these processes, &c., which we have regarded as allied, any contradictions and exceptions in them, may have depended upon the intervention or interference of other special circumstances and phases of more or less subordinate importance. And we are thus justified in believing that notwithstanding such differences and deviations from the others, they are still very probably allied, as parts of a more general, or higher whole.

We must also take into consideration a further and most important circumstance to which allusion has already been made, viz., that our fragmentary investigation of various parts has in some degree stood in the way of our clear comprehension of the conditions or effects of these parts themselves in their whole extent, just because they are only fragments, or individual parts of the whole. Under the most favorable circumstances we may have succeeded in acquiring an approximately correct knowledge of any given part, say a function, a diseased state, or the effects of an individual external influence, such as an article of food, or a remedy; in ascertaining, with more or less exactitude, the circumstances under which they occur, and in tracing their causal connection and conformity individually. But even these, when we reconsider them, do not appear clear and intelligible. On examining them more closely, it will appear that we cannot acquire any considerable increase of knowledge concerning them, so long as they remain in a state of isolation and separation from other things with which they are allied, or perhaps essentially connected. If, for instance, we had come near ascertaining what occurs in the action of an external agent, such as wind or rain, or in any one of the processes of the living body, such as respiration, or in any individual phenomenon in a patient (*e. g.*, diarrhœa, the formation of exudations, or an abnormal excretion by the skin, kidneys, &c.), and wish to learn by the co-operation of what circumstances it has occurred, it will soon become evident that we cannot clearly understand these individual occurrences, either in themselves, or in their conditions, without a previous knowledge of many other things.

Nor can the grounds of all this long remain hidden. An individual occurrence is soon seen to stand in a close mutual relation to, or even causal connection with, various others; its agreement with others in certain main points soon indicates the necessity of investigating and becoming acquainted with these also. In the one case it was necessary to understand the causal connection and conditions of various other processes in the atmosphere—*e. g.*, the formation of mist and dew, of snow and hail, the conditions and laws of the variations of temperature at different hours of the day and seasons of the year, and those of the air-currents generally,—in the other case the behaviour, conditions, and effects of other processes of ingestion and excretion, of various changes in the composition of the blood, of the circulation, and, lastly, of all the processes of endosmose and exosmose, &c. In like manner we find in any disease, taken as a whole—*e. g.*, in scrofula, dropsy, inflammation, gout—that it blends with others in various ways, that it is variously connected with other deranged and diseased states, and that certain changes and derangements in many of these diseases have, in essentials, the same ill-defined boundaries. Others again agree too obviously in relation to the circumstances and conditions of their development and course, and in the various phenomena they present, to admit of a doubt as to a certain similarity, or even essential identity—for example, the various fevers and other diseases in so-called miasmatic neighbourhoods, or in those which prevail in the tropics, or in unhealthy, overfilled hospitals, and under other similar circumstances. And the same essential likeness will often be perceived in the workings of external influences which are apparently dissimilar. In many substances, some used simply for food, some remedial, and others poisonous, a close observation of their properties and a deep investigation into their effects will bring to light an agreement among them in many essential particulars, in spite of the individual peculiarities in which their difference is apparent.

All this tends to urge us to ascertain the causal connection of things which are allied and analogous, and to trace the relations in which these more or less homogeneous phenomena and processes may stand to each other. We set to work accordingly to seek for a causal connection, for conditions and circumstances, which are, in all of them, the basis and producing cause; or in other words, we seek for a certain order of things, a conformity of their development and course, to which to refer all the processes and phenomena which, in certain essential points, co-operate, or agree with, each other.

Success in this would lead us on to the discovery of more general or higher laws—*i. e.*, laws from which we might deduce various phenomena and processes which had previously appeared more or less heterogeneous, but are now found capable of being explained upon kindred principles with these. It would also lead us to a realization of our foreboding that these, in their conditions and causal connection, in the conformity of their development and course, or operation, were agreed either in the main, or at least in several essential points, experience having already shown us their more or less intimate connection with each other, and made us aware that they all commence, run their course, or operate under essentially the same, or, at least, very analogous circumstances, while by this their co-existence and co-operation a common result is produced.

To ascertain and demonstrate all this we must proceed as we should do in the investigation of causal connection, conditions, and laws of operation in general natural science. We must form certain surmises and hypotheses concerning the individual phenomena and processes, and weigh the possibility of their possessing a definite kind of dependence and origin, or operation. All those series of phenomena, effects, or influences, which experience and our *a priori* notions induce us to class together, must be tested and compared with each other in the greatest possible number of cases and separated into their individual integral elements, and the conditions, or operation of these must be determined by means of the usual auxiliaries to investigation. We must endeavour to discover in all of them a certain activity, certain occurrences and circumstances, which we may regard as their common and essential elements, perhaps as the circumstances upon which they all equally depend, or as the active agents in them all. The solution of our problems, and the object of our investigation, are only to be attained by ascertaining those common circumstances and processes upon which those series of phenomena, effects, &c., assumed by us to co-operate, or to be allied by virtue of certain analogies, really depend, or by which they are essentially influenced. We must, therefore, in the case of all these, be able to demonstrate an agreement in certain essential points and even in the nature of their causal connection, conditions, and operation, and we shall thus have it in our power to deduce them all from essentially the same laws, or, in other words, to explain them in the same manner.

Thus it has long been known that chlorine bleaches—*i. e.*, destroys colouring matters—as well as that it removes bad smells, foul gases and compounds, &c. It was afterwards discovered that it has a great affinity for hydrogen and so-called bases in general, and, from the great intensity or energy with which it strives to enter into combination with them, it was surmised that its capability of destroying colouring matters, and of removing noxious smells, might depend upon this its property and mode of operation, so that when it was proved that hydrogen and other basic, electro-negative bodies were essential and constant components of those gases, &c., the accuracy of such a surmise was confirmed. The same circumstance being shown to be the essential and ultimate condition of various processes, what had previously appeared to be entirely heterogeneous effects and processes were reduced to one and the same cause, and brought under the same law.

When attention was more specially directed to the digestion of the individual organic elements and components of our food, one of its most interesting and enigmatical circumstances was noticed in the fact that coagulated albumen is gradually dissolved in the stomach and intestinal canal, and finally enters the blood and general economy. It was ascertained by experiment that neither water, nor any solution of the individual salts and other materials with which the albumen comes into contact in the alimentary canal, are capable of dissolving it. Thus, while we failed to recognize a solvent for coagulated albumen, the fact of its solution was apparent. We observe the analogous phenomenon in the circumstance that certain metallic salts—*e. g.*, corrosive sublimate and nitrate of silver—when employed internally or applied to blistered or suppurating surfaces, are, to all appearance, precipitated by the fluids in the stomach and upon the moist *cutis vera* when deprived of its epidermis. Deposits were observed to be formed which were not soluble in water, dilute acids, or other ordinary simple solvents. Nevertheless persons who have taken such metallic salts for a considerable time, or to whose skin they have been applied, experience their general, so-called constitutional effects—*e. g.*, salivation, in the one case, and the peculiar colour of the skin not unfrequently observed to follow the long-continued use of preparations of silver, in the other. But since we can no longer believe in purely sympathetic or dynamic effects of this kind, we cannot doubt the fact that those metallic compounds and deposits must in some unascertained way have been dissolved and absorbed. It next appeared that these metallic salts are by no means decomposed in the stomach and upon suppurating surfaces, as was at first inferred from the formation of those deposits, but that the latter rather consist in combinations of those metallic salts, as such, with certain elements of the fluids in the stomach and of the pus, &c., upon the skin; that they are, in fact, combinations of them with the albumen and chlorides of the latter. On further investigation it has appeared that the solution of these apparently insoluble substances—on the one hand the metallic compounds, on the other the coagulated albumen of our food in the stomach, &c.—are effected by one and the same process, or circumstance, viz., by their combination with the alkaline salts or chlorides and albumen of the gastric juice and other secretions, and of the pus, &c., upon the skin, to form so-called albuminates, and finally by the gradual solution of the latter in the excess of albumen in those fluids. And the same process, or circumstance which was thus ascertained to be the common condition of these apparently different effects, is now found sufficient to explain many other analogous facts and effects—*e. g.*, the solution and absorption of many other metallic compounds, of many other coagulated substances in the solids and fluids of our food.

Various mechanical and pharmaceutical means had long been employed as topical applications for arresting hemorrhages, and partly for preventing the further development of inflammation of external parts, tumours, &c., partly for dispersing, and effecting the absorption of, the latter. Thus, for instance, in hemorrhage, pressure, ligatures on arteries, cold, alum, substances containing tannin, &c., were employed—in the case of external inflammation also cold, mechanical pressure with tight bandages, &c. We have already succeeded in showing, with great semblance of probability, that in all these cases, means and processes, apparently very different, prove beneficial by producing the same result—in hemorrhage, for instance, by causing coagulation of the blood, and its albuminous constituents; in inflammation and tumours, by impeding, or preventing, the access of blood to the parts, and perhaps further by an interruption, or a certain modification of the processes of nutrition and conversion in them. Paralysis of the limbs has been observed to follow upon the most dissimilar influences and changes—*e. g.*, extravasation of blood into the brain, or spinal marrow, or softening of these, the pressure of tumours upon these central organs, direct injuries, chronic poisoning by lead, &c. We have also ascertained with tolerable precision that partly certain substantive changes or derangements of these central organs, or of certain portions of them, partly an interruption of the normal communication between them and the peripheral nerves, are the intermediate link, or common condition of those phenomena.

It has long been known, that in the most dissimilar diseases certain derangements, or affections of the stomach and intestinal canal are constantly met with, for many phenomena in chronic and acute diseases—*e. g.*, in gastric and nervous fevers—indicated this plainly enough, as did also certain appearances observed in the dead body and the real, or apparent results of certain remedies and modes of treatment. It is also known how Broussais was first led by these facts to assume an irritation, or actual inflammation of the mucous membrane of the stomach and intestines as the common determining and essential circumstance in the most dissimilar diseases. In other words, he generalized his gastro-enteritis, as others, at a later period, generalized "inflammation," in whatever organs and tissues it occurred, and as, in our own days, the so-called Vienna school have generalized "exudations." Others again have long been accustomed, in all the possible states of disease, rather to seize upon functional derangements of innervation as they exhibit themselves under various modifications, whether in the form of pain, spasm, hallucinations, delirium, &c., or as paralysis, or relaxation of individual parts of the body. And when, at a later period, in consequence of discoveries in the physiology of the nerves, attention was especially attracted to the spinal cord, and its so-called reflex functions, many pathologists endeavoured to prove that partly a functional excitement or irritation, partly paralysis of the spinal system, or rather of certain portions of it, were the ultimate common condition and, consequently, the essential agents in those diseased states. An attempt was even made to explain or deduce the origin and nature of all diseases—not even excepting inflammation, congestion, and the like—from the same circumstance. In a precisely similar manner, others, in the most recent periods, have rather singled out the chemical portion of the vital processes in health and disease, and the attempts to refer not only inflammation and so-called "dyscrasies," but almost all diseased states to "crases" of the blood, or, in other words, to generalize the latter—*i. e.*, to extend them as the common condition or essential circumstance to the greatest possible number of diseases—are well known.

It is no doubt true that, like all such attempts at explanation and generalization in our science, those just alluded to are very imperfect; that great errors have been committed, and the requirements of a sound logic by no means fulfilled; for we were everywhere in danger of assuming some circumstance as determining and essential to various complex phenomena and processes in the living body, without having first established its precise importance and actual influence upon the other processes individually, and, therewith, the fact of their dependence upon it. Sometimes too it would be difficult to demonstrate that the process or circumstance which we have assumed, in a given case, as determining and essential, has even existed or been in operation at all, so that its general share in the ultimate result cannot but remain doubtful. Yet these attempts are useful in showing us how the human mind goes to work in the process of generalization or reduction. Our exertions in them being directed to the deduction of a series of hitherto detached, apparently unconnected, and even heterogeneous phenomena, or processes, from essentially the same common process or circumstance whereby they may be explained, our attempts at generalization thus lead to an added value for individual facts and experience, while these, on the other hand, are the better understood through the synthetic connection in which such attempts place them.

We cannot carry out a thorough generalization without an adequate basis of established facts, while all our so-called experience remains, without such a generalization, a mere chaos of isolated empirical facts and individual observations. We do not understand them even while they stand before our eyes, until we are able to view them in their internal connection with each other, and with other allied and analogous things. It is only by bringing them into a certain relation to each other, by referring them all to the same determining process or circumstance, that they acquire, as it were, vitality, and are brought within the range of our perception. We can then comprehend them as far as, in the present state of our knowledge, they can be comprehended.

It almost always happens, in such cases, that some one endowed with a lively imagination and talent for combination, and with an unusual amount of positive knowledge, is led by certain analogies and facts to surmise some connection between phenomena and processes previously regarded as isolated. For the more perfect explanation of them his imagination, and previous knowledge, provide him with the ways and means whereby to surmise at least a certain common element as the actual determining circumstance for all, and to calculate the probability of this precise internal connection between them. The chief point, however, is to furnish the actual demonstration of this, their causal connection, which may be possible and, perhaps, even probable, or, on the other hand, not exist at all; and to this end it rests with him to demonstrate the dependence of a number of processes, &c., upon one common process, or circumstance.

But we know from experience that it has ever been more easy to surmise than to prove the fact of such a connection between the various phenomena and processes with which physiology, and the theory of disease and its treatment, have to do; and we may conclude that this will always be the case, because it seems to lie in the very nature of the subject. For a closer examination makes it evident, that the kind of demonstration which science requires is, in the end, no other than the deductive process itself,—*i. e.*, the explanation or deduction of certain more complex processes from the conditions and laws of their more simple elementary processes. The difference consists alone in our having to deal here, not only with an individual complex process, or with an aggregate of phenomena, but with a whole series of such, which appear to us, indeed, to be allied, or to agree in all their essential points, but which as certainly differ in many other respects. In both cases we proceed synthetically; but in our attempts to generalize, we undertake a synthesis upon much higher ground, and in a much greater range than in the construction of the connection and conformity of an individual process—*e. g.*, of an individual function or disease. The difficulties, therefore, the opportunities for errors and false conclusions, which are considerable even in the simpler attempt to deduce and explain an individual case, must here be increased in a tenfold degree. Our purpose in these attempts is, to deduce numerous, by no means entirely homogeneous series of phenomena, processes, &c.—whether they occur in external nature, or within the living body—from one and the same process, from the same determining circumstance, or, in other words, to resolve their special conditions and laws into higher and more general ones. How can we hope, however, to do this with any certainty in our science, in which the conditions and conformity to fixed laws of an individual process, or the laws of operation of an individual determining circumstance, have become so little known to us, and that only with approximate probability; in which we are, therefore, unable, in the series of those phenomena and processes, to distinguish with certainty what is determining from what is dependent, what is more essential from what is less essential, or accidental and secondary?

We evidently venture, as it were, upon the second, infinitely more difficult step, before we have consistently taken the first, much easier and preparatory one. It is evident, however, from what has already been said, that we can never take this first step with complete success unless we take the second along with it. Indeed, we learn from the history of medicine, and of every other natural science, that this second step has often been taken *before* the first, and that with good, though perhaps only provisional, results. And in this is to be found the vindication of every attempt to generalize in medical science—even of those which are purely hypothetical and *a priori*—as well as of every theory and synthetic combination. The very nature of our intellectual being, and an innate impulse to acquire scientific knowledge, urge us equally towards this point, and nowhere more strongly than in sciences such as ours, in which every step leads to phenomena and processes, the conformity and causal connection of which individually, and still more collectively, are but too enigmatical. And this enigmatical character of the things we are concerned with is a hint, at the same time, for caution in the notions we allow ourselves to form with regard to cause and effect, and their general connection, showing how easily we may assume the merely accidental, simultaneous, and consecutive occurrence of certain circumstances and events as a causal or essential and constant one. Daily experience, and the observations and pretended results and conclusions of the scientific investigator, as well as of the practical physician, also teach us this.

This circumstance, however, far from being an absolute and inevitable impediment to comprehension, may serve as the first stimulus to further and more correct investigation, and will even, in many respects, be our best guide in it. What has already been said of a general correction of our theories and combinations by simple experience, and more comprehensive and correct observations, applies no less to these attempts at generalization and theoretical deductions on a larger scale. For if we examine more closely how we have been led to continue our search and what has conducted us, in the end, to the discovery of general laws for whole series of phenomena and effects, we shall find that it has been the circumstance of our *a priori* notions concerning them, their originating causes, and the effects to which we imagined them to lead, having been proved on examination to be fallacious. Circumstances, processes, and influences, which we had previously regarded as determining, constant, and essential, we now ascertain to be more or less subordinate and accidental. More exact investigation, more extensive experience, has taught us, that though they were present when we had the opportunity of observing a certain event or a certain effect, they might have been absent without any change in the main result, and without interfering with the occurrence of the event, or effect, in question. We observe a function, for instance, to be performed, a certain disease to commence, or a cure to be effected, without the circumstances and agents assumed by us as essential (*e. g.*, in the former case a certain process, in the latter a supposed cause of disease, or a certain remedy), having contributed anything towards them, because, as they did not exist at all in the case in question, they could not have exercised any influence upon it.

Under favorable circumstances, on the other hand, a fortunate generalization, or reduction, has been successful in discovering, for the processes and effects in question collectively, a certain other process, or circumstance, not only as constantly present, but also as the determining influence for them all—upon which, therefore, the circumstances or agents themselves, which had previously been regarded as determining and essential, are proved to be dependent, and which must now, on that account, be regarded as the essential, real, and immediate condition of the others.

Thus, in the instance alluded to above of bleaching, &c., by chlorine, we can no longer speak of its properties of destroying colouring matters, removing noxious smells, &c., as being, in themselves, the causes of such effects, after having ascertained the chemical affinity of chlorine for hydrogen and allied substances, and its energetic combination with them, to be the immediate and essential condition of its bleaching property. According to an old notion, a halo round the moon indicated rain; it was afterwards ascertained that the production of the halo depended upon watery vapour in the atmosphere, resulting from the accession of a greater quantity of such vapour than the atmosphere could take up in the gaseous form,—*e.g.*, from a rapid decrease of temperature. This comparative excess of watery vapour was thus ascertained to be the immediate cause both of the rain and of the halo, while no influence in the production of the rain could be ascribed to the latter in itself; and it became necessary to investigate the conditions of such excess of watery vapour in the atmosphere,—*i. e.*, the laws of temperature, of the process of evaporation, and of the air-currents, &c., for the better understanding the production of the rain.

Thus, it was formerly believed that the atmospheric air, as such, was the essential condition of respiration and of life in general; or that the contact of the finger with any body was the cause of our feeling the latter. In the course of time, however, it has been shown that the presence of one only of the constituents of the atmosphere, viz., oxygen, is the essential condition of respiration; that not the inspiration and expiration of oxygen, in themselves, but its absorption, and the giving off of carbonic acid from the dark venous blood (the exchange of these gases generally), and the concomitant changes thereby produced in the blood, are the most essential processes in respiration, and in determining the continuance of life itself. And since it was further asked, what were the sources of this carbonic acid and of the carbon in the blood, and what was the action of the oxygen inspired, Liebig was led to his theory of the blood-corpuscles mentioned above, and of the iron contained in them, as well as of the part which the latter plays in the process of respiration. In like manner, it has been ascertained, with regard to sensation, that it is not produced by the mere contact of a body with the finger, but rather by the influence the two surfaces coming into contact exert upon certain nerves of sensation in the finger and, through these, upon the brain, or so-called sensorium; and, in the end, the uninterrupted communication between the latter and those nerves in the finger was ascertained to be the immediate and essential condition of the sensation.

It was believed, not long ago, that the peculiar change in the intestinal canal (the ulcers, &c., in it in typhus), was to be regarded as an essential circumstance, and frequently even as the condition, of entirely different phenomena and derangements in that disease—*e. g.*, not only of the diarrhœa and pains in the abdomen experienced by such patients, but also of the fever and debility, and even of the stupor, delirium, &c. When simple experience taught us, however, that essentially the same phenomena and derangements may occur notwithstanding the absence of all those palpable changes in the intestinal canal—*e. g.*, in the so-called exanthematous typhus—and when our comprehension of the concomitant processes was increased, we could no longer regard them as essentially dependent upon those anatomical lesions. It thus became necessary to seek for other modifying processes, or derangements, which are more constantly present, both in the so-called exanthematous and in the abdominal typhus; and attention was now directed to certain changes in the composition of the blood, in the process of respiration, and in the generation of heat; in various processes of excretion, and in the conversion of tissues within the body, &c. Could these, in their turn, be proved to be the essential and determining processes in those diseases, all those various phenomena and derangements in typhus would be reduced to the same causal connection, and the common condition of all the known forms of typhus, and perhaps of many allied states of disease, ascertained. For similar reasons, the theory of the formation of so-called exudations took, not long ago, the place of the previous theory of inflammation; for it was not only ascertained that the process of exudation (this formation of so-called exudations), was the most constant and essential phenomenon amongst the local symptoms and changes in inflammation itself, but that similar exudations were observed in many cases which did not correspond to the more ancient notion of inflammation. In a similar manner, the action of nitrate of silver upon the skin and surface of ulcers, and their corrosion by it, were at first observed as something altogether peculiar. It was ascertained, at a later period, that this salt of silver is also capable of producing the same effects upon mucous membranes in the stomach and intestinal canal; and, finally, that in both cases it comes into contact with the same substances —*i. e.*, with certain salts and chlorides, with albumen, &c.— and that its combination with those substances, according to ordinary chemical laws, is everywhere the condition upon which its local effects depend. Still more recently, the more or less analogous mode of operation of various other substances —*e. g.*, of certain salts of mercury, copper, and lead—has been referred to essentially the same process or circumstance.

In all these cases, further investigation has shown a previously unknown process, or circumstance, to be the intermediate link in causation, the proximate condition upon which certain other phenomena, processes, &c., which had before been regarded as the essential cause of the event or effects in question, themselves more or less directly, if not entirely depend. The latter can, therefore, no longer be regarded as the cause of the event or effects in question, but only as their more remote conditions, as causes of a secondary and subordinate kind; for the cause of the circumstance previously regarded as their essential condition and adequate cause, has itself, as it were, been ascertained. Thereby, however, we have solved our problem of generalization already alluded to—*i. e.*, we have ascertained one and the same causal connection for numerous phenomena and effects, a connection and conformity of a higher and more comprehensive kind; or, in other words, a higher and more general law than the first. We have succeeded in demonstrating for a whole series of effects or processes, which we had previously deduced from their own special influences and conditions, one and the same circumstance, or process, as that upon which they all depend, and consequently, as we are accustomed to express it, to generalize the latter. Phenomena and occurrences, each of which previously appeared to commence and run its course in its own peculiar manner, according to its own conformity to fixed laws, are now seen to commence and run their course in one common manner, and in conformity to one common law.

This causal connection, this newly-discovered more general law, is also much more constant and trustworthy than those first surmised, simply because the operation of a more immediate cause, or essential circumstance, cannot so easily be interrupted, or even cancelled, by other intervening influences or circumstances, as the operation of a more remote and subordinate condition, of a comparatively accidental influence and circumstance. The events and effects which we had previously deduced from the latter, depend, as is now evident, not precisely, or at least not immediately, upon such influence or circumstance in itself, but upon something intermediate and nearer; and when we have been made aware that this may exist and act, not only under those circumstances, in coincidence with the process or influence falsely regarded as cause, but equally under other circumstances, many things will become intelligible which had previously been obscure and perplexing. Thus the circumstance or influence which we had been accustomed to regard as the peculiar and adequate cause of a certain event, could, in general, only be followed by this effect on the supposition of different circumstances, more proximate and powerful influences, being present and in operation. And, on account of the very presence of these latter circumstances, the effect which we had expected to arise from the former one might, in some cases, fail to be produced; or, conversely, the event or process which we had previously regarded as its effect, might be observed without the presence or operation of the very cause, influence, or circumstance, upon which we had imagined it to depend.

But now that we have become better acquainted with the causal connection, and have ascertained the proximate causes, the actual determining circumstance or process, for the event in question, the result is far otherwise. Its influence and mode of operation remain essentially the same, notwithstanding the variations in those comparatively subordinate, accidental, and only secondarily modifying, or co-operating circumstances. Its effects will, therefore, be more or less constant, and should they chance to be absent—*e. g.*, in consequence of the intervention or interference of other influences, we shall find ourselves better able to understand the cause of such absence. Thus, the bleaching effect, or destruction of colouring matters by chlorine, may frequently not be produced, because, for instance, hydrogen, and similar electro-negative elements are not present in them, or are protected from the action of chlorine by the presence of other substances. It is, at the same time, more intelligible why entirely different substances are capable of producing the same bleaching effect as chlorine—*e. g.*, iodine, bromine, acids, &c.—since we know that they possess an analogous affinity for certain constituents of many colouring matters.

Since we have learnt that the atmosphere, in itself, is not the immediate, essential condition of respiration, and of all the processes connected with it, we also understand why, for instance, some animals can breathe and live in the water equally well with others in the air, and why others die notwithstanding the free access of air in respiration.

Since more extensive experience has taught us that malaria, in itself, is not the adequate cause of intermittent fever and other miasmatic diseases, we more easily understand why men may be attacked by them in districts where no marshes exist, as well as in fenny districts, and on the flat and muddy banks of rivers, &c. And so long as we imagined that we saw in various remedies, which had been employed in many different diseases, the essential and almost adequate cause of recovery, it was difficult to explain why, in many cases, recovery failed to follow the employment of those remedies: why recovery frequently occurred where no such remedies had been employed; and why the same disease may be cured equally quickly and certainly under the employment of the most various remedies. All this, however, is easily understood when we have ascertained an entirely different circumstance to be the constant and essential condition of such cure, and have once become convinced that the natural progress of all the vital processes, the so-called spontaneous tendency to recovery is, under favorable circumstances, its especial and immediate condition in most patients.

If we take it for granted that we have, in the above-mentioned manner, demonstrated a certain common process or circumstance to be the special determining one for a series of phenomena and processes, which simple experience has enabled us to recognize as allied or analogous—if we have succeeded in proving a conformity in their origin and course—the mind of man will not, in the end, be satisfied even with this. The same motives, the same innate reasons which led him to this first step in his generalizations, will, in combination with subsequent experience and discoveries, lead him further on in the same path. For, in spite of our previous attempts at explanation and reduction, we still meet with many apparently entirely heterogeneous series, or groups of natural phenomena and processes. We have, perhaps, succeeded in demonstrating their causal connection and the conformity of their origin and course singly, in each individual series and species, but they still remain isolated and without any mutual relation to each other. They rather appear to be widely separated, and this the more, inasmuch as we have hitherto discovered only differences, and a more or less general heterogeneity in their external appearance, in their characters, in the circumstances and conditions of their occurrence, and in their laws—in short, in their whole nature. On the other hand, we had, perhaps, scarcely ascertained a single point of resemblance and agreement in them which could lead us to surmise a relation between them, still less the essential similarity of their origin and course.

We now suddenly find ourselves taking this great step in advance. We find that such series of phenomena and processes in external nature, or in the living body, as we had previously regarded as altogether peculiar and heterogeneous, because they appeared to commence and run their course in their own particular manner and order—for which we had, therefore, assumed causes and laws entirely peculiar, as well as adopted peculiar names—agree, little as we had suspected it, in all their main points. Continued observations, and the general extension of our knowledge, have led to the discovery that the circumstances under which they occur, or act, the manner and order in which they run their course, agree in their essential points, and that their conditions and causes, their effects and results, are, notwithstanding all their apparent differences, the same in all.

Whenever we succeed in demonstrating this—*i. e.*, in discovering one and the same causal connection, the same kind of conformity for such various series of phenomena and processes—we have also discovered a "natural law of the most general, or highest kind," for we have succeeded in reducing the special laws of several hitherto widely separated phenomena, or processes, to one and essentially the same law, and have thereby perhaps approached as near as it is possible for us to do to the attainable limits of explanation and generalization in the study of nature.

This has already been frequently effected in physics and chemistry. While the electrical phenomena, properly so called (frictional electricity), which were the first discovered, were long regarded as something entirely different from the more recently discovered electricity of the galvanic battery, their essential resemblance did not, in the end, fail to be recognized. It was found that they manifest themselves in essentially the same manner, and are produced in accordance with the same laws—in short, that common electricity and galvanism are identical. And when it was further observed that electrical phenomena are produced by the magnet, and, conversely, magnetic phenomena by electricity and galvanism; when the laws of this so-called induced electricity became more and more known, it was necessarily concluded that magnetism and electricity must agree in their essential points. The same thing has, also, within a certain range, become more and more probable for the phenomena of light and heat, so that all the phenomena which had previously been regarded as peculiar agents and forces, as so-called imponderables, appear to be, for the most part, nothing else than the various modes and tendencies in which the constant relation of the sun to our planet manifests itself (Humboldt). A similar generalization was carried out by Dalton, when he demonstrated that the most dissimilar bodies combine with each other, and replace each other in decompositions, &c., in definite and constant proportions, or weight-relations (called the theory of chemical atoms and equivalents). But the most brilliant example of a similar discovery is the reduction of the laws which regulate the motion of our earth and of all the planets of the solar system to one and the same law of weight (Newton's law of gravitation); or, in other words, the proof that the motions of all the heavenly bodies, and the fall of any given body towards our earth, depend upon the mutual attraction of bodies in the ratio of their mass, distance, &c.

Nothing similar has yet been effected by us in the general field of organic nature, and its peculiar processes and occurrences; at all events, we have ascertained nothing with equal certainty, least of all in relation to the phenomena and processes peculiar to the *living body;* for, to be able to carry out such generalizations in any science, or to demonstrate one and the same kind of conformity in the origin and course of a whole series of phenomena and effects—to bring them into a correct causal relation to each other—we must first have ascertained the causal connection and conformity of each individual phenomenon, or process separately. After all, it is of little moment how we set to work to seek and find them—whether by the strict but tedious method of analysis and deduction, or by that of synthetic combination—but discovered they must be, and no genius will in this case compensate for the knowledge of simple, or individual facts. It has already been mentioned, that we have not even demonstrated the conformity in accordance with which the individual vital processes and functions are performed, nor are we aware of all the conditions and laws which govern their development, and the influence which they exercise upon each other. It also follows from this, that we cannot, with even approximate certainty, reduce the latter to more general and higher laws, or demonstrate the same causal connection, the same kind of dependence and occurrence for a number of them. We do not yet possess the individual pieces, wherewith, by arrangement and accurate joining, to construct a more perfect edifice. And perhaps the reason why we have not yet ascertained any such higher and more general laws, is partly that we have sought for these too soon, without first determining those which are more simple and elementary. Even a Newton stood in need of the discoveries of numerous predecessors, who had observed the individual phenomena in question, and established their laws, before he could bring their separate elements into their exact relation to each other; and a Liebig would probably not have been able to connect mentally so many previously isolated and obscure processes in the living body, in a chemical point of view, if its individual component parts and their connections with each other, as well as the materials introduced and affecting it from without, and the various products of excretion, had not previously been more or less known to him. All the powers of the human mind, imagination, and *a priori* synthesis, however bold, can be of little avail in the present state of our knowledge of the vital processes in health and disease. Bacon's warning, "Itaque hominum intellectui non plumæ addendæ, sed plumbum potius et pondera, ut cohibeant omnem saltum et volatum," is even more applicable to our science than to any other.

Yet history, up to the present time, teaches us that, in spite of all existing difficulties, such generalizations on a large scale, even in our department of science, have not only been undertaken, but also frequently carried out with more or less success. The attempt of Liebig and his school to deduce the processes of the fermentation and putrefaction of organic substances, as well as the most important chemical processes in the living body, in health and disease (*e. g.*, respiration, digestion, the processes of excretion, the generation of heat), from the same determining circumstance, or, in other words, to reduce them all to one and the same process—*i. e.*, to the conversion of organic substances by oxygen, and to their combinations with it—must be regarded as the grandest instance of this kind. A kind of continuous process of oxidation, or, as it were, of slow combustion in various forms (depending immediately, in the living body, upon the oxygen introduced in respiration), was thus discovered to be the common and essential condition of all these changes in the composition of organic substances both within, and external to, the living body. The latter were thus shown to be under the influence of processes analogous to those of combustion in the common signification of the term, and the act of combustion, or oxidation, was thereby generalized —*i. e.*, extended to many apparently very dissimilar processes.

This holds good, also, for those processes in the body in disease, as well as in health. From an increase, or decrease, in the intensity of that process of oxidation, it has been sought, for instance, to deduce the origin of many dyscrasic diseases and products of disease, from fever and inflammation to gout, lithiasis, &c.; also to account for emaciation and loss of weight (*e. g.*, in phthisis) from an increased conversion and excretion of organic constituents in consequence of an abnormal combination of them with oxygen; by which the increased temperature of the body in that disease is also explained. The same theory also seemed sufficient to establish an analogy between fermentation and putrefaction, and the mode of origin and operation of certain organic poisons, such as contagions and miasmata. As poisonous substances or compounds may be formed during fermentation and putrefaction (*e. g.*, the poison sometimes formed in sausages), their deleterious action in the living body was explained by a contact-action of those putrefying substances, analogous to that of yeast, upon certain constituents (albumen, &c.) of the living body. These enter into a similar state of chemical movement, into a similar process of conversion with the albuminous substances in the sausage-meat itself, or with the true ferment (Kleber) in yeast. The further analogy of these processes to the general action of organic substances, when in a certain state of conversion or putrefaction under entirely different circumstances, was next observed. The origin of many diseases was thus explained—*e. g.*, the ill health of many inhabitants of towns, from the use of water containing such organic substances; the origin of so-called miasmatic, epidemic, and endemic diseases, fevers, &c.—from the putrefaction of large quantities of organic matters in moist, marshy districts, or after inundations; lastly, the outbreak of epidemic typhus, puerperal fevers, &c., in crowded, badly-ventilated spaces, in which, consequently, an unusual amount of organic and animal matters exists in a state of conversion and putrefaction. It will easily be understood that all this, to some extent, affected our general line of conduct. The same idea, the same experience, led to the building of roomy hospitals, to better ventilation, and to the adoption of certain dietetic, hygienic, and prophylactic measures, both on a large and small scale. The advantage of the filtration of water through the ground and sand is explained by the oxidation and conversion of the organic matters contained in it; further, the necessity and advantage of establishing a greater fall of water by drains, &c., because its stagnation, and the consequent oxidation and conversion, or putrefaction, of the organic matters in it, are prevented.

It can scarcely be doubted, that in all these cases there is much which is hypothetical, and possibly altogether erroneous; but every one who can understand that, by attempts of this kind alone, processes hitherto isolated and widely separated from each other are to be brought into an intimate mutual relation, must acknowledge the value of such generalizations. If their causal connection, in the precise manner assumed, is by no means established, we have, at any rate, come infinitely nearer than before to an understanding of it. Thus, for instance, we first learnt from simple experience that dysentery prevails in the latter part of summer; and, because about that time the heat is greatest, and the fruit ripe, the occurrence of that disease was referred either to the heat, in itself, or to the consumption of fruit. Further experience, in other places, however, and more exact statistic data—especially the circumstance that many other diseases are most frequent and fatal about the same time (*e. g.*, intermitting and remitting fevers, plague, jaundice, cholera, typhus, the so-called apoplectic fevers in the tropics; and here, the generality of the diseases of acclimatization)—soon cast a doubt upon the correctness of deducing dysentery from the above-named causes. The districts and places in which dysentery and all the other diseases just mentioned were observed to occur with the greatest constancy and intensity were compared with each other, and it was found that such diseases were most frequent in marshy neighbourhoods, in the vicinity of stagnant water, in wide tracts of untilled land, in narrow and badly-ventilated streets, and houses with deficient attention to cleanliness, drainage, &c. The presence of a quantity of organic substances, the conversion and putrefaction of which are essentially promoted by the summer or tropical heat, was finally recognized as the circumstance common to them all. Instead of the heat, in itself, or the consumption of fruit, narrow streets, &c., another common condition could now be surmised with infinitely greater probability—*i. e.*, the development of deleterious gases and so-called miasmata, and their action upon the human body. And perhaps this circumstance may, at some future time, be ascertained to be itself remote and secondary, and the peculiar changes in the human body and in certain of its processes—*e. g.*, an increase of its temperature (*i. e.*, of the processes of oxidation from which it results), a consequently more rapid, or abnormal, conversion of certain of its constituents, or of the blood, &c.—be shown to be the more immediate, or direct cause.

Nor are there wanting, in the theory of other diseases, empirical hints concerning their essential uniformity of origin, course, and termination, and conformity as to the general character of their laws. So-called inflammatory diseases, accompanied by fever, and other affections which run a rapid course, were long ago classed together; cachectic, dyscrasic, or nervous diseases, were also grouped together; because processes or changes essentially the same were believed to occur in whole series of them: in the former class, we found the point of union in local changes, such as accumulations of blood, or the formation of certain exudations; in more general changes—such as a certain alteration in the circulation and composition of the blood, in various processes of excretion, in nutrition, in the generation of heat, &c.—in the latter class, it was discovered in certain derangements of innervation in various parts. Thus, we know that in the same individual, diseased conditions which, from the mode of their occurrence and from their course, we regard as dissimilar, are apt to occur in a certain constant order: in one case, simple derangements of digestion will lead to hemorrhoids, gout, lithiasis; in the other case, simple excitements of the mind and nervous system will pass on to actual neuralgia, disordered intellect, apoplexy, paralysis, &c. Ricord's theory, that secondary syphilis in the parents may produce scrofula, rickets, and other allied diseases in later generations, is also well known. We have long known from experience, that it is amongst the poorer classes, from deficient food and clothing, &c., that diseases are everywhere most frequent and severe; not only such as are epidemic and endemic, but also atrophy, scrofula, rickets, convulsions in children, the formation of tubercles, phthisis, marasmus, dropsy, &c. Instead of regarding age and sex, occupation and mode of life, or certain errors in diet, as the essential conditions of these diseases, as was so frequently done formerly, we have now recognized one circumstance common to them all, as their constant and most essential cause, viz., poverty—or rather its attendant bad food, unhealthy dwellings, and general want of attention to cleanliness and proper clothing. It has, further, been established that the manner in which diseases arise is not essentially modified by difference of climate, any more than by the difference of race or sex, but that the influence of these only extends to certain variations in diseases in relation to their degree, course, malignancy, and frequency.

Could we but once succeed in separating the numerous forms of disease into their individual elements and in ascertaining their causal connection, we might then go on to observe which of those elements are common to the occurrence of many diseases, and under what circumstances they group themselves together in various ways to form the types of disease assumed empirically. It would doubtless become evident that the latter, in spite of all their individual dissimilarities, are by no means so essentially heterogeneous as they appear to be, but that they are rather to be regarded as oscillations and modifications within tolerably narrow limits, in comparatively subordinate and secondary points; and, finally, that they exhibit those dissimilarities externally because various processes of a secondary nature in them are, in many respects, modified by a number of accessory circumstances. And if we could once succeed in determining the changes which those individual processes undergo in the healthy, or diseased, in consequence of the operation of various agents of the external world and under the influence of our remedies, the essential similarity of their effects in the living body would, in whole series of diseases, become evident. If, for instance, it could be ascertained that the abstraction of blood in plethora, inflammation, fever, &c., was not beneficial on account of the quantity of blood in the body being thereby lessened, but because the hydrostatic pressure of the column of blood and especially the oxidation and conversion of the albuminous and other constituents of the body, the generation of heat and a certain excitement of the nervous system, &c., were thereby diminished; and if it could be further ascertained that a diminution of the vital heat is here the immediate determining circumstance, we should be better able to explain why cold, spare diet, and cooling drinks, rest, dark rooms—in short, every thing which tends to diminish the vital heat, as nauseating medicines, acids, neutral salts—have similar good effects in such diseases. We should have succeeded in reducing the mode of operation of all those remedies to essentially the same circumstance, and should better understand, not only why the same remedy, the same mode of treatment, may be beneficial in diseases apparently altogether dissimilar, but also why apparently altogether dissimilar remedies and modes of treatment have, in the end, the same result in the same disease. This process of reduction, or generalization, will thus probably, or indeed certainly, gain ground gradually in our special department, and the mass of different diseases, as well as of our remedies and modes of treatment, be necessarily reduced to a much smaller number. In the former, the ancient *Ægritudo* will again more or less take the place of the innumerable diseases of our systems; in the latter, precedence will be given to the natural course of the vital processes in conformity to internal constant laws, which we, under the most favorable circumstances can only assist by our co-operation, and that, though by ways and means the most various, in essentially the same manner.

This may suffice to show why, in the study of life in a state of health and disease, as well as in the study of natural phenomena in general, the final object of our scientific efforts must be placed in deducing the greatest possible number of effects from the smallest possible number of causes; or, in other words, to resolve the conditions, dependence, and conformity of the greatest possible number of apparently heterogeneous phenomena and processes into the smallest possible number of hypotheses, or assumptions of laws, and in so doing to bring them under the widest and most general laws. It cannot be said where, in the progress of our intellectual powers in this direction, the pillars of Hercules, beyond which we cannot pass, stand. Much, however, already tends to show that the phenomena and processes which we call "life" agree in certain and even important points with those of light, heat, and electricity; and perhaps, in accordance with this idea, we might be justified in assuming as the final object of our investigations and generalizations in the study of life, the progressive connection of this "life" of certain peculiarly constituted and peculiarly arranged materials, with the phenomena and processes of the material world in general, and the reduction of the laws of both to essentially similar, or analogous ones, and in every case to the smallest possible number. The advance of our knowledge may not only reduce all the vital phenomena and processes to a small number of elementary processes, and bring these into conformity with each other, but the same process may in time be extended to everything which exists and occurs upon our earth. As all things had their origin simultaneously, successively, and out of each other, so are all things more or less intimately connected as constituent parts only of one great organism.

For the present these are, it is true, mere hypotheses, or, at best, probable possibilities, and it would be sinning greatly against all the principles laid down for the investigation of nature to steer at once for that great final goal, or even begin to form surmises concerning it. We must, if we would reach it, begin by being convinced that we cannot yet ascertain those most general and ultimate laws. We do not, at present, clearly understand the causal connection of any individual process in the living body ; still less are we aware how the more complex functions and processes are connected with each other and in conformity to what laws they occur. We must, therefore, first of all, as has already so often been mentioned, endeavour to determine these laws and the circumstances under which many hitherto unconnected series of phenomena and processes correspond in their mode of origin and course. We shall thence be enabled to demonstrate experimentally that the same conditions and causal connection exist in other series of vital processes, and thus have it in our power to reduce them to one and the same law.

On the other hand, however, it is precisely a science such as ours, in which all the phenomena and processes are highly complex in themselves and dependent upon the most various and, consequently, fluctuating influences, that gives the greatest stimulus to the hope of discovering such general laws. The very complexity and multiplicity of causes and effects, and the consequent oscillation and instability of the final result, are the best proofs that no definite conformity has hitherto been ascertained for them, but yet, when viewed under another aspect, the best guarantee that such may and must be found. If we consider the constant and uniform course of organic development in relation to form and materials, as well as to its vital phenomena; if we consider the unvarying order and conformity in which all the different tissues, the whole complex structure of plants and animals develop themselves in the impregnated germ, from the cell to the perfect organism, and, parallel with these, the multiform phenomena or manifestations of activity from the first moment of life to its cessation, it may be regarded as more than a surmise that, in all these cases, no great number of heterogeneous agents, or causes, are in operation. It appears to be something more than a mere hypothesis that these laws and the definite order in which they act may, in the end, be reduced to a very small number. The numerous malformations which may and do occur in the living body, and, in a certain respect, the numerous forms of disease to which it is liable, have, notwithstanding all their variations and changes, only confirmed that supposition. Thus essentially the same plasma, or exudation, may perhaps develop itself in one case into tubercles, or cancer, and in another into the mere induration or thickening of a tissue; and many of these special forms of development appear, in the end, to obtain only because that which has assumed the one form cannot assume the other.

The manifold and varying character of those vital processes is probably dependent only upon the complex combination of the organs and tissues in which they occur, upon the multiplicity and constant variations of the internal and external agents and materials which co-operate to produce them, and upon the variously operating properties of those agents and materials themselves—in other words, upon the complex and changeable nature of the circumstances and influences which jointly effect one and the same result. The mode and conformity in which the latter act, and in which those phenomena or processes in the living body occur, are probably less varied in their nature. Such complexity is rather apparent than real, and it arises from the many different kinds and series of vital phenomena, *i. e.* from the manifold modes and directions in which the substrata or materials of the body manifest activity or vitality; just as the multiplicity of the chemical processes rather depends upon the different nature of the substances and their combinations with each other, than upon any differences in the laws themselves, by which those substances and combinations of substances are accustomed to act upon each other.

Even now we may surmise that all these processes and actions in the living body occur in conformity to a few simple and essentially analogous laws, and, for this very reason, we may anticipate the eventual reduction of those vital processes to a small number of laws, and look for the consequent advance of our generalizations in the science of life. At present, it is true, we can only hope to carry out such generalizations approximately, and thus it may remain for a long time to come; but with the advance of the general investigation of nature, especially in a physico-chemical point of view, we shall doubtless come nearer to ascertaining more definite laws, and to gaining a greater number of indefinite links for the causal connection of our phenomena and processes, and thus gradually advance to the knowledge of more general laws. If we have hitherto succeeded in ascertaining but few of these, we must the more diligently, in every fresh discovery that takes place in the various departments of nature, examine the different series of phenomena so enigmatical to us, and endeavour to ascertain upon which of them such a discovery may possibly throw more light, or what general advantage is to be reaped from it in the way of our practical progress.

The wider our horizon becomes, the better shall we be able to connect together many things that have hitherto been unexplained, obscure, and isolated, and by linking these with other better known occurrences, with which their dissimilarity is proved to be only apparent, bring the more and the less familiar into connection, and thus extend our knowledge of the latter. And though this knowledge may fluctuate according to the degree and development of our general knowledge and views, and vary also with the varying mental tendency and judgment of individuals, a vast ultimate gain in the way of a clearer perception and a more comprehensive generalization will none the less have been realized.

SECTION VI.

ON CERTAIN AUXILIARY OPERATIONS AND PROCESSES.

OBSERVATION AND EXPERIMENT. — HYPOTHESES, COMPARISON OR ANALOGIES, AND THE CALCULATION OF PROBABILITIES IN GENERAL. — TECHNICOLOGY, TERMINOLOGY AND DEFINITION, DESCRIPTION.—GROUPING OR CLASSIFICATION.

HAVING endeavoured, in the above, to represent in a connected form, and as clearly as possible, the objects of investigation, and the modes by which it must be pursued, it now seems desirable to explain their several and technical details, and the expedients and operations to be adopted in such investigation.

We have already endeavoured to explain how our search after experimental truth tends to connect these various operations with itself and with each other, by bringing the objects with which we have to do under a conformity to general laws, and by assigning to each of our mental processes its due place in the mechanism of investigation. We have now to test, one by one, some of the more important levers and springs of inquiry, and trace their appointed share in the general process of investigation, with the clearness indispensable to their correct and appropriate employment. And this is necessarily of the highest interest, as upon it depends the degree of certainty with which we are justified in relying upon the results to be obtained by its assistance.

In all cases the observation of things in their actual condition is the first step towards their further investigation, as an insight into their nature is only to be obtained through the contemplation of their existing conditions. We shall, therefore, first speak of observation, and its attendant, scientific experiment. This observation, these experiments themselves, involve, however, a certain co-operation on our part—involve, in short, our *ego*—which constitutes the transition to further mental operations, to hypothetical conjecture, to comparisons and *a priori* combinations, with whose aid we cannot possibly dispense in the investigation of causal connection and of the conformity of general phenomena and processes. Hence springs the necessity of duly estimating the value of observation. We shall speak finally of certain comparatively subordinate and more formal auxiliary operations, no less to be dispensed with, and exerting the greatest influence upon our methods of investigating and observing, and even upon our very opinions and conceptions, such as language, nomenclature, description, definition, and classification, as these are connected with the theory and practice of medicine.

I. OBSERVATION AND EXPERIMENT.

Our final aim in all investigations of natural phenomena and processes—and, among such, of those of medical science—is the understanding of their causal connection and conformity to fixed laws, and the ability to establish them by experimental demonstration. This demonstration consists, as we have seen above, in the establishment of all the conditions under which the phenomenon, or the occurrence in question takes place. We must, therefore, in the first place, have observed these phenomena and processes, and their conditions in many single cases; we must be able to infer that the things which we have met with in a certain number of instances as actually present, as really occurring in this or that manner, as constantly happening together or in a certain order, and qualifying each other, would exist equally and exert the same influence in all cases of a similar kind. In other words, as it is from these observations and established facts that we deduce the causal connection and laws of the phenomena and processes in question, we must be able to extend what we have found in a certain number of single instances, to all phenomena of the same kind, or of the same series, and thus generalize the results of our observations.

To arrive at a clear comprehension of phenomena and processes, we must first have observed them in a greater or less number of single cases or examples, we must have become acquainted with them in all their sentiently appreciable qualities, as well as in all their relations to each other, and hence the necessity of thoroughly correct, *i. e.*, minute and complete observation, becomes self-evident.

It is not, indeed, the exact province of logic to teach the art of observing, and still less has it the power to form apt and expert observers. All this can only be effected in each particular department by education, practice, and training. The true province of logic, as we have already pointed out in its application to medical science, is to teach us the means and processes through which we may attain to a knowledge of the phenomena and incidents with which we are concerned, and gain an insight into their causal connection and conformity to fixed laws. And, since observation occupies so important a position among these, it becomes our object to test how, by its aid, we may best attain our final aim. If logic is not meant to teach us how to observe, and can give us no rules to guide us in this respect in any given case, it can teach us something no less necessary, namely, how to estimate what we have observed, and to subject the results of our observations to an appropriate examination. It can show us what conditions each observation must fulfil before we may venture to accept its results as true, and rely upon them as being altogether conclusive.

These conditions, these demands upon our observation, must be self-evident. All the phenomena, with their concomitants in each separate case, whether occurring in nature, or under circumstances which we have ourselves induced—*i. e.*, by experiment—must have been observed correctly and investigated amply. We must, in all cases, observe things as they really exist; we must always be convinced, and be able to prove, with regard to what has once come under our perception, that it occupied exactly this and no other position. Our observation must be in itself thoroughly exact and conformable to nature and truth, and also, in an especial manner, objective enough—that is, mixed as little as may be with any views, deductions, or interpretations of our own framing—as all these may possibly be false, and are, probably, narrow and defective.

This looks, at first sight, a comparatively easy matter, as all that is required for such observation appears to be but the due discipline of the senses, as each department of study teaches us to use and apply them, assisted, in some circumstances, by the correct employment of certain auxiliaries to observation—*e. g.*, the microscope, the thermometer, the stethoscope, &c. It is evident that, were the operation of our senses altogether free from the possibility of error and delusion, everything with which those senses could make us acquainted would be clear, and in conformity with truth and nature. Our observations, in this case, could never contradict each other, and a difference of opinion upon the subjects with which they were concerned would be simply an impossibility.

But experience has, with regard to observation, a far different lesson to teach us, and particularly in our special province. Too frequently, as we all know, our observations, in a greater or less degree, contradict each other; and almost daily is this a source of dispute and discussion. What one man looks upon as an established fact—because he believes that he has observed certain things as thus existing, and in this peculiar relation to each other—another has failed to find in a similar case, and under circumstances precisely similar. In a patient, for instance, in whom A has perhaps observed inflammation of an internal organ—as the brain or intestinal canal, or a catarrh of the mucous membrane of the latter—B has been unable to find any of these, but perhaps has noticed something entirely different—*e. g.*, typhus or simple disturbance of the nervous function, a so-called spinal or cerebral irritation, or, it may be, indigestion only. In the same manner, A has perhaps traced a certain disease to certain causes, and been able to cure it by certain remedies; while B has been unable to see anything of the kind, and would possibly have found the same remedies ineffectual in the case of the same patient.

Contradictions of this kind plainly indicate that the mode of observation must have been defective or erroneous somewhere and in some direction, and betray that what has been observed has not been the simple and truthful fact or observation which it has been taken for. The fault may often be traced to haste and carelessness in observing, to prejudice, ignorance, partial views, or other defects, on the part of the individual observer. It is unnecessary here to take these errors into consideration, as they are, in a certain degree,

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inseparable from all human speculation, and are only to be avoided by care or diligence. It is more important to our object to point out another source of delusion, frequently recurring, because it is connected less with the individual observer than with the nature of observation itself.

We are apt, namely, in the process of observation, to confound, almost instinctively, two things which are entirely different, and to mix up with what has been actually observed, the impression it has made upon our minds and judgment, thus confusing perception with the deductions to be drawn from it. What we think we have really observed, what we believe our senses have actually detected, we concisely describe as observation; yet it contains not only what we have really observed and learnt through our five senses, but includes, at the same time, something entirely different—that is, our interpretation and explanation of what we have sentiently observed and adopted.

Thus, we say that we have seen erysipelas of the face, or an ulcer of typhus in the intestinal canal; that we have heard a certain valvular *bruit* in the heart; that we have felt schirrous tumours in the abdomen of a sick person; or parts of the fœtus in that of a pregnant woman. In reality, however, it is not erysipelas we have seen, but a red, tense, shining surface in the face; on the mucous membrane of the alimentary canal, perhaps, an excavation with a certain ground and border, but no ulcer of typhus. In the cardiac region, we may have heard, instead of the normal sounds, an abnormal, but not a valvular *bruit;* in the abdomen, we may have felt bodies with certain contours, of a certain consistence and mobility, &c., but no schirrous tumours, no fœtal limbs. But upon the ground of former experience, whether our own or that of other men, we are agreed to call the groups of phenomena thus sentiently recognized, erysipelas, ulcer of typhus, valvular bruit, &c. Even in the moment of observing them, we have unconsciously, and almost instinctively, compared what we have recognized in the present case with what, in similar cases, we have ourselves already witnessed, or have in other ways known or heard about them, and are thus ready to pronounce upon their nature from their coincidence with these already-noted phenomena.

Much in this way, on first sight of a distant city, we should say, perhaps, from observation, that it lies so many miles from us. But we do not see this distance itself, any more than we see an erysipelas or an ulcer of typhus, all that we truly see being an indistinct assemblage of houses and other objects, through whose apparent size, as contrasted with what we know of their real magnitude, we are able to infer rightly of the distance that separates us from them. Just so, when any one asserts that he has seen an individual affected with fever, typhus, or scrofula, he has, in reality, never seen either the fever, typhus, or scrofula in question. For not only do these diseases not present any condition so defined and appreciable by the senses as a local inflammation, an ulcer, a bruit, or a tumour, but we should be unable to recognize sentiently the latter diseased conditions, as such, even if some of these were present.

In all these cases, we must first consider the possibility, or probability, that all the phenomena we have been able to observe in the case before us correspond, completely and essentially, to such and such others already observed by us in similar cases. We compare the former with the idea or picture of the latter already existing in our minds—often, also, of many others (as, for instance, in the so-called differential diagnosis of our patients)—and by this means are able to judge of their resemblance or identity with these, and of their difference from certain others. What we have, therefore, denominated observation, and been accustomed to consider the direct result of the evidence of our senses, is, for the most part, a deduction—even if an unconscious one—from our former experience and the notions of certain things resulting from it, as—*e. g.*, of phenomena and processes in the healthy, in a so-called vital function, or of a disease, as erysipelas, fever, typhus, &c.

These notions, conceptions, ideas, and images, whether correct or incorrect, whether formed in consequence of our own former experience, or in consequence of the communications and instructions of others, have existed and slumbered, as it were, in our consciousness. We have abstracted some picture, some particular conception of certain things and phenomena, which thus, in a certain sense, has existed within our consciousness, and not external to us, and we are now reminded of this picture, and of our notions concerning it, by what we have observed in the case before us. With them we involuntarily compare all the phenomena or things formerly recognized through our senses; and, if we find that the latter coincide with our picture and conception in all their chief points, we now declare them to be indentical, and give them the same name. Our former picture or notion serves us here as a means of comparison and type for all similar cases.

In all cases, it lies in our very nature to be able to see and recognize nothing through our senses, without, at the same time, drawing from it a conclusion concerning the postulates —*i. e.*, concerning that which might have occasioned these our perceptions—and forming for ourselves some notion of them. For of these, our senses, of themselves, could never teach us any thing. What by their aid we are able to recognize calls forth our reflections and judgment, our combinations or notions concerning them. And only by the creations and effects of these, in short, by our own co-operation in the manner pointed out, can we observe, whether correctly or falsely, anything whatever. We should, therefore, in a given number of cases, endeavour to comprehend, as we best may, the phenomena which are common, constant, and essential to such cases; we should next abstract, from numerous observations, the idea—*e. g.*, of a certain function or disease, and, finally, of life or disease in general, and thus create for ourselves, by our own exertions, some conception of the particular phenomena which we have recognized through our senses. All that we can observe of any object, or process, is only concerned with the properties and states which we are able to recognize in it —*i. e.*, the impressions which our senses receive from it. On the other hand, our notions and assertions concerning it—which we class together under the head of observations—refer not only to these our sentient impressions and perceptions in themselves, but also to the object itself—*i. e.*, to the total result of these sentient impressions and the notions we form from them. From those individual phenomena, and from our sentient impressions in general, we unconsciously draw conclusions concerning the object itself, and its whole nature; and the notions and conceptions with which we compare a new case, and the phenomena observed in it, appear to have arisen in the same way.

We need not here ask how and when these previous notions have been acquired. It is sufficient to *know* that they exist, and that we must everywhere, if we would obtain a clearer idea of what we briefly term "observation," bear in mind the fact of their existence, co-operation, and connection with all that we become aware of through the senses. It follows from this, that such kind of observation not only includes what we have actually observed—*i. e.*, what we have ascertained by the aid of the senses, but also the notions we have formed of what we have observed, and influences our notions, not only of the phenomena or properties that have come under our observation, but also those which we form with regard to the object or objects which may have exhibited them. We never express simply what we have really seen or felt—in short, observed —neither can we do so, however earnestly we may wish it. What we express is our own explanation of what we have observed, compared, and grouped with many other things; we describe it, in short, as it *appears* to us. For this very reason however our *ego,* our whole previous knowledge and experience, our notions and views at the moment, acquire a much greater influence over the mass of our observations than we are for the most part conscious of. However exact and objective our observations may be, there is always much of what is subjective, much of our *ego* involved, and in most cases, especially in the case of what the physician meets with at the bedside, our conclusions, notions, and views of what we actually recognize, form a much larger part of our observations than the latter in itself.

It naturally follows from this that we must strive, in our observations, to distinguish what we do really observe, from what we do not observe; to recognize what, in the phenomena and objects observed by us, may reach our consciousness directly through the senses, and what, on the contrary, is merely the product of our conclusions from it; we must be aware, for instance, how, in our interpretation of what we have observed, we have compared it with other things, and have brought it into some relation to our previous notions. And it is the office of logic to direct our attention to this necessity, because the circumstance of confounding these two so heterogeneous elements in our observations, is a fertile source of error.

Thus, in the investigation of a vital phenomenon, or of a state of disease, everything which we have recognized by the aid of the senses may be perfectly correct—*i. e.*, may agree exactly with the natural state of things—yet the observation as a whole, and the manner in which we express it, may be incorrect in consequence of the interpretation we put upon what we have observed. We may have observed certain phenomena and changes in a patient with sufficient accuracy, but the opinion we express—*i. e.*, our assertion that we have observed a certain disease in that patient—may be none the less erroneous, so soon as it leads us to infer the existence of a given disease from the phenomena which we have recognized, and to regard the latter as symptoms or proofs of a state which may, in reality, have no existence. This species of error in our observations is as possible as in the case of a hysterical female, who may really experience the sensation of a ball in the throat, or of another individual, who may see sparks or hear various noises, although, in the case of the former, no such ball exists, nor, in that of the latter, any external cause to produce such effects. In both cases perception, sensation, sight, hearing, have been doubtless, in themselves, correct; the error lies in the interpretation of them, and in inferring from them the actual existence of a compressing, shining, or sounding body, or, in other words, in inferring the existence of any causes or circumstances capable of occasioning those sentient perceptions. In all such cases we have attributed what has been actually observed by us, to influences and circumstances which have not, in reality, produced it.

These examples serve to show how much, in our interpretation of what we have observed and in the notions we form concerning it, depends upon our previous knowledge and upon the nature and extent of our previous experience. If any one, whom experience has rendered more familiar with such hallucinations, no longer allows himself to be deceived by them and led, for instance, to assume falsely the actual existence of bodies producing such sentient perceptions, equally little will the experienced and circumspect physician be led into any erroneous and unfounded interpretation of the phenomena and conditions observed in a patient—*i. e.*, into the assumption of a diseased state which has no real existence; and he will be the less exposed to this danger in proportion to the extent of his previous knowledge and experience of the particular phenomena which each case presents.

Since we are compelled to interpret what we observe in any given case, without delay, and to form for ourselves some notion concerning it, and since this our interpretation of it depends essentially upon the notions and views we entertain at the moment with regard to similar phenomena, as well as with regard to many other collateral things, the importance of these notions and views, in even the simplest observation, becomes evident. If our previous notions are correct, they will facilitate our interpretation of what we observe in any given case, and render the conclusions we draw from it more trustworthy; while, on the other hand, the incorrect, or partial and obscure character of our previous notions will essentially interfere with our correct observation of the things before us.

One of the most important requirements for the formation of such clear and correct notions will therefore be the habit of accurate observation, which includes attention and exactness, and demands that we draw no conclusions from the phenomena observed beyond what they safely tend to. It is also necessary that we should be able, in any given case, to render a true account of all its details, and to reason them out with clearness. But the possibility of such a kind of observation presupposes many other things—not only knowledge of the subject, practice, the capability and habit of taking in all the points of a given case clearly and comprehensively, but also a good memory, by the aid of which we may recall, at the right moment, the results of our previous experience, so that we may be able to compare with them what is new in the case before us. It follows from this that the kind of observation we have shown to be indispensable must be infinitely less attainable in the study of vital phenomena, and especially of those which it is the province of the physician to investigate, than in the physico-chemical or anatomical department—in the simply descriptive natural sciences. It is easy to understand how an observer may much more easily deceive himself or overlook various important things in the former than in the latter, from the absence of clear and correct *a priori* notions of life, disease, &c., and of the assistance which such would render. The same special circumstances, the very difficulties in the nature of our subject, which so much, in any given case, impede observation, and so easily give it the stamp of incompleteness, obscurity, and one-sidedness, have exerted a similarly adverse influence upon our predecessors, as upon us.

The observer at the bedside in particular has always been compelled to form his notions of a disease and of its origin and conditions, as well as of its cure, and of the effects of his remedies in it, from a more or less incomplete examination of phenomena and incidents; and it will be long before our followers are, in this respect, much more advanced than ourselves.

The physician, and especially the practical physician, cannot possibly gain any clearer notions of a disease, or sufficiently correct ideas of the state of the patient before him, not only because this is simply impossible in the present state of our knowledge, but because he has seldom, if ever, felt called upon to give to others a clearer and more distinct account of his observations, still less of his conclusions from them. Many scarcely ever make any serious attempt to do so; but if any one does make it, whether in an earnest, scientific spirit, or from a mere disposition to theorize, it soon becomes evident that he has no certain knowledge, no correct and clear notions and data upon which to rest. He rather finds himself involved in a complication of hypotheses, of mere possibilities, with which, if he possess any knowledge of himself and of his subject, he cannot possibly rest content. And in this respect he shares the lot of the statesman, the historian, and the moral teacher.

On the other hand, however, the able physician and experienced practitioner are by no means incapable of observing the state of their patients as rapidly and distinctly as is necessary for the purposes of treatment—*i. e.*, of concluding from what they recognize in their patients, and from all the peculiar circumstances of a given case, what is imperatively called for and how they may best cure or relieve. They will, it is true, be unable to give, either to themselves or others, the precise grounds upon which they found their opinions of the case, or their treatment of it; and it is with these only—*i. e.*, with the demonstration of the accuracy of their views, that logic has to do. But they cannot furnish any adequate grounds for their views, because they have been able to obtain only imperfect hints and notions from their previous observations—in short, from their experience. They may indeed have formed, though perhaps unconsciously, such views and conclusions as have enabled them to adopt beneficial measures in a given case, but not such clear and correct notions of the state of the patient as to put them in a position to deduce from them all their grounds for those measures, and to thoroughly explain the reasons for their adoption of that particular mode of treatment.

It then becomes evident from what has just been said how much our conception and interpretation of what we have seen—*i. e.*, our observation as a whole, must depend upon our *ego*, upon our previous knowledge and views, and upon our mental capacity—in short, upon the general tendency and development of our intellectual being. We draw our conclusions concerning the nature, properties, and relations or causal connection of the phenomena and objects before us, less from our immediate observations by the senses than from our previous experience, knowledge, or views of similar things, and from what we regard as most probable in the case before us. They depend therefore upon the capability of our minds to receive impressions from what we see, and upon the nature of such impressions; also upon the trains of thought, the feelings, interests, and impulses called forth in the individual observer, and upon the direction these may take. This explains why observation will always vary in accordance with the special character of the individual, and why it has always varied and will continue to do so according to time and place. It was not without cause that Rousseau complained that "it is a melancholy fact that so much philosophy is required for the observation of what we see every day." Thus the ancient Romans, with all their cultivation, saw, when passing through Switzerland, nothing of the beauties which are objects of admiration to us.[1] They complained only of the bad roads; and even a Julius Cæsar employed the time during the passage of the Alps in writing a grammatical treatise "de analogiâ." In a similar manner, it was long before painters

[1] Compare A. von Humbold, 'Kosmos,' vol. ii, 25.

knew how to draw buildings and many other objects precisely as they saw them—*i. e.*, in perspective—not as they are in reality. In short, man is and remains, in a certain sense, an egotist, even in the investigation that may seem most candid, and in the simplest and most circumspect observation. It is through a necessity of our nature that we are accustomed to observe and conceive a given phenomenon or object only in so far as we are able to form some notion of it. Whenever, therefore, a phenomenon or object is not of so simple a nature as to be appreciated by the senses, and taken in by them at once, in its whole extent—which is obviously impossible in the case of any natural phenomenon, and most of all in those connected with our peculiar department—we shall be inclined, on that account, to conceive and judge of it in accordance with our own *a priori* notions. The manner in which an individual observer judges of a given case will thus always depend more or less upon his general train of thought. In the observation of each individual case, much will depend, therefore, upon the amount of our previous knowledge, upon our general mode of judging and drawing conclusions, and upon the path, so to speak, by which we approach a given phenomenon, or a scientific question or investigation. Of the innumerable things which nature offers to our view we select those only which happen to attract our attention. We often take a childish pleasure in empty shells and fragments, and persuade ourselves that we have found the kernel or essential part, simply because we choose to regard it as such. The interest we feel in our own notions and views of various things is often infinitely greater than that which we take in the things themselves as they actually exist, and our reason, as Kant remarks, recognizes only that which it has created after its own design.

These truths, as applied to observation in general, make themselves doubly felt in such a study as ours, and upon us, in an especial manner, it is incumbent at all times to bear in mind the exact state of things. For, in medical practice, not only do the most manifold and complex phenomena and occurrences present themselves simultaneously to the observer, without his being able to isolate and fix them, but he cannot even recognize directly all the functions and organs, from whose varying phenomena and changes conclusions might be drawn concerning the processes and occurrences at work in them. We have always been necessitated to observe the phenomena and processes which are the objects of our investigation, superficially and from without, and to draw conclusions, from the individual phenomena which we are able to recognize, concerning the other, infinitely more important ones, which occur within the body. It lies, therefore, in the very nature of our subject, that in our case, observation and comprehension must depend much more upon the mental peculiarities of the individual, than they do in that of the physicist and chemist; for in our branch of science previous views and habits of judging are much more intimately concerned than in theirs; and our observation, as a whole, will always possess more of a subjective than of an objective character.

We easily persuade ourselves that we proceed to an observation candidly and without prejudice, perhaps because we are conscious that we do not seek to demonstrate or refute anything arbitrarily and *a priori*. We rely upon our fixed and honest intention to see and judge of all the phenomena presented to us precisely as they are, and not as we might wish to find them; and because an individual may feel no interest in the result of his observation in a given case; because it may appear to be indifferent to him whether it confirm or refute his personal views, he, perhaps, considers himself protected from all these dangers. Yet this very feeling of security, which appears to him to be so well grounded, may endanger the accuracy of his whole observation. In a certain sense, it is almost absurd to suppose that any one can free himself entirely from prejudice, and proceed to an observation without being influenced by his previous views, notions, or peculiar mental tendency. This is pretty much as if any one were to take out his eyes for the purpose of ascertaining how he could see without them. Bacon understood this much better when he said "intellectus humanus luminis sicci non est; sed recipit infusionem a voluntate et affectibus. Quod enim mavult homo verum esse, id potius credit."[1]

Even the apparently unprejudiced and impartial observer strives, like every one else, to attain some end, for some purpose or other lies doubtless beneath his observation. We *must*

[1] De interpretatione Naturæ, lib. i, aphor. 47.

seek to answer some question, to determine the presence or absence of certain phenomena, &c., for without such motive we should never undertake any observation or investigation whatever. It follows, in fact, from the very attempt at investigation, that we desire not only simply to observe and recognize certain phenomena, but also to understand them, and to this end we must strive to ascertain their causal connection, conditions, and laws. Thus the physician endeavours, from the aggregate of the phenomena which he observes in a patient, to determine his actual state, and the nature of his disease—in other words, the conditions and causal connection of all those phenomena. He endeavours to ascertain the part played by certain occurrences and circumstances, and to acquire a certain understanding of the effects of deleterious influences and habits of life, as well as of the remedies to be employed, and the mode of treatment to be adopted. The same thing holds good, in essentials, for the observations of the physiologist. It is precisely because we have some special object in view, that our investigation acquires the requisite exactitude, and that our attention is directed to, and fixed upon, various phenomena and things which, perhaps, from want of interest on our part, might have escaped us altogether. This pursuit of a definite object involves little or no danger to the accuracy and impartiality of an observation, if the things to be observed are sufficiently clear and simple not to admit of very different modes of conception or interpretation; if they have become more or less known to us in their conditions and effects, and in their relations to other things; or if, perhaps, in a given case, only one new phenomenon or occurrence, only one enigmatical circumstance presents itself among others already long familiar to us. But this is never the case in medical science, because all the phenomena and processes which come under our observation are not only, in themselves, in the highest degree complex and fluctuating, but have all remained more or less obscure in relation to their conditions and effects, in short, to their causal connection and conformity to fixed laws. Instead of being able to acquire a clear understanding of the latter, we have always, even under the most favorable circumstances, attained only to more or less probable conjectures, to certain hypotheses and views. The beginner has always been compelled to content himself with explanations of this kind, or, perhaps, with mere words which say all and nothing, because his teachers had nothing better to offer.

Whether our views and notions of various things be correct or not, they will not fail more or less to influence our conception and interpretation of all which we recognize by the aid of the senses; and so long as our knowledge of the vital phenomena and processes remains so uncertain and, at the best, so imperfect as at present, everything which we observe in a given case will, on that very account, admit of various interpretations. Our attention will easily, perhaps without our being conscious of it, be directed only to that which we are willing to see, or at least to that with which we are most familiar; in both cases the result is much the same. We shall further arrange and judge of what we observe, just as it may correspond with our experience and previous views, in short, with the precise stage of our mental development. The practical man, with his peculiar interests and tendencies, will take a different view of everything from the theorist, while that taken by the latter will depend upon the particular school to which he belongs, upon his general train of thought, and upon his personal intentions and wishes. But this we are far from regarding as prejudice, because it appears to us to be the only correct and possible view; neither can we believe in any one-sidedness or partiality in our observations, because we feel conscious of having honestly investigated all the points of the phenomena and occurrences before us, in so far as they are accessible to *us*, or rather in so far as they appear to us to have a real existence and importance. The physiologist, and with him the physician, occupy much the same position as the historian and the statesman, who are, perhaps, even less familiar with the phenomena and with the causes and laws of the occurrences with which they have to deal than ourselves. In politics, one man takes an entirely different view of the same things from another, because it happens to correspond better to his wishes, and to the interests of his party; he easily persuades himself that he sees much which has no real existence, while he overlooks, or sees imperfectly, all that is opposed to his views and tendencies.

That the accuracy of observation in medical science is daily exposed to these dangers is a well-known fact. If we regard a certain portion of a complex whole (*e. g.*, of a function, or disease), or certain of its elements, or certain circumstances and influences which affect it, as important and determining, and if we seek to investigate and demonstrate the operation of the latter, we are but too apt to observe only what attracts our special attention. And this all the more readily, because everything which offers itself for our investigation is, in itself, multiform and complex, and because the things with which we have to deal may, in the absence of ascertained conditions and laws, be classified and interpreted, with almost equal probability, in various ways. The phenomena and occurrences to be observed and investigated are, therefore, brought into mutual relation in accordance with the views of the individual. We assume such an internal relation and causal connection between them as we had previously regarded to be real, or at least most probable, and which we have, perhaps, sought, in a given case, to ascertain and demonstrate. Our conclusion, if it deserve to be so called, will thus almost inevitably depend, in a great measure, upon our previous views and notions—*i. e.* upon our experience and upon our interpretation of facts already known to us. This our previous mode of interpretation and explanation may, as has already been stated, be correct, or erroneous and deficient; in every case it is never strictly correct in relation to any vital phenomenon, because it is nowhere based upon a clear insight into the actual state of things. On that very account, its application to a given case, though more or less correct, can never be completely so, a fact confirmed by the result of every theory and attempt at explanation.

If we bring to the bedside of the same patient a disciple of Brown or of Broussais, an empiric of the old, or one of the modern stamp, an adherent of the so-called Vienna anatomical, or of the Giessen chemical school, a nerve-pathologist or a blood-pathologist, each will recognize a different state of things. The opinion which each forms of fever, for instance, and similar aggregates of symptoms, of their causal connection and dependence upon various local or general changes and conditions, and of these in their relations to each other, will be different from that of the others. Each of them, if he reflects upon it at all, will form a different notion of all that he has been able to observe; he will arrange and combine the various phenomena in the patient after his own manner—*i. e.*, in accordance with his own point of view; and, if the same remedy be administered in a given case, the assertions and opinions of each concerning its effects will equally differ. For each has expected from it different services and modes of operation in accordance to his previously formed theory; he will, therefore, interpret what he has observed in the manner which best corresponds to his own views, and in the remedy employed will acknowledge only such effects as it has been his aim to produce.

So long as Brown's system prevailed many believed that digitalis produced an excitement of the heart and circulation. According to their view the pulse was first accelerated and rendered fuller before it could become weaker and slower, and they asserted that the subsequent depression depended upon the previous excitement (Saunders, Hutchinson, and others). If another, a disciple of Broussais, for instance, had observed exactly the same thing, his interpretation of it would have been entirely different, because he would not attach the same signification, or an equal importance to such excitement of the circulation, &c. In like manner, at the period when the simple, non-mercurial treatment of syphilis prevailed, patients were believed to be injured by mercury in a degree never observed before or since. A homœopathist of the old school, and a modern disciple of Rademacher, will equally claim for themselves, and for the influence and mode of operation of their remedies, credit for much which the impartial, and, perhaps, correctly reasoning physician, regards simply as dependent upon the natural course of the disease. The ordinary practitioner will scarcely hesitate to assert that he has obviated various diseases or abnormal conditions by means of some remedy or mode of treatment. He willingly regards the changes which take place in the state of his patients after the employment of them, as their effects when those changes are favorable—*i. e.*, as the results of his practice, while, if the changes are unfavorable, he attributes them to the natural course of the disease, or to various other influences and circumstances. He will further interpret the effects of his remedies in exact accordance with his own theories and views. Because, for instance, in persons submitted to the influence of anæsthetic agents, intelligence and consciousness, in the first place, then sensation, motion, and respiration, are successively suspended, ether and chloroform are said to act first upon the cerebrum, then upon the cerebellum, next upon the posterior, and, lastly, upon the anterior pyramids of the spinal cord and their nerves. And when their action is extended to the medulla oblongata life is said to hang by a single thread. A Flourens, a Baudens, and others, take this view of their mode of operation because it happens to correspond to their theories of the functions of those portions of the nervous system.

In relation to the accuracy and scientific value of our conclusions, there is scarcely any difference to be found between the ordinary practitioner, or even the homœopath or disciple of Rademacher, and the modern physiological physician; for the endeavour of the latter towards a more scientific advance and judgment is still far from complete, and seldom to any great extent feasible. His interpretation of what he observes, though it may approach nearer to the actual state of things than theirs does, will not be less uncertain and arbitrary. Just so will the physicist interpret the same phenomena and processes in the living body, and even in the inorganic world—*e.g.*, in the air, water, or the soil, with their action upon each other, upon vegetation, man, and animals—very differently from the chemist, and both again differently from the professed physiologist or ordinary practitioner.

In all that has been already advanced many may doubtless, if they are so inclined, find sufficient grounds of discouragement, and feel justified in mistrusting even the first and most indispensable methods of comprehension in medical science. Yet to perceive difficulties does not necessitate that we should allow them to overcome us, and in the case of observation, especially in such a science as medicine, it has already been shown that it is by no means such a simple and intelligible thing as might be at first believed, and that Fontenelle was quite right when he said "l'art d'observer, qui n'est que le fondement de la science, est lui-même une très-grande science."

If we compare, for instance, our present methods of observation and investigation in the whole field of vital phenomena, and even at the bedside, with those employed but a few decennia ago, we cannot fail to recognize a great improvement in them. Not only have we acquired, by the recognition of various anatomical relations and chemico-physical processes, better starting points for our observations, but, by the employment of such aids, the general value of our observations is advanced far beyond that of earlier periods. In illustration of this it may be sufficient here to mention the more frequent and efficient employment of various auxiliary means, such as the thermometer, scales, &c.

On the other hand, it is equally certain that, until we become better acquainted than we are at present with the internal connection and peculiar conditions and laws of the objects of our observation, our interpretation of them must always be more or less obscure and arbitrary. For, in the present state of our knowledge, no one, however earnestly he may strive to do so, can entirely avoid the danger of regarding phenomena and circumstances as determining and essential which are not truly so, and of drawing from this first error a second one, in the conclusion that certain other phenomena occurrences are the effects of these. Thus it remains, at present, always more or less at the option of the individual to explain what he observes in various ways, and to refer it to various causes. And this fact brings us back to the point from which we first started—viz., to the final object of all our observations. This, as we have already seen, by no means consists in simply looking at, and recognizing by the aid of the senses, various phenomena, but rather in approaching, by investigation, nearer to their due comprehension, and to a certain insight into their causal connection and laws. Our aim, therefore, becomes, to observe the phenomena and occurrences with which we have to deal, in the manner best adapted to the attainment of this end; and in doing this, less depends upon the number of cases observed than upon the mode in which we observe and draw conclusions concerning them, although, under some circumstances, the number also is important—*e. g.*, for the establishment of simple empirical facts and laws. A single observation may thus be infinitely more valuable than a hundred others of a different kind, whether it

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be that something entirely new and important has been discovered, a new feature brought to light in some object which has long been under observation, or attention directed to a circumstance previously overlooked. The acumen of a Galileo enabled him to deduce the laws of the pendulum and its oscillations from the sight of a lamp swinging in a church.

The main point must always be to recognize, in a given case, not only the individual phenomena, features, or fragments, that go to make up the whole, but rather that whole itself, with everything of which it is composed. All its phenomena and circumstances must be correctly appreciated as they actually exist, not only in all points, but also without prejudice or onesidedness. Such, at least, are the hints daily given to the observer; but, unfortunately, through the peculiarities of his mental constitution, man is incapable of fulfilling all these requirements to their fullest extent. In the general study of vital phenomena, at least, it is absolutely impossible; and thus, even those who have demanded such things of others have themselves never been able to accomplish them. Every process in the living body, every occurrence in external nature, is so complex, and made up of so many others, themselves dependent upon, or at least modified by, many additional ones, that at present, and doubtless for a long time to come, the most acute and best informed observer cannot hope, in any given case, even to *recognize* them all, much less to submit them all to due investigation. Remaining unaware of the existence and operation of numerous circumstances and influences, he cannot observe *directly* by far the greater number of the phenomena and processes in the living body, especially the most important, but must rather arrive, by various by-paths, at conclusions concerning their reality and course.

On this account, those best acquainted with their subject will the least give way to bitter and dangerous self-delusions; and it is precisely the younger and more enthusiastic observer who has been, perhaps under the character of an innovator or advocate of exact investigation, in our, and in every day, the most given up to them. In practice at the bedside, for instance, such men deceive themselves by imagining that *they* have been the first to observe objectively and completely.

One of them (Reveillé-Parise) has said "On ne dit plus, je crois, je pense, mais j'ai vu," but heaven knows how little it must be, even if it were true or possible! We daily speak, for instance, of having observed fever, dropsy, diarrhœa, or other aggregates of symptoms, while, in reality, we have only observed various phenomena and changes which accompany those diseases, and noticed, superficially enough, certain modifications of the circulation, of the temperature and external condition of the body and its individual parts, and of various excretions, &c. At the same time, we have, perhaps, not endeavoured to determine sufficiently exactly and comprehensively the more constant and often determining phenomena —*e. g.*, the increase or decrease of the temperature of the body by the thermometer, the state of the excretions by the scales and other physico-chemical auxiliary means. For this reason, such observations scarcely deserve the name, for they add nothing to our knowledge, and their results are not to be relied upon.

No one who has compared the older with the more recent methods of investigation, will doubt that medical observation, as connected with bedside practice, has gained much in accuracy and in an objective point of view; that, as regards the physiological investigation of the body, in health and disease, we have learnt to observe a much greater number of phenomena and processes, and to do so much more exactly and comprehensively than was attained to by our predecessors. And if it is denied to us to fulfil the requirement already alluded to—viz., to observe all the features of a given case quite correctly—we must the more strive to accomplish all that is acknowledged to be possible. It will thus always be a main point in our observations, to ascertain as exactly as possible, both in the healthy and in the sick, not only individual fragments of the whole, individual phenomena and occurrences, but rather everything which is in any way accessible to us. It will, therefore, always be more advantageous to determine the temperature of the body by means of the thermometer, and to weigh and test chemically the urine, stools, and other excretions, than, by a superficial examination, to obtain only approximately correct results. Bacon has said, in reference to this point, that we must not fear any bad smells, any "res viles et turpes. Omnia enim munda mundis; et si lucrum ex lotio boni odoris sit, multo magis lumen et informatio ex re quâlibet.[1]"

To attain this object, however, and to observe all the phenomena and occurrences with sufficient accuracy, every distinct series or kind of them, every separate branch of science, requires a particular training of the senses, and, above all, under an able teacher. Only by frequent and correct observation do we gradually acquire an eye, as it were, for everything which is to be seen and observed, and that experience and knowledge of the subject which can alone render a clear and rapid survey and a correct judgment possible. And this holds good alike for the physician at the bedside, the naturalist or physiologist, the moral teacher, psychologist, or statesman; and is true of all whose vocation leads them to observe special series or kinds of phenomena and occurrences. As the practised musician is capable of distinguishing, in a full orchestra, the tones of each individual instrument, while the untrained ear merely recognizes them as a whole, so the practised stethoscopist alone can recognize the finest modulations of *his* sounds so as to found a correct opinion and diagnosis upon them. If the seaman, from certain changes in the colour of the water, can foretell the coming storm, and if the geologist can, from the examination of a single stone, tell the place from which it was taken and the nature of the strata both above and beneath it, so can the microscopist and experienced physician see things for which another and less experienced eye would look in vain. The professed chemist and physicist will observe the chemical or physical elements of the phenomena and processes in the living body far more correctly than the physiologist and physician with their, for the most part, inferior knowledge of the same things. It is seldom given to an individual to be familiar with all the collateral points of his objects of investigation; and if he ventures into a department only half known to him, the danger of errors in his observations must increase in proportion to his ignorance.

All this has, however, no direct relation to medical logic and lies altogether beyond its limits, for it would be a serious mistake to look for the chief office, or most essential attribute of a suc-

[1] Parasceve ad historiam naturalem et experimentalem, Aphor. 6.

cessful observer and proficient naturalist in such technical expertness and practice, in themselves. If our object can be none other than the understanding of what we observe, and the attainment of an insight into the conditions and laws of certain phenomena and processes, the mere observation of them, however correct and exact it may be, can by no means, as so many have believed and as the old "medicina tota in observatione" expresses it, be regarded as the main point.

It is infinitely more important to observe and examine the things presented to us so as to draw from them the safest possible conclusions concerning their causal connection and the mode and conformity of their development and action. We must seek to distinguish, amongst the phenomena and processes in the living body and in external nature, just those which stand in a constant and even essential, or causal relation to each other; and since only aggregates of phenomena and processes present themselves to us, in complex forms, the observer must be capable of representing them to himself mentally, as isolated one from the other and standing in a certain relation to various others, and this without forming any notion of them, or assuming any relations for them, not strictly consistent with the actual state of things. Even the phenomenon which may present itself to us under the comparatively most simple aspect, is so made up of many others, is so dependent *upon* them, and is itself, in many cases, so determining for them, that we have no alternative but to take it to pieces as completely as possible. It is not he who simply sees the things before him, but he who observes them with his mental eye and recognizes the individual elements and parts of which they consist, clearly distinguishing them in the general stream of phenomena and occurrences, who is to be regarded as a good observer. This, as has already been explained in detail, is the first, indispensable condition of all progress in the investigation of the causal connection and conformity of the phenomena and processes with which we have to deal.

In observing them, therefore, it is especially important that we should employ our five senses in the right direction, and turn our attention to what is determining and essential, which must then be made the starting-point for further investigation. But the essential points differ with every kind and series of phenomena and processes in nature; those of the simple chemical and physical phenomena, for instance, from those of innervation, or the more complex processes of nutrition, conversion of tissues, and excretion in the living body.

Our views, also, as to what really constitutes these essential points have always varied and will continue to do so with the stage of development of our knowledge in each branch of science. What appeared to the observer and investigator of a century ago to be determining and essential in the things with which they had to deal, no longer appears so at the present day, and a Paracelsus might well regard many things in a light which would now justly procure for any one the reputation of an obscure and fantastic thinker.

Neither will the exercise and correct employment of the senses, in themselves, though aided by all the auxiliary means possible and by the best and most accurate instruments, enable us to discover, amongst the mass of phenomena and occurrences before us, what is strictly essential. Our best aid here must be sought in an adequate knowledge of our subject and an acquaintance with all its collateral points and questions, and in a faculty of combination derived from natural tact, to be improved by practice. Our task is, to discover, in the aggregate of phenomena before us, just those which stand in a constant and essential relation to each other, and to ascertain, amongst all the individual processes, the particular parts of them upon which certain others depend. And yet, as they present themselves to us in nature, they give the observer no indication which of them, among many others, may be the essential and determining ones. We are, therefore, reduced to the necessity of forming conjectures so as to gain a more or less clear idea as to the probability of certain mutual relations of the phenomena and processes which we are about to investigate,—and we must, at least, be able to form an approximate notion of what is essential in a given case, and where it is to be looked for. Just as any one who is seeking for an object which has been lost would naturally think of all the places in which it can possibly be, and search, first of all, in those in which it is most probably to be found; and by this means would avoid many fruitless attempts.

Scientific discovery thus demands something far removed from mere good luck and accident, something also far beyond the keen and practised employment of the senses; it asks for mind, a clear and unfettered survey, and an almost poetically creative imagination or capability of construction. The discoverer and successful finder must already, to a certain extent, be aware of a connection between the things he desires to investigate; he must have acquired, whether by simple observation, or by the aid of experiment, some previous notion, or at least foreboding of their nature, before he can advantageously seek for and, in the end, find them. For this reason a blind Newton, Dupuytren, Rokitansky, or Liebig, would doubtless have seen and discovered more than a hundred ordinary observers in full possession of their eyesight; just as Huber, notwithstanding his blindness, penetrated the mystery of the difference of sex and mode of propagation in bees, which others, with all their eyes, had failed to do. Thus we have not owed the most important discoveries which have been made in astronomy, in the living body, and in other objects of research, in all cases to those who have possessed the most complete apparatus for the purpose. The unfortunate Keppler, though persecuted almost to the death by bigots and others, made many of his most important observations upon the Prague bridge during the period of his heavy affliction, and Olbers discovered two new planets from the balcony of his house.

On a closer examination of the method by which men have arrived at their discoveries, we find that, having first come to regard various things as possible, or even probable, which had not attracted the attention of any one else before them, they have then pursued their investigations in the particular direction to which they may have been led by a more comprehensive and penetrating contemplation of the phenomena and processes in question than others had attained to. From this they have passed to the examination of new features, to which, in comparing them with other phenomena and objects, their attention may have been so drawn as to render them alive to the importance of previously unrecognized analogies. But this, the chief element of successful and productive observation, is most unfortunately neither to be learnt nor taught. Like every other talent, it is innate, and like every other it may be exercised and developed, both by constant and appropriate employment in the right direction, and by the example of able teachers in each department of science. To the physician in particular the individual observations of a Cruveilhier, an Andral or a Stoll, may render more service in this respect than the most complete compendium of nosology; and the *post-mortem* discoveries in individual subjects, taken up and elucidated by a Rokitansky, may do infinitely more towards enabling him to recognize and appreciate anatomical changes correctly than the 'Handbook' of the same physician, excellent as it is.

And it is precisely the interweaving of our previous knowledge and surmises, of our personal views or notions, with the investigation of the actual state of things, which forms a connecting link between the whole process of induction *a posteriori* and hypothetical deduction *a priori;* between the simplest and most sober investigation, by the aid of observation and experiment, and mere speculation. However dissimilar these methods may appear to be, and however much they are really opposed to each other in certain respects, there is, nevertheless, an internal connection between them as much as between the general activities and tendencies of the human mind which are the least apparently similar. The special active element—viz., the intellectual nature proper to man—is essentially the same in them all. The career of all successful and eminently intellectual investigators has been but an illustration of the intrinsic oneness of the various methods by which investigation is pursued.

The man who is best able to recognize new and important features in the things known or assumed before his time will always be the one to make the greatest number of discoveries, to throw the most light upon departments previously obscure, and to ascertain entirely new and more correct relations between the phenomena, &c., with which he has to deal. For all this presupposes that the various doubts and questions connected with the subject must have been present in his mind; that he has weighed within himself the sufficiency of the things which others have been content to take for granted, and adopted among them only such as, after the ordeal of profound reflection and exact investigation, he found answered by a secure affirmation.

Newton, when asked how he had arrived at the discovery of the laws of motion of the planetary system, could give no other answer than that "he had meditated constantly upon the subject;" and when Bentley admiringly and politely remarked, that "Newton had killed all the game, and left nothing more to discover," the latter said, with more reason, that "there was game enough in every bush, if one only knew how to start it."

Experiment has, in the course of time, become the most efficient means of starting and running down this "game;" but since such direct attempts are, fundamentally, nothing more than a particular kind or method of observation, all that has been said upon that subject may safely be applied to experiment also. The boundaries between them are still further effaced by the circumstance, that, in certain respects, transitions, or intermediate steps, exist between experiment and simple observation. If we place a living being—say a man—in certain new, unusual circumstances, for the purpose of maintaining or restoring his health, which is but another way of expressing that we have placed him under the influence of some remedy, we already perform a kind of experiment; for, in all such cases, we no longer observe the natural, undisturbed course of things, but various modifications of it, resulting from our own interference. We experiment, to a certain extent, upon the patient, as the physiologist does upon animals, but do not use the term "experiment," because, in this case, we have no scientific object, but rather a purely practical one, in view, and because we do not employ these means for the furtherance of knowledge, but to promote the health or cure of a fellow-being.

The peculiarity in which experiment differs most essentially from simple observation is, that in the former we are not called upon to observe certain phenomena and occurrences simply as they present themselves to us in the ordinary course of things, but under circumstances which we have ourselves produced artificially for the purpose of better investigating and becoming acquainted with those phenomena, &c. We produce certain changes in them; we bring the phenomenon or occurrence in question into various new relations, and cause the circumstance or influence we wish to investigate to act in various ways; we combine or isolate them, for the purpose of arriving at a better understanding of their conditions in the one case, and of their effects in the other. Man takes the initiative, as it were, of nature; and, by placing himself in a more favorable position, extracts from her clearer and more definite answers. Thus, Bacon was correct when he termed experimentation a "vexatio naturæ;" for in it we bring various phenomena and things, various occurrences and circumstances, to the tribunal of our judgment, as witnesses or proofs, which would never have come of themselves, and still more certainly would have given no evidence, at least not of such a character as to enable us to arrive at an insight into the real course of things. What was previously dumb is made to speak and to answer to our questions, and we are enabled, in a certain sense, to endow even stones with language.

But if experiment has become one of the most important, and even the determining method of arriving at a knowledge of nature, it was far otherwise before a powerful impulse had been given, in particular by Bacon, to this method of investigation. It lay in the very nature of things, that the mind of man could only arrive gradually at the employment of this method. It was necessary that he should already, by simple experience and observation, have acquired a certain knowledge of nature; that he should first have directed his special attention to various things, and learnt to take a more lively interest in them, before he could proceed to this artificial mode of interrogating her. It is well known to how great an extent these natural difficulties were enhanced by superstition and by religious and philosophical tendencies in general, even in the 17th and 18th centuries. It was necessary that a Bacon should endeavour to prove that the majesty of the human mind is not degraded by this sober and exact investigation of nature, and that man is neither too dull to attempt it, nor incapable of carrying it out. Even to the present day, we have not learnt to employ experiment in the investigation of disease, with reference to the development, course, and internal connection of its peculiar conditions, either much better or much more productively than the alchemists, for instance, did with reference to chemical processes.

But here, also, as was proved with reference to simple observation, it does not fall within the province of logic either to lay down rules for our path, or to direct us how to proceed in it. This belongs to the respective branches and departments of natural science; and all that can be of interest or benefit on this subject has already been laid down when speaking of the process of induction. The work of logic here is that of dealing with the correct appreciation of results, and of the inferences to be drawn from them. Its office is, therefore, to give certain principles and aids for the arrangement of our artificial attempts, so that we may be able to draw from them, when fairly carried into practice, safe conclusions concerning the question at issue.

Here the same rules hold good, and the same points are to be kept in view, as in the case of simple observation. Experiment must, above all, be carried out in the manner, and under the circumstances, which may best enable us to attain our object —*i. e.*, the elucidation of the question at issue, and the better understanding of the objects it is concerned with. But the phenomena and occurrences themselves, which we thus give rise to artificially, or which we have, in various ways, changed, modified, and placed in various peculiar circumstances—in short, all the things which occur during our experiments and all that takes place in connection with them, must be observed and interpreted as carefully and objectively as any other phenomena in the ordinary course of things. It is also certain that the rules for observation and progress in this method of investigation will always undergo certain important modifications in consequence of the peculiar nature of experiment, and of the method in which it is brought to bear upon the particular objects it may have in view. On a closer examination, it will become evident, namely, that if, in many respects, experiment possesses essential advantages over the simple observation of phenomena—*i. e.*, as they present themselves to us in nature, it has, on the other hand, its especial dangers and difficulties.

The chief superiority of experiment, as an aid to investigation, must always consist in the circumstance of its being the sole engine by which we can ascertain, at least approximately, what are, in any given case, the effects of the individual agencies of the external world and of the various influences which affect the living body, and what is to be laid to the account of the action of the individual processes in the latter and of their dependence upon each other and upon things external to it. We are thus able to place them, one by one, on a fixed and artificial plan, in various circumstances; to bring them into such relations, and to observe them in such junctures as will enable us to draw more certain conclusions concerning conditions in the one case, and effects in the other. For, as has already so often been insisted on, everything which we meet with in nature is much too complex for us to hope to discover from it, as such, what is determining and what is dependent, or to decide upon what may be the causal connection and laws of operation of those individual agencies and influences, or the conditions and internal conformity of any individual process in the living body. If, therefore, we are ever to acquire clearer notions on these points than we possess at present, we shall evidently be indebted for them almost exclusively to experiment, which affords us the above-mentioned advantages in investigation, and which we may in vain look for from the simple observation of nature.

Experiment places it in our power to produce the particular kind of circumstances under which the given effects of an influence, the given conditions of an occurrence or effect, manifest themselves most clearly, and allows us to arrange everything for observation, just as is requisite for the discovery of the causal connection and conformity to fixed laws of the phenomena which we are seeking to investigate. It is only by artificial attempts that we can isolate an individual phenomenon, an individual agent or influence, for the purposes of observation, without at the same time being compelled to include so many other elements as always present themselves in the natural course of things. We change, to a certain extent, the order of nature, and create a something for ourselves which we can better bring within the scope of our comprehension. We make things which, taken in themselves, are intricate and obscure, in consequence of the endless variety and complexity of their collateral circumstances, more clear and simple; we bring to light, and render more accessible to the investigation of our mental eye, what nature has, so to speak, concealed. In one case, we free nature from her bonds and coverings, that she may speak to us more clearly and openly; in another, as the case may be, we confine, change, or take her to pieces, that we may hear the tones, and observe the phenomena which our object requires that we should recognize, disguised as little as possible by others.

In short, if we have, as simple observers of nature, been more or less her slaves, experiment makes us, in a certain sense, her masters; and if this, for reasons already mentioned, occurs much less frequently in our special department of science than in those of the physicist and chemist, investigation, if in a less high degree, still obtains for us the advantage of less obscure answers to our more definite questions.

A further advantage, which nature would seldom if ever afford us, consists in our being able to renew our attempts as often as appears necessary in each individual case, and, in this way, ascertain more easily than we could otherwise do, the conditions of a given process or occurrence, and the mode and laws of operation of a given influence or determining circumstance. And we shall the better succeed in this the more we are able to vary the circumstances in which we choose to place the occurrence or influence in question, and to observe its bearing under them. We may thus, in the end, ascertain in the one case all the essential and determining circumstances required for the development and manifestation of a given effect or phenomenon, and in the other, the varied modes by which a given circumstance or influence acts, and learn how it bears upon other phenomena and occurrences. Every one knows how much our knowledge of many vital processes has been increased by the repetition and variation of experiments, and how much more familiar we have thus become with the phenomena of light, heat, and electricity, with the laws of gravitation and hydrostatics, in short, with all the chemical and physical processes, and their conformity to fixed laws, not only in external nature, but also in the living body itself.

It follows from all this, that experiment facilitates our investigations and struggles for knowledge much more than simple observation, because it affords us more convenient and infinitely more direct and certain means of attaining our end, and because we can adapt it to our particular object in each case, and can observe the things to be investigated as frequently as is necessary under the precise circumstances which appear most likely to further our knowledge of them.

But experience has shown that it is from this very quarter that the accuracy of experiment, and the whole reliability of its results, is most endangered. We incur a greater risk here, in certain respects, than can be laid to the account of want of skill in our attempts, or want of care in making them. The latter evils are always to be avoided, if we will, but the facility with which we may be deceived and led astray in every artificial attempt, by our personal aims and tendencies, lies in the very nature of such attempts, as well as in that of the subject and of man himself.

If, for instance, in simple observation, we are apt to confound our own interpretation and explanation of what we have seen, with what we have really seen and observed, or to throw the two into one, the danger which, in the case of experiment, is entailed upon the accuracy of our conclusions, must be doubly great. For here our *ego* intervenes, and our notions and arbitrary views play a much more important part in opposition to nature and the actual state of things, than in the simple observation of the latter. Our intention to seek a certain something, and to see and find *it*, and not anything else, stands out much more prominently in the one method than in the other, and has often indeed been our only inducement to make the attempt. Here, therefore, more than in any case of mere observation, we incur the risk of conceiving and judging of what we have recognized by the senses, and which really exists and occurs, precisely as best corresponds to our views and notions, to our intentions or wishes. In short, nowhere so easily as in experimentation, can the objectivity and accuracy of observation be obscured or rendered altogether illusory by the subjective intervention of the individual observer. Whether it be that we are led away by interest, a too lively imagination, or actual prejudice, or that, from indolence or too great eagerness to find what we are seeking for, we strive to obtain a maximum of demonstration with but a minimum of observation, the fact remains clear that we are apt to see things which have no real existence, and to overlook or misinterpret others which are actually present. And in our appreciation of the results of our experimentation, kindred dangers will beset us, just as the aggregate and variations of phenomena with which our subject presents us, may happen to be numerous and complex, dubious and enigmatical, in any given case. For under such circumstances, the very nature of the thing itself gives much more scope for an arbitrary selection, and, consequently, for subjective assumptions and explanations, while, on the other hand, there is a greater or less deficiency of fixed points upon which the mind may rest, of firmly established and generally acknowledged facts and laws, by which every one must perforce abide in his attempts at explanation.

We can easily understand from all this why the facilities for error are nowhere so great as in our peculiar department of science, and the history of medicine up to the present day furnishes sufficient proof of this. Yet even physicists and chemists are frequently led into error by their experiments, especially when these are undertaken for the direct demonstration of their hypotheses and views at the time. Here also they may easily err in such of their conclusions as refer to the more enigmatical and complex, or less firmly established phenomena and occurrences in their respective sciences. The chemist, for instance, in reference to organic combinations in the living body, and to the chemical processes at work in it, and in his whole calculation of probabilities concerning them; the physicist, in reference to the electric, magnetic, and meteorological phenomena, and to those of light and heat, &c. Any one who has passed a considerable portion of his life in experimenting upon the electricity of the atmosphere, and has directed his attention to that especial subject, will not, it is true, easily assume its presence erroneously, and still less will he be likely to overlook it where it is really present and in operation. But he may be easily led into the error of ascribing to it a greater amount of influence than it really exerts; in relation, for instance, to the more complex processes in the atmosphere, such as storms, whirlwinds, and the like. He will not always be able to resist the temptation of regarding electricity as the determining cause of various phenomena and occurrences, which are, indeed, observed simultaneously, but of which the conditions and causal connection may be entirely different, and in which electricity itself is perhaps but a dependent and secondary phenomenon.

That more or less similar impulses frequently lessen the reliability of physiological experiments, and interfere with the correct appreciation of their results, is well known. But this is the case in a much higher degree when a certain personal interest on the part of the experimenter comes into play, his eagerness, for instance, to demonstrate and defend some view, or conversely to refute it; still more when the results of a given experiment admit of a practical application in life, and the experiment has, perhaps, been made solely for the furtherance of practical objects, such as the diagnosis or cure of a disease, the demonstration of the benefit or injury resulting from the adoption of certain hygienic measures, or influences, or from the administration of certain drugs. It is true that perhaps no attempts are more important and useful than such as these, but in none, on the other hand, are errors in the interpretation of their results, or false conclusions drawn from them, more dangerous. Thus it was, perhaps, perfectly legitimate to endeavour to render the diagnosis of gonorrhœal and syphilitic affections as exact as possible, even by inoculating the persons affected with their own virus (Ricord). It is now known, however, how much mischief may be done by these means, while the discoverer of them has represented them as perfectly harmless, and is still convinced that such is the case, because he interprets certain changes and deteriorations in his patients in a different manner to his opponents, and cannot see in them the effects of the inoculation. It is also well known that we form conflicting and therefore erroneous opinions with regard to the effects of our drugs upon the healthy and sick, and upon animal life in general. These have their origin partly in the personal interests of the observer, partly in the neglect of indispensable precautionary measures in the arrangement of our experiments, and still more in a faulty appreciation of their results, and of the conclusions to be drawn from them. Thus it happens that, in the case of almost all the conclusions of medical science, there is left, as in the results of simple observation at the bed-side, an opening for doubt and contradiction.

All this tends to confirm the general requirement of logic, that we should never rely fully upon any single direct experiment, in itself, and still less regard any hypothesis or previous view as proved by our direct experiments, unless we have obtained such clear and definite results as will not leave any other interpretation than the one put upon them possible. The greater, therefore, may be the interest we take in the results of any experiment, the more caution must we exercise in the explanation given of it, and in the conclusions drawn from it. We must not be over-hasty in deducing anything from individual experiments, because it happens to correspond to our expectations and wishes, but should, for this very reason, examine the more carefully into the possibility of a deception. Bacon, therefore, had good reason for wishing that his "experimenta lucifera" should rather be undertaken as "experimenta fructifera," or, in other words, he greatly preferred experiments which further our general knowledge, to those which afford the prospect of an immediate application of their results to any particular practical purpose, whether it might be to commerce, to the arts, or, as in the present case, to therapeutics. And if the results of any series of experiments are of such a nature that only doubtful conclusions can be drawn from them, sound logic requires that this fact should be clearly recognized, so that we may not represent them to ourselves as anything better, or more trustworthy than they really are.

The whole reliability and conclusiveness of an individual experiment and its results must vary according to the complexity and general character of the phenomena and things experimented upon, and according to the question which is sought to be answered, or, at least, rendered less obscure by them—in short, according to the nature of all the points connected with the problem. The simpler may be the circumstances under which we perform an experiment; the less complicated are the relations in which its object stands to other objects; and the less fluctuating are the modifications it is in consequence exposed to, the more exact will be the conclusions we are likely to draw from it.

Thus a single experiment, correctly performed and observed, may serve to answer a given question with a degree of certainty which could not, in the case of many other questions, be attained by hundreds of experiments performed under the most varied circumstances, and this because we have been able, in this one case, as not in the others, to test and confirm our observations by the direct and easy aid of the outward senses.

A small number of experiments, in some cases one only, has sufficed to show that water and other fluids will strive to enter vessels exhausted of air; that quicksilver and other metals expand by heat; and that prussic acid, chloroform, carbonic acid, &c., in large quantities, are fatal to men and animals. And if, on any single occasion, powdered charcoal, after having been swallowed by an animal, had been found in its blood, or in the interior of its organs, in short, in places where experience had taught us that carbon in this form is never found, the fact of its ingress into those parts of the body would be proved with certainty.

In a similar manner a small number of experiments have been enough to show the effects produced on the living body by various mechanical injuries, such as the division of the spinal cord, or pneumogastric nerves, or the extirpation of an organ, or in the administration of various drugs or poisons. A few experiments would, under certain circumstances, sufficiently establish the fact that opium, chloroform, and morphia produce stupefaction or insensibility; that they alleviate or entirely remove pain, neuralgia, &c.; that ague may be cured by quinine, and bronchocele by iodine; so soon as we had learnt by experience that the pain, the ague, or the bronchocele would otherwise, if left to themselves, have continued a certain time, whereas, after the employment of these agents, they usually cease very rapidly. But if, in all these cases, we should wish to become acquainted with the details of these effects, or even to ascertain experimentally the mechanism and causal connection of the phenomena we have recognized through the senses, an entirely different series of experiments, with manifold variations of the circumstances, will become requisite. By such means only can we arrive at the clear understanding of what we have previously known merely as facts established by simple experience. To be able, for instance, to judge of the effects produced by the division of the pneumogastric nerves, we must first observe the changes which take place, the processes and functions which become deranged, such as contractility and secretion in the stomach and intestinal canal, the solution and digestion of individual articles of food, various processes in respiration and excretion, the generation of heat, &c., and investigate each of these separately.

Thus fresh demands for the arrangement and performance of our experiments constantly present themselves according to the particular kind of objects, phenomena, and processes to be investigated, and according to the precise nature of the questions to be answered. They will differ when the question refers to simple physical and chemical processes, and when it has to do with those in the living body; and also vary in the simple establishment of various facts, and in the investigation into their causal connection, conditions, and laws. It is the province of the respective branches of science to explain all these various requirements clearly in their several order, but the discovery of the causal connection and conformity to fixed laws of certain phenomena and processes will always, in a scientific point of view, continue to be the main object of our experiments. We cannot content ourselves with mere curious discoveries, while, for various practical purposes, our need of scientifically correct deductions remains so great.

To obtain more definite explanations on these points by the aid of experiment, the questions to be answered must, in the first place, be put as simply and clearly as the existing state of our knowledge at the time renders it possible. We must also be able to judge which of the various phenomena, effects, &c., occurring in the course of our experiments, is to be ascribed to the circumstances and changes produced by us, and which, on the contrary, may have had its source in circumstances and influences entirely different in their nature, though operating at the same time. That the opinion we form of them may be rendered as easy and correct as possible, and that the phenomena, changes, &c., which we observe in them, may not admit of different or even erroneous interpretations, the latter must be as little complicated by others as possible, and least of all by any whose conditions and laws are unknown to us. Our experiments must, therefore, always be undertaken under the simplest possible circumstances. All the collateral circumstances and influences which may exert a modifying influence upon a given occurrence, or affect the result of a given experiment, must be excluded as completely as is practicable, or, where exclusion is not feasible, their presence and effects must be recognized and controlled.

In all these respects, however, medicine, for obvious reasons, is less favorably situated than other sciences. We labour, in comparison with the physicist or the chemist, under an especial disadvantage, inasmuch as we are unable to produce vital phenomena and processes at will, or even to experiment separately upon those which occur in the living body, in isolation from the various influences which affect them. For if we seek to experiment upon the living body under circumstances so complex as those met with in nature; if, for instance, we try to produce artificially the aggregate of phenomena existing in any actual disease, or to bring a complicated amount of influences to work, as they do in nature, upon the human body, we are not, with all this, any nearer the explanation of their conditions or laws of operation, in short, are as far as ever from the secret of their causal connection; as far, that is to say, as the simple observation of the occurrences in nature left us. Our experiments would only teach us that certain phenomena, events, and effects are wont to manifest themselves, and to follow each other in a certain manner, but we should not be any nearer ascertaining their internal mechanism, or the conditions or laws of their development and course.

If we were able, for instance, to produce artificially all diseases as they may present themselves in man (*e. g.*, gout and lithiasis, scurvy, ague, typhus, &c.), we should not, by this means, advance much further in our knowledge of them—*i. e.*, not beyond the *post hoc propter hoc*—any more than a physiologist who attempted to produce a living body artificially. For it may be foreseen that causes and effects would everywhere be complicated to such an extent that we could never, from the occurrence of the latter in themselves, acquire an insight into their conditions and causal connection. We have not succeeded in doing so, for instance, in reference to small-pox, syphilis, or glanders, although we have long been able to produce these diseases by inoculation; for we have never succeeded in ascertaining what part each of the specific poisons plays in these "contagions," or the internal mechanism by which the diseases they produce are developed. And although millions have been vaccinated for the purpose of protecting them from the contagion of small-pox, many even now regard the reality of such protection as by no means demonstrated. Essentially the same thing holds good of our experiments with drugs, especially in the sick, because the circumstances are too complex and fluctuating, and because too many other things, concerning the influence of which we are at present unable to form any clear notion, modify the result, whether this be a cure or the reverse. Even in experiments upon animals, it often remains doubtful what part of the changes produced—*i. e.*, of the supposed effects of our drugs—is attributable to the circumstances and influences brought into operation (*e. g.*, a remedy, a poison, or a mechanical lesion), and not rather to entirely different concomitant circumstances and influences (*e. g.*, changes in the atmosphere, in nutrition, or in the habits of life during confinement). This applies more particularly to experiments extending over a considerable period of time, in which no means which act so rapidly and directly as strong poisons or the knife, are adopted, but where we rather seek to ascertain the manner in which certain drugs, aliments, or variations of temperature act upon the human frame.

But however seriously the attempt may have been made, the human mind has not always succeeded in overcoming such natural obstacles, or in arriving at its ends in spite of them. As in the perception of the physiological relations in the healthy living body, so also in the perception of diseased conditions and of their development and cure, we shall always receive the more aid from experiments the better we are able to adapt them to the particular questions to be answered.

The nature of these requirements in the carrying out of our experiments follows from what has been advanced in Section V concerning the analytical or deductive method. We must, first of all, seek to ascertain the individual processes in the living body, with their conditions and separate action, by the aid of artificial means, and next trace the laws of operation of the individual influences which affect them, whether they be hygienic or therapeutical. Just so we must commence with those parts or elements which are comparatively the most simple and accessible to experiment; first of all, therefore, with the individual physical and chemical processes in the living body, and in external nature which surrounds and affects it.

And for the attainment of the main object of our experiment, *viz.*, the exact determination of the causal connection and conformity to fixed laws of the occurrence or influence in question, it must be placed artificially in the most varied circumstances possible, and its behaviour in each case, with the modifications it undergoes, carefully observed. Hence follows the further requirement, of investigating separately, by the aid of appropriate experiments, the action of all the modifying influences and circumstances which may happen to be in operation at the time, and which may easily render the result obscure, so that we may be better able to recognize them as essential or the contrary, and, if it be necessary, to exclude them with some degree of certainty.

Until this has been done, all the other circumstances connected with our experiments must remain exactly alike, and all the so-called accidental influences, in short, everything which may, in various ways, affect and modify the results, must be excluded as far as possible. Thus, if we wish to investigate experimentally the influence and mode of operation of a given substance, or of a certain degree of temperature, upon men or animals, we must, first of all, take care that the latter are themselves alike—*e. g.*, of the same age, and in the same state of health; and the same conformity must extend to their food, mode of life, &c. This precaution must especially never be neglected where, in consequence of such differences, any variation or modification of the general result might be expected to occur; where, therefore, a doubt might arise whether, and how far, the result is to be attributed solely to the circumstance brought into operation intentionally in the experiment, or perhaps to those other collateral circumstances and influences.

Experiment, to accomplish all this thoroughly, presupposes a sound knowledge of the subject, and a definite plan, based upon this knowledge, which must be carried out consistently and circumspectly in the whole investigation. And this is nowhere more necessary than in reference to the problems and experiments of the physiologist and the physician, if they would arrive at conclusions in any degree accurate. In other respects, the same rules hold good here, as for observation in general.

Experiment will always afford the most important and genuine discoveries to those who not only confine themselves to a single question or object of investigation, but also select such as are most clearly defined in their outlines, and examine them as thoroughly as possible in all their relations. Instead of selecting for experimentation vague and ill-defined phenomena and objects, and for that very reason investigating them in a more or less superficial or onesided manner, we must select simpler ones, such as we can investigate as comprehensively as possible under a great variety of circumstances. It is always more profitable to advance from the simple to the more complex and difficult, than to adopt the contrary method. And just because this simplest and most natural of all the rules of logic has seldom been observed in medical science, above all in Germany, where the attempt has so frequently been made to arrive at an immediate understanding of the most complex processes by the aid of experiment, as well as by simple observation, scarcely any clear insight has yet been acquired into the nature of those which are comparatively most simple.

By commencing with what is most simple, we constantly, in the course of our investigations and experiments, discover new features, new relations of the object before us to numerous others, relations the existence of which we had perhaps previously never suspected. In the investigation of the latter we also obtain infinitely more certain and definite results, because we are able to connect what is new, and still obscure, with what is already more familiar, and to deduce the complex from the more simple—*i. e.*, from the laws of its individual elements. And since the science of medicine may safely be regarded as the most intricate and obscure of all sciences, we should not seek to attain our final object—*i. e.*, the comprehension and explanation of processes and circumstances still enigmatical—directly by the aid of experiment, but must rather content ourselves with mere preparatory and approximate results. We shall approach the nearer to this object the more we succeed in imitating the methods of experimentation adopted in those other branches of natural science which have so far outstripped our own. For as, by the results obtained and by the principles inculcated in them, a new light has been thrown upon many obscure points in the vital processes, and a new and more productive direction given to our own investigations, so the technical arrangement, and the accuracy of experiment can, doubtless, only gain by the imitation of the methods of investigation which they have adopted.

For the solution of the problems of medicine proper—*i. e.*, of the theory of diseases and their cure—experiment has, on the whole, been rarely employed, and that only in a very deficient manner. We have advanced but little from the stage of the simple observation of our phenomena, &c. (which goes but a short way in helping us to the full understanding of them), towards that of intentional experiment artificially carried out; or, at most, we have but ventured upon the first uncertain steps in this direction. And this is the more to be deplored, because it is only through experiment that we can expect to be enlightened as to the conditions by which diseases act and develop themselves; in no other way can we learn the internal mechanism of their individual processes, or, acquainting ourselves with their conformity to fixed laws, form a probable conjecture as to the effect of our remedies in bringing back a healthy and normal state.

In relation to the employment of the experimental method, we stand much upon the same point as that upon which chemical experiment stood in the times of phlogiston, or even of alchemy; and as there the phogiston hypotheses so long stood in the way of every more exact investigation, so *our* attempts have been impeded by the old hypotheses of an "archæus" or a "pneuma," and by the more modern ones, of a vital force, a typical or natural healing force, and lastly, by the so-called ontological conception of diseases as special entities, no less than by the belief in mystical medicinal forces. But as, in chemistry, weights and numbers, and the more accurate methods of investigation, and consequent scientific comprehension, made their way in spite of all opposition; so in our department of science, a similar state of things must evidently gain ground in the end. In relation to mechanico-physical and chemical phenomena and processes, at least, a good beginning has already been made. Surgery, however, with its mechanical lesions and their cure, and toxicology with its poisons, have left therapeutics and the theory of diseases far behind.

The latter branch of science has not even been able, with all its experimental investigation of the external world and its individual influences or agents, to seize upon the precise points and features which are most important for the physician; for, with few exceptions, we have not yet clearly ascertained, by the aid of experiment, how and why, in consequence of their action, certain diseased conditions, or certain elements of disease, or more simple derangements of certain processes in the living body, are accustomed to arise. With reference, also, to our drugs and curative agencies, we have scarcely begun to investigate, experimentally, in what manner, and to what extent, it is practicable to induce the retrogression of various derangements of the vital processes, and to promote a consequent return to a healthy condition, and this, because we know, as yet, next to nothing of their actual operation in our patients, or of the manner in which they may possibly affect the internal mechanism of diseases—*i. e.*, those individual processes or occurrences in the living body which have deviated, in various ways, from the normal condition.

We everywhere stand in need, therefore, not only of experiments generally, but of such as are of an entirely peculiar kind—*i. e.*, such as are carried out according to other more strict and productive methods. But as in all medical inquiry, so, also, in experimental investigation, it is that of the simplest elements of a disease, and of its individual producing causes, which is most important. Instead of contenting ourselves with what appears to be nearest at hand, and is, perhaps, most requisite for certain practical purposes (*e. g.*, for diagnosis and the simple empirical treatment of our patients), and not even, as so frequently happens, carrying out this intention with certainty, we must first seek to acquire, by the aid of experiment, a certain knowledge of the things with which we have to deal—*i. e.*, to determine, more and more exactly, their causal connection, and the laws of their development and action.

The so-called disciple of Hippocrates, or the empiric of our own times, can no longer smile at these efforts as vain, ignore them as the offspring of theoretical and dogmatical aspirations, or pass them by without any lively interest. With all his so-called experience, he has not, in reality, learnt to understand anything, nor will he ever do so by the means which he adopts. He can, therefore, have no just grounds for complaint, if we declare his methods of investigation unproductive for science, and incline to discard them from its service.

II. HYPOTHESES, COMPARISON OR ANALOGIES, AND THE GENERAL CALCULATION OF PROBABILITIES.

Enough has already been said in the foregoing pages to show why, with all our knowledge and perseverance, we must at present and for a long time to come, perhaps always, be restricted to more or less probable conjectures in medical science. Allusion has also been made to the important services which may be rendered by the adoption of experimental and preliminary notions, and by the comparison of things still obscure, with others which are more familiar and, perhaps, analogous—in short, by an approximately correct calculation of all the arguments for or against any hypothesis, or view. These means must also not be regarded as merely the first preparatory steps to further investigation, but also, in the present state of things, as a temporary substitute for established truths and positive knowledge in medical science, perhaps for centuries to come. In every department of human knowledge the auxiliary or compensatory means, however fluctuating and unreliable, are the more important, and must the more frequently be employed, as far as possible, in proportion as our insight into the phenomena and processes in question remains imperfect, and the ascertained and scientifically established laws obtained for them are less numerous.

Whether we are called upon to form an opinion concerning the development and conditions of any process or occurrence (*e. g.*, a disease, the effect of an agent of the external world, or of a remedy), or whether we desire, as practical physicians at the bedside, to exert some influence upon it, the uncertain character of our notions remains essentially the same. For we have everywhere only approximate, more or less vague grounds for the view we take, while any resting-points, based upon positive and exact knowledge, are for the most part wanting. But if it be denied us to determine or explain anything with perfect accuracy, and to base our practice upon firmly established principles, we must the more earnestly strive to ascertain, at least, what is most probable. Here the calculation of probabilities is our best aid, which the physician will have as much occasion to employ at every moment and at every step as the statesman or the moral teacher in their several departments.

Which amongst the many circumstances and influences to whose operation a given patient has been exposed are precisely those upon which his falling sick has depended? How are the various symptoms and derangements observed in him connected with each other, and what changes and processes are to be regarded as essential and determining, what on the contrary as comparatively accidental and subordinate? By what precautionary measures may an attack of disease be best prevented, under given circumstances, and by which of the many means at our disposal can the disease, when present, be most certainly and rapidly cured? And lastly, is it probable that the patient, in his peculiar circumstances, will recover from this or that particular disease, and that, too, rapidly and completely, or is the reverse of all this rather to be expected? These and similar questions are those which the physician is called upon to answer at every moment.

For this our answer, however, we nowhere possess an adequate amount of knowledge, but only, for the most part, a doubtful and approximate insight into all the points, without a correct appreciation of which that answer must be very indefinite, if not altogether impossible. We have, in reality, even under the most favorable circumstances, only more or less probable grounds upon which to base our judgment, and it is by means of the calculation of probabilities, which cannot, moreover, be taken in the strict mathematical sense, that we must endeavour to form the most correct possible conclusion from such data, notwithstanding their uncertain character, and to approach as nearly as possible to the truth.

Here again, it is not the province of logic to teach us how to employ exactly, or arrange, as it were technically, the calculation of probabilities in any individual case. For such knowledge depends entirely, in each branch of science, upon the questions and objects of investigation, and upon the special circumstances in any given case, and there only can the requisite details be explained. It is more important here to examine into the services which such a calculation of probabilities may render us, and to ascertain more exactly what we may hope to attain by its aid.

Our task here, as well as in simple observation, consists in showing in what points of view and with what precautionary measures these hypotheses and the calculation of probabilities in general are to be applied, and by what proofs from entirely different quarters they must be supported, that we may at least rely upon their results and upon the conclusions we draw from them, as far as their uncertain nature will permit. For whether we avail ourselves of them for the discovery of the causal connection, conditions, and laws of certain occurrences in nature—in short, as the means of investigation and for the acquirement of knowledge—or for the attainment of certain practical purposes, the main point will always be, to employ these auxiliary means and processes themselves as consistently as possible. In other words, therefore, we must endeavour to discover, amongst so many possible hypotheses and analogies or comparisons, precisely those which are most correct and which approach nearest to the true state of things, and, with our more or less indefinite materials for calculation, to draw our conclusions as correctly as possible, and to ascertain what is most probable.

The misuse of hypotheses, and the many errors and delusions to which they have led, have brought them almost entirely into discredit, not only with physicians, but also with other investigators in natural science. It has often been thought that they could not be avoided carefully enough, especially in more recent times, since the so-called more exact methods of investigation have begun to be employed even in medicine. Nearly the same thing applies to the employment of simple comparisons and analogies, for here also men have not scrupled to group together, superficially and arbitrarily, things which, in reality, offered scarcely any points for comparison, or of which the one was still as little understood as the other. For that reason the comparison of them with each other, under existing circumstances, could only be uncertain and arbitrary, and our knowledge of them, at best, but little increased.

In spite of all this, hypotheses, uncertain comparisons and analogies, and the conclusions drawn from them, have always been avoided nominally only, nor could this be otherwise, simply because they are indispensable. For, as we have already had occasion to observe, we could not, without them, form any notion of a single phenomenon or process in the living body, or of the state of a patient, any more than of the mode of operation of an external influence, or of the beneficial or deleterious effects of our therapeutical interference in any disease. Equally little could we hope more closely to investigate, still less to explain all these things without hypotheses or conjectures.

Fortunately, however, such hypotheses or conjectures are not naturally and necessarily so dangerous and useless; still less does the value or worthlessness of them depend upon their mere name, but rather upon their nature and upon the purposes for which we propose to employ them. It depends upon our own capability of forming consistent, and as it were legitimate, hypotheses and conjectures—*i. e.*, such as may impart a certain real knowledge of things previously enigmatical and unexplained, or at least lead us on to further investigation, and in the end, perhaps, to the discovery of the real state of things. It also especially depends upon whether we accept them only as what they are in reality and not as anything more, perhaps as facts and perfect explanations; it further depends upon whether we are willing to test their validity by the method of strict investigation, and to demonstrate it by the results of sound experience, or whether we seek, on the contrary, to base them only upon other, perhaps arbitrary and *a priori* conjectures and hypotheses, and thus to give them a certain semblance of truth.

A natural and indeed perfectly legitimate and necessary impulse, a want felt by the scientific investigator as well as by the practical physician, makes us desire to understand all that we observe to occur in nature—*i. e.*, to ascertain its conditions, laws, and internal connection. Men are wont to take an interest in all these things, however, only in so far as they are themselves concerned—*i. e.*, only in so far as they can understand them, or perhaps employ them for their own advantage or that of others. But nature everywhere offers us in her complex phenomena and processes only enigmas and questions, without affording any clue to the understanding of them. For us they have, as such, scarcely any real existence: we must first of all be able to form some notion concerning them, and this we endeavour to do as well as we can.

What we ascertain concerning them through the senses is indeed, in itself, clear and positive enough, but the manner in which we must represent to our minds the internal connection, the relations of cause and effect in it, is not equally plain. For that which we do not understand, and therefore cannot yet explain, we assume some causal connection, some kind of principle, either secondary or primary, as possible or probable; in other words, we form some hypothesis as well as the present state of our knowledge enables us to do. It cannot indeed be denied that such hypotheses must always be regarded as venturesome and doubtful, and, in a certain sense, as premature and arbitrary, but we have no alternative, at least in medical science. And if nothing remains for us but to form more or less probable conjectures concerning the causal relations and internal connection of the things with which we have to deal, it must be our object to shield ourselves as far as possible from mistakes in the interpretation of them, and to avoid drawing from them arbitrary conclusions.

A false hypothesis, an entirely groundless theory is, however, often better than none at all for more correct knowledge in future, as well as for our practical wants at the bedside; for it is only by venturing upon some such conjecture, however false, concerning the conditions and laws of phenomena and processes previously altogether unexplained, that we can hope, in the end, to ascertain them and to bring the phenomena, &c., themselves more within the scope of our comprehension. Thus doubtless Columbus would never have discovered America unless certain probable grounds for assuming the existence of that continent had previously been made known, and unless, having skilfully put together and correctly appreciated those grounds, he had put to sea in reliance upon them.

Such hypotheses, however, seldom present themselves to our minds during the separate consideration of any individual phenomenon or occurrence; still less could we venture to assume for it, without strong grounds, any special and new mode of development—*i. e.*, any kind of dependence and causal connection in it, differing from those already known. Other phenomena and things occur to us immediately, and as it were instinctively, of which we are reminded by certain resemblances: and here we call to mind especially such as present the most striking analogies, in certain points, to the particular phenomena before us, and concerning the causes and connection of which we have perhaps previously acquired some definite knowledge. Lastly, we go on to assume that all these phenomena may in the end commence in essentially the same manner and depend upon the same circumstances and causes. In short, we consider the probability that they may all have one and the same causal connection, and that, consequently, phenomena and processes still obscure and new may be referred to the causes or laws of those others whose resemblance to them in certain points struck us at first.

Whether this hypothetical conclusion has been at least approximately correct, or false, must depend entirely upon the degree of similarity and accordance which existed between all the things which we had compared with each other. Everything depends, therefore, upon the correctness and clearness of our conceptions, upon the extent of our field of view generally, and especially upon our previous insight into the conditions or laws of some at least of those phenomena and circumstances. In short, hypotheses and comparisons of this kind can only be rendered admissible by bringing the phenomenon or process in question into juxtaposition with others which are really analogous in certain main points; and it is equally certain that this comparison of things still unknown and unexplained with others, can only facilitate the comprehension of them if we are already more familiar with the latter than with the former, especially as regards their conditions or laws. If we succeed, by these our hypotheses and comparisons, in coming as near as possible to the discovery of the real nature of the things with which we have to deal, a most powerful aid has evidently thereby been given to the investigation into their causal connection and to the comprehension of them generally. Allusion has already been made, in connection with other auxiliary means, to the important services rendered by hypotheses and comparisons as links in the process of induction.

But since it has already been shewn that in the employment of such comparisons and hypothetical conclusions everything in the end depends upon their correctness in each individual case, or, in other words, upon the question whether the precise points and circumstances which led us to assume the essential similarity of certain things and occurrences, and of their causal connection, are in reality determining and essential, or not, the uncertainty and frequently the uselessness of these comparisons and hypotheses in medical science is evident enough. For, as has been explained on a former occasion, we have remained to the present time more or less ignorant as to what really is determining and essential in all the phenomena and processes with which we have to deal; and on that account our conclusions from the comparison of them with each other, and from various analogies between them, must at present be regarded as uncertain and often as entirely fruitless.

Thus, for centuries past, various vital processes and various diseases have been grouped together on the strength of certain analogous points in them, without any notable addition to our real knowledge of them, and that for the simple reason that those of one group were as little understood as those of the other. "Fever," and "inflammation," especially, have always been regarded in medicine as belonging to the most important aggregates of symptoms, and numerous theories concerning them—*i. e.*, hypothetical attempts to explain the phenomena and processes in them, have never been wanting. Every period and every school up to the present day has made the attempt, as well as the existing state of knowledge permitted, and in the manner which accorded best with its own particular views. But they have all remained mere hypotheses—*i. e.*, the conditions, conformity to fixed laws, and causal connection of those phenomena and processes have never yet been clearly demonstrated; indeed, we have not even become as familiar with *all* the phenomena in persons so affected as simple observation alone would have enabled us to become. Thus, for instance, the changes in the temperature of the body, in its electricity, in the processes of evaporation and excretion by the skin, kidneys, mucous membranes, &c., the variations in weight, and the modifications resulting from particular external and internal circumstances have, to a slight extent only, been made the objects of exact scientific observation. Equally little have we, as yet, succeeded in ascertaining the circumstances, with their influence and mode of operation, in consequence of which those various changes and derangements commence and cease; neither have we by any means determined with certainty whether, and how far, the return to a healthy condition may, or may not, have been essentially facilitated by our artificial measures, and which of these have been more successful than others. Numerous views, however, obtain on the subject, and the most various attempts at explanation have frequently been made.

However valuable, therefore, hypotheses, comparisons, and analogies, and our deductions from them, may be as the first step and the stimulus to further investigation, they are worthless in many respects as a determinate conclusion. An incorrect hypothesis, a comparison, however rash and vague, will often indeed render us greater services than the entire neglect of such means, so soon as they lead us to further investigation into the conditions and laws of things still enigmatical, but it is equally certain that the best and most ingenious hypothesis or comparison is highly dangerous if we accept it as an actual explanation of them. Every attempt, even the most imperfect, to seek at least for such an explanation by positive investigation will possess a higher value than the most ingenious conjectures or hypotheses, although the multitude often judge otherwise, and easily accept the latter while they ignore the former. But the multitude—especially in medicine—have different wishes and requirements from science itself, and may, consequently, content themselves with things which the latter must reject. Experience seems to show, at least, that in the department of physiology, and still more in that of therapeutics, some one or other is constantly striving to appear as a kind of Newton or Keppler, in fact every one who has ventured to propound a comprehensive theory or entire system: and the others who, like the Israelites, are constantly expecting their Messiah, for the most part willingly believe what he asserts. They greet him with acclamations so that he perhaps himself believes in the truth of his doctrines, until the false prophet is recognized and crucified.

It may, therefore, be regarded as a certain step forwards if we give out our views and hypotheses as worth no more than they really are; if we do not deal with them as with established truths and facts; and if we do not seek to render the latter subordinate to them, but rather endeavour, by further conscientious investigation of the actual state of things, to furnish the confirmation or refutation of them. And precisely because this has happened more rarely perhaps in medicine than anywhere else; because hypotheses have, on the whole, been less used than abused; and because they have more frequently tended to produce confusion and bitter delusions than to increase our knowledge, they have fallen into disrepute with many.

In reality, however, hypotheses vary greatly in their nature and in respect to the services which they are capable of rendering, and it would be both unreasonable and illogical to estimate them all according to the same standard. A distinction must, therefore, be made between rational hypotheses and such as are visionary and more or less absurd; between such as are productive of results and such as are useless. In the case of the former, we may (though led on at first, perhaps, by our purely *a priori* surmises and conjectures) succeed in the end in discovering the actual state, the conditions and connection of things previously obscure, and in demonstrating more or less clearly, by direct investigation, the accuracy of our hypotheses. In the case of the latter, on the contrary, we remain in our hypothetical position, and allow our conjectures and subjective views to take the place of laws established by experience. In the former case our hypotheses, comparisons, and analogies have led us to further exact observation, perhaps to investigations by means of experiment; indeed they may even, from having approached sufficiently near to the real nature of the things in question, have first rendered such investigations and their successful results possible. In the latter case, the exact opposite of this occurs, and from the very nature of the hypotheses themselves they can, at best, only be maintained by the exercise of much ingenuity, and by forcing nature, as it were, to suit the particular views adopted.

It can scarcely be doubted, in fact, that the most talented and successful investigators have often first arrived at their most important discoveries, and highest and most comprehensive generalizations, by mere hypotheses, or certain *a priori* views. One was perhaps shrewd or fortunate enough to select and examine what others had overlooked or rejected. Another had perhaps surmised, and subsequently discovered, certain analogies, certain internal relations or laws, where no one before him had suspected their existence. To both the idea occurred that it might be, that it was so, and their merit or success consists only in their having hit upon the right point —*i. e.*, an innate relation, or causal connection between things which it thereby became possible further to investigate and prove. In the generality of cases, some striking phenomena and facts led to investigation in a certain direction. Other somewhat similar facts, which were afterwards met with, perhaps confirmed the probability of a given conjecture, until further search led at last to the discovery of a causal connection between certain things previously unexplained and apparently unconnected—in short, of a new law.

Thus, in the generalization of the process of oxidation already alluded to—*i. e.*, of the action of oxygen in the living body—Liebig and others perhaps started chiefly from the fact that notwithstanding the large quantity of oxygen consumed in respiration—several hundredweight in the course of the year —no increase in the weight of the body occurred, such as might have been expected from that circumstance. It was evident, therefore, that the oxygen taken in could not possibly remain in the body, but must escape, in combination with the hydrogen and carbon of the latter, in the form of water and carbonic acid, or eventually be excreted, in combination with certain nitrogenous and other components of the body, in the form of uric acid and urea: and because it was observed, in reference to men and animals, that in the last-mentioned products of excretion, and in the bile, urine, &c., and their salts, more oxygen exists than in the blood from which they originate; and because, on the other hand, it was found that in peas and the like less oxygen exists than in pea-straw, a continuous process of oxidation or combustion was assumed to be going on in the former, while the reverse was supposed to occur in plants (Rose).

The more or less successful result of such hypotheses, or rather of the investigation based upon them, combined with a certain innate impulse, perhaps with a consciousness of their own mental power, may frequently have induced others to risk something similar in their department of science—*e. g.*, in medicine. But theirs were no master-minds, and were incapable of following out their conceptions with the same success, because, in the present state of our knowledge, this is next to impossible for the physician, and even for the physiologist. Here, as well as in jurisprudence, "ignoratio nocet."

Every one, it is true, is at liberty to form various notions concerning the enigmatical and unexplained questions or objects with which he has to deal—*i. e.*, concerning their internal connection with each other and their mutual dependence and action; and nowhere, perhaps, has greater use been made of this freedom than in medicine. It rarely happens, however, that any one has better grounds for his views and explanations here than the arbitrary conjectures of the moment. Any given thing appears to him in a certain light because he happens to view it in that light. Thus one man sees the true source and starting point of nearly all diseases in the nervous system, in the brain or spinal cord, or in the ganglia and their derangements; another, perhaps, assumes a so-called blood-crasis as the essential and determining cause of every disease. Again, nearly all diseases and the effects of miasmata and the like, as well as fermentation, putrefaction, and all the processes of conversion in organic matters, might be regarded as essentially dependent upon the formation and presence of fungi, sporules, infusoria, &c., because some of these have, perhaps, everywhere been observed to be present. One man, as experience has shown, may attribute the occurrence of cholera and all other possible diseases to the drinking of bad water, to the exhalations from stagnant pools and morasses; another, on the other hand, to an excess or deficiency of electricity in the atmosphere; or he may refer the diseases of towns to the carbonic acid in the air which its inhabitants breathe, or to the sulphuretted hydrogen in the water which they drink. With almost as much reason might any one attempt to explain the comparatively healthy condition and small mortality amongst the richer and better-fed classes of the population, when contrasted with the poorer, to the fineness of the cloth which they wear, or of the carpets upon which they tread. And whoever is inclined to do so may not only attribute the cure of his fever patients to the employment of calomel, chlorine, opium, or even simple water, but he may adopt the most various views concerning the mode of action of these remedies in each case. It also lies in the very nature of the subject itself, and is therefore undeserving of the least blame, that every one should attempt to explain as best he may the more or less unfamiliar phenomena and occurrences with which he meets, and employ sometimes one, sometimes another of the hypotheses and comparisons with which his knowledge furnishes him, or which appear most correct according to his views.

The very existence of such hypotheses and the large number of them is a *testimonium paupertatis* for each individual investigator and still more, almost, for his whole department of science. Unfavorable indeed is the *status* of a science in which each labourer enjoys such a freedom, which might almost be termed a fool's freedom; in which we are compelled to form such an estimate of the value and importance of our hypotheses; and—what is worst of all—in which such vague assertions still find belief with some.

The chief danger of all our hypotheses and comparisons consists less in the circumstance that they are employed to suit the imagination or caprice of individuals than that they too often pass for something better—*i. e.*, for the explanation of things previously obscure,—and are not only given out, but also accepted, as established truths. It is overlooked that all these hypotheses and comparisons possess a value for science only in so far as they actually suffice to explain occurrences previously inexplicable—*i. e.*, in so far as they enable us to deduce them from laws already known, or from others recently discovered and established. It is overlooked too that we can only rely upon them when the accuracy, or at least probability, of such deductions and explanations is strengthened by reasons which are worthy of belief because they agree with our experience and with the actual state of things—in short, with all the facts and results of actual investigation.

Those investigators of nature, who have known how to employ hypotheses with the greatest success, and who have thus brought them into a certain repute, are the very men who have, at the same time, been least satisfied with them, as such. They have all doubtless been willing to attach a higher value to them only when they fulfilled the requirements alluded to above, and when their probability or accuracy could be confirmed by further investigations and by a conscientious examination of facts. It was not the mere construction of hypotheses, not *a priori* surmises and conjectures, in themselves, which could lead them to actual discoveries, but rather the circumstance that they had hit upon such as were correct, and that this correctness eventually admitted of demonstration. Thereby alone did they succeed in converting their hypotheses into a law, into truth. Their comprehensive and penetrating glance had perhaps already been able to detect certain essential relations or resemblances between various phenomena and occurrences, which had escaped the eye of ordinary observers. And because all these things suddenly admitted of being brought into the true and only possible relation to each other, they were better able to explain them than others who had gone before. They discovered for them a particular kind of dependence or action, in short, of causal connection—new laws, therefore—or referred them to others already known. But it was not possible to do this by the aid of their hypotheses, in themselves, but only through the subsequent demonstration of their accuracy. Their success and their merit consisted only in this, that their hypotheses were precisely of such a kind as to admit of demonstration, and were confirmed in the end, by all the known facts. It was necessary that all the conclusions to which their hypotheses naturally led should first be shown to be true—*i. e.*, to agree exactly with the actual state of things, and suffice for the perfect explanation of them,—before the hypotheses themselves could be accepted as correct.

This presupposes, before all, a department of science, a branch of knowledge in which such demonstrations by individuals are possible. Various series of phenomena and occurrences, though isolated perhaps, must already have become known, at least approximately, not only in relation to their mere existence, but also to their causal relations, conditions, and laws. Further, the questions at issue—*i. e.*, all the points to be determined by those demonstrations, as well as the methods and processes for their investigation or solution—must be of such a nature that we may be able to ascertain something certain and positive concerning them.

Thus it was necessary that much should already have been ascertained concerning the magnitude and distance, the relative position and motions of the heavenly bodies; the labours and observations of astronomers for many centuries were required before a Keppler or a Newton could arrive at his brilliant deductions. It is well known, also, how astronomers continued to take observations over half the earth, according to the strictest and most exact methods, with the best instruments and other auxiliary means, in order to prepare the way for the solution of similar questions. In like manner, chemistry had to collect numerous, though perhaps isolated facts and proofs. The conformity to fixed laws of various phenomena and processes had to be determined before a Liebig, for instance, could advance further by their aid, and carry out his calculation of probabilities with a certain amount of likelihood.

Physicians and physiologists, on the contrary, have scarcely ever penetrated beyond the surface in their department of science. They have succeeded, it is true, in establishing the actual existence of certain series of phenomena and processes in the living body, together with the mode of their manifestation and sequence. But we have nowhere ascertained exactly what takes place in them; we know next to nothing of the conditions and laws of their occurrence, and the physiologist, in reality, is little more enlightened than the pathologist. Our methods of observation and investigation have only begun to prepare the way for knowledge of this kind in the future—*i. e.*, we have only just begun to employ them in such a manner and in such a direction that we may hope, in the course of time, to ascertain something positive concerning the real nature of the things with which we have to deal.

In the present state of our knowledge, therefore, it appears altogether impossible that hypotheses and comparisons should meet all the demands which may be made even upon such of them as are sound and consistent; still less can any individual hope to furnish proof that those which he employs exactly fulfil all the requirements which may possibly arise. The conditions and laws of all the vital processes are so little known to us that we are unable, as yet, to construct any hypotheses concerning them with even an approximation to certainty; and, if we should accidentally succeed in doing so, we could not, in the present state of our knowledge, and with our present methods of investigation, prove the truth of our own assertions. Equally little can we proceed with certainty in comparing with each other the precise processes in the living body, or the influences which act upon them, even though they may agree in their essential points, and that for the simple reason that these latter are themselves still too little known to us.

All this also serves to explain why hypotheses and the employment of analogies not only do little to increase our knowledge of the things we are seeking to investigate—infinitely less, for instance, than in physics and chemistry—but why they even more frequently perhaps impede the acquisition of it; and the more comprehensive our hypotheses and comparisons may have been, the more frequently will this result follow.

For the present, therefore, it seems more advisable to seek to become better acquainted with the circumstances and laws of each individual occurrence, and with the mode of action of each individual agent or influence in it, instead of venturing upon numerous hypotheses and conjectures concerning entire aggregates of such, or even, perhaps, concerning them all. Each positive demonstration of even the most insignificant new circumstance or occurrence would usually possess more real value than the most ingenious conjectures concerning all those which might possibly arise. We should also be the more able to rely upon our hypotheses and comparisons, and they would the more tend to add to our real knowledge, the more we had, in the course of time, become acquainted with the causal connection and laws of at least individual phenomena and processes.

Thus, even if we have formed a certain hypothesis, or drawn a certain comparison, we can never venture to rely upon it; and if this holds good for every department of natural science, it applies especially to medicine. In it, least of all, can we be satisfied with our conclusions from any hypothesis, or analogy, and we must strive, more and more, to replace them by others more trustworthy, drawn from facts and direct investigations. Since the former, under the most favorable circumstances, can only be regarded as conjectures, or as a calculation of probabilities, in which we have perhaps reasoned with tolerable accuracy, perhaps altogether falsely, we must always retire upon the only sound basis of our knowledge—*i. e.*, upon the actual state of things. We must endeavour, by repeated and close examination of the latter, to apply a kind of counter-test to our previous views and deductions. We must examine, therefore, whether any new case, any new phenomenon or occurrence, or perhaps the same phenomena and occurrences under different circumstances, are opposed to our hypotheses, to our previous conjectures and deductions. If we meet with such a contradiction we must examine further in what the sources of error may have consisted, or upon what the differences in the results obtained at different times may have depended. We must endeavour to ascertain what circumstance or influence has varied in each case, or what new one has been superadded, so as to produce those variations or modifications in the general process or result, with which we have already become acquainted.

These further tests and examinations, which are a simple duty in our search after truth—our highest aim—also lead not unfrequently to entirely new discoveries, and to unhoped for elucidations of the questions at issue; and this again is the highest reward of his labours, for which the true and conscientious investigator can anywhere look.

An infinitely more important and certain auxiliary in the calculation of probabilities in medical science than hypotheses and analogies has been more and more recognized in the collation of cases and observations of the most homogeneous nature possible—in short, in statistics or the numerical method—and in a certain sense, and to a certain extent, this recognition has been most justly made. Allusion has already been made, in speaking of the inductive process, to the employment of this method as an aid to investigation. We there endeavoured to explain the reasons why no exact conclusion can be drawn from such statistical computations, in themselves, concerning the conditions and causal connection of certain phenomena and occurrences, or concerning the mode of operation and office of various influences in them. It is self-evident, also, that for essentially the same reasons the safe employment of such numbers in our calculation of probabilities must, for the present at least, be very difficult, if not altogether impossible. For all the products and results of statistics, important as they may be in other respects, cannot, in themselves, afford us any safe points of support for the elucidation of an individual case, any more than for the discovery of the causal connection of our phenomena, &c. The complication of circumstances, the number of co-operating and modifying influences and conditions are everywhere too great, and on the other hand our knowledge of their effects or laws individually is too slight, and the results of statistics are at present of too general and uncertain a nature to enable us to draw any precise conclusions from them.

In many instances we learn from statistics, indeed, that, of many existing and operating circumstances or influences, some one has more probably determined a given effect,—*e. g.*, a disease, a cure, or a death—than any of the others; also that, of many effects or consequence—*e. g.*, an attack of a certain disease, its cure, death, or a certain duration of life—one will occur more frequently under given circumstances than another. In the latter case we may thus, by the aid of our computations, rather anticipate a certain event than some other perhaps opposite one; in the former case we shall be able to deduce a certain event and its development with much greater probability from one given circumstance than from another. All this, however, for reasons which are evident enough, admits of no direct application to an individual given case, nor can it lead us to any even remotely safe conclusion concerning its nature. So many special circumstances and influences come into play in each individual case, and complicate the question and calculation to such an extent, that the latter, notwithstanding all the numbers and results of statistics, cannot possibly be a safe one, even if it were much more exact, and the office and influence of all those modifying circumstances much better known to us than they really are. This also explains why, in no given case, precisely that will occur which our statistical computations might lead us to expect, or, in other words, why each individual case has its own special result, which could not possibly have been calculated with certainty from those general products of statistics, and does not agree exactly with them.

If, for instance, it appeared from statistics that of 1000 inhabitants of a given country or place, who are in their 20th year, at least 800 live from 10 to 15 years longer, no one will attempt to infer therefrom that the duration of life in any given individual amongst them will be precisely the same. On the contrary, the probability is infinitely greater that such individual will either live beyond the age of 30 to 35, or die before he attains it. Again, if we know that, of all the persons attacked by Asiatic cholera in a given town, 50 per cent. die, and an equal number recover, we cannot on that account assume for an individual cholera patient that the probability of his dying is precisely as great as that of his recovery. Such probability will rather be greater or less than the one established statistically, according to his particular circumstances—*e. g.*, his age, rank, and constitution—or according to the severity of the attack. And if it had been observed in all the hospitals and similar institutions of a country that of 100 persons attacked by inflammation of the lungs, 80 had constantly recovered, and 20 died after the employment of certain remedies—*e. g.*, bleeding—no certain conclusion could be drawn therefrom concerning the case of an individual patient. His disease and general condition will always be of such a nature, that the probability of his death or recovery will be either much above or much below the statistical average; and in no case will the probability of his death stand to that of his recovery in the exact proportion of one to four.

These results of our statistical calculations have, therefore, only a general and, as it were, superficial value. It follows from the very manner in which such general results have been obtained, that they are applicable only to a whole series or class of cases and instances essentially similar to each other, but not to any individual given case. For here again, numerous special circumstances and influences are in operation—in cases of disease, for instance, age, sex, constitution, mode of life, season of the year, climate, neighbourhood, &c.—in accordance with which the state of things in a given case will always vary, and the general result of statistics consequently undergo a certain modification.

In addition to this, all these results of our statistical calculations—*e. g.*, of duration of life, of mortality, of disease and its fatality, of remedies and their effects—hold good only for the particular kind or combination of circumstances under which they have themselves been observed and collected. They also, equally with our experiences and simple empirical data, have a certain value only for a given place, a given time, or a given class of men, &c.

For this reason the probability of a long life, or of good or bad health, in the case of A or B, will vary greatly with the circumstance of his living in Germany or France, in the Delta of the Ganges or on the banks of the Mississippi; just as a wealthy man has a very different prospect of health and long life from an indigent inhabitant of the same place. Thus, when an individual is attacked by intermittent fever, typhus, or Asiatic cholera, the probability of his recovery, even under precisely the same personal circumstances, will vary greatly if the attack occur in Berlin, Vienna, Bengal, or New Orleans. Essentially the same thing holds good of all our statistical calculations concerning the part played by various causes of disease, and concerning the influence of individual social circumstances or hygienic agents upon health and duration of life, or of the various modes of treating a disease upon its events.

In all these cases, therefore, we cannot speak of a calculation of probabilities in the strict sense of the term; for the great complexity and constant variation of all the circumstances render the safe application of general statistical results to an individual case impossible, and thus make every attempt to submit the latter to exact computation a mere illusion. And since, in all these cases, nothing can be calculated with certainty, the attempt to do so, and too great a reliance upon the results of such calculations, often bring with them the same dangers as when others—*e. g.*, physicians of the old school—rely upon their so-called experience. In one case, as in the other, we shall equally be investing ourselves and our assertions with a semblance of certainty to which we can lay no claim whatever.

It is true that we are daily compelled to make a kind of calculation of probabilities concerning the question at issue, and to carry it out as well as we can in an individual given case. Here, however, we fortunately possess means very different from, and more certain than, statistical numbers in themselves. We shall attain our end with infinitely greater certainty, namely, by seeking to estimate and determine, as exactly as possible, all the circumstances which may be important and influential in the case before us, for the purpose of drawing a probable conclusion from them. It makes no difference in the main point whether the question be about the appreciation of various processes in the living body, or of the calculation of the probable influence of various social circumstances, of the event of a disease, or of the best mode of proceeding in the treatment of it. A close scrutiny of the individual case before us, in all its details, will aid us much more in judging of its nature, and in forming a probable calculation of what will occur in it, than the employment of mathematical formulæ, or statistical numbers, in themselves.

In our calculation of probabilities, also, our success will depend chiefly upon our knowledge of the subject, and upon the extent and solidity of our experience in the particular branch of science. The accuracy of such calculations will thus only gradually increase in proportion as we are more and more able to clear up the obscurity and uncertainty respecting all the questions and objects which are presented to us, by means of positive and exact investigation, whether it be by the simple observation of the actual state of things, or by the aid of experiment. And precisely because statistics are in this respect also among the best of our auxiliaries, their results will always retain their important position in our calculation of probabilities. If we do not acquire, by their aid, any insight into the conditions, causal connection, and conformity to fixed laws of the processes which we are seeking to investigate, they afford, at least, a sufficiency of important facts and empirical laws from which we may judge more certainly of each individual case, and predict with more probability what will or will not occur. If, for instance, statistics have once clearly shown that not less than 60 per cent. of those attacked by Asiatic cholera in a given place always die, the death of A or B, so affected, will always appear more probable than if the mortality, in the same place, and under the same circumstances, never exceeded 20 per cent. We cannot, it is true, make any definite prognosis for A and B, but the general probability of their death in the one case will stand to that in the other in the proportion of three to one.

Nothwithstanding all this, our calculation will ever remain very uncertain, and one by no means to be relied upon, unless we can call to our aid further knowledge, and other auxiliaries, from the quarter already alluded to. The more extensive and exact our knowledge of at least individual occurrences and their laws becomes, in the course of time, the sooner shall we be able, not only to draw conclusions from them concerning the nature of many others, and concerning their conditions and effects, but also to predict with much greater probability what will occur in the cases which come under our observation. For, with that clearer insight into the actual state of things, and with the increase of our general knowledge, we shall acquire more and better grounds for assuming one condition of things to be more probable than another.

Essentially the same advantage accrues for our judgment of each individual case. If we once succeed, for instance, in discovering, in a given case, the existence of some particular more important circumstance, the action of some one more or less essential and determining influence, previously unknown to us, not only will the development and occurrence of a given effect or event become much more intelligible, but we may, in other cases also, with more probability than before, look for, and therefore predict, the occurrence, or non-occurrence, of this particular effect or event.

If, for instance, we knew nothing more concerning a sick person than that he was out of health, we could form no opinion concerning the probable duration of his disease, or concerning the probability of his recovery or death. So soon, however, as we ascertain that he is attacked by Asiatic cholera, for instance, our opinion on those points will at once become more definite, and will differ greatly from the one we should form if we ascertained that his disease was a simple diarrhœa, or common continued fever. So long as we know nothing more concerning a patient than that he has much cough and expectoration, and suffers from fever and pain in the chest, our view of the danger of his state, and of the probability of his recovery, will be infinitely less clear than if we had succeeded in ascertaining that he was suffering from simple bronchitis, from inflammation of both lungs, or from acute tuberculosis. And since it has been discovered that, in persons affected with continued fever, various deeper-seated changes in the alimentary canal, ulcers, &c., frequently occur, with which the diarrhœa in that disease stands in some intimate connection, the prospect of checking this diarrhœa by our remedies will differ greatly from what it was formerly when we had no knowledge of the existence of those changes in the alimentary canal.

If we are told only that a certain European is living in a tropical country, we are unable to conjecture and assert anything with certainty concerning the probability of his continuing in a state of health, or of his falling sick and dying. If we once know, however, that, in the place where he lives, yellow fever, dysentery, cholera, and the like, yearly occur epidemically, we shall be able to form a prognosis for him, and that a more unfavorable one than if such were not the case. This prognosis will also depend very much upon various other circumstances—*e. g.*, that the European in question has lived there some weeks only, or many years; that he is temperate and robust, or dissipated and weakly; or that he is a merchant or traveller in good circumstances, or forced to labour hard, with bad food, in an unhealthy dwelling or neighbourhood. We cannot, it is true, form a probable calculation whether an individual who has been condemned to ten years' imprisonment will continue healthy and leave his prison alive; but this will be much more possible if we find, on examining him, that he is scrofulous, phthisical, or dropsical; or if statistics have shown that, of 100 persons confined in that prison, at least 20 to 30 have died annually.

What has already been said may suffice to show why our calculation of probabilities, equally with our hypotheses and conjectures, will gain in certainty the clearer our perception of the occurrences and circumstances with which we have to deal becomes. So long, however, as we have advanced no further in this respect than at present; so long as the essential elements in a given case, the causal connection and laws of individual phenomena and occurrences in it, continue to be more or less unknown to us, we shall also be unable to determine whether and how far all the things which we have observed in the case before us stand merely in an accidental, or in an essential and causal relation to each other. For the same reason we shall not be able to judge, or to calculate with certainty, whether, under given circumstances, a given effect or event will or will not occur.

III. LANGUAGE, TERMINOLOGY AND DEFINITION, DESCRIPTION.

No attempt can be made in this work to explain in detail the infinite importance of language generally, as well as of individual words and forms of expression, for our judgment, our investigations, and, in the end, for our comprehension and knowledge as a whole. Such an attempt would far surpass both the powers of its author and the limits of his task.

All that it is necessary to bear in mind here is, that we everywhere not only think, judge, and draw conclusions for ourselves in words, but that we have generally no other means at our command of expressing and communicating to others our observations, deductions, or abstractions, our general notions and views, or of becoming acquainted with those of others, except by words—*i. e.*, by the aid of language. All that we assert concerning any phenomenon or occurrence, or that we learn concerning it from others, whether it be correct or false, is conveyed by words; and since this has been the case from childhood upwards, our whole knowledge, our whole world of notions and views is, as it were, laid up in words. Language is thus most intimately blended, and has become almost identical with our intellectual *ego*. It has its birth and grows as the latter develops itself and grows; it has its origin in our views and notions, in our conceptions of various things; it is, as it were, only the expression or mirror of them, and reacts ceaselessly upon them. The ancient Greeks even employed one and the same word (λογος) for reason and speech.

In short, language, as every one knows, is the indispensable organ, or instrument of our whole intellectual life, whether for ourselves, or for our intercourse with others. It is not, however, a mere passive instrument but rather one which exercises the most important influence upon us and upon our whole mode of thinking and judging. Locke, in his celebrated 'Essay on the Human Understanding,' has rated the importance of language for the mind, and the dependence of the latter upon it, so high, that he may rather be said to have written a treatise upon the origin and development of language and forms of expression than upon the influence of these on our notions and views. There exists, in reality, so close a connection, or mutual reaction between both; language, words, and the world of our ideas can so little be separated from each other; that we are unable to judge of the latter, or to assert anything concerning it, without having first carefully weighed the former and become acquainted with their influence upon our mode of thinking. Words and expressions stand, to a certain extent, in the same relation to thoughts and ideas as the skin does to the body (Herder).

On a closer examination it becomes evident that we cannot express anything in words unless we have some conception of it, nor can we utter or hear any word without its immediately affecting our conceptions. Every word, when once uttered, comes into further operation by calling forth some kind or series of thoughts and notions; it therefore, in the end, exerts a much greater influence upon our judgment than the word in itself—*i. e.*, the mere sound, would exert. With every word, or expression, such, for instance, as innervation, miasm, irritation, or inflammation, we connect certain previous notions and views which our knowledge and experience bring with them. Such expressions are, in fact, only short symbols of those notions or views, which, whenever we employ or hear them, awaken in our minds the same thoughts or ideas as we had formerly connected with them, and which we had, perhaps, previously acquired on similar occasions and from similar observations. Our interpretation of the case before us will also, without our being conscious of it, be influenced more or less, according to circumstances, by this intervention of our previous notions, this definite kind of association of ideas.

Thus, if the notions we have always been accustomed to associate with a certain expression, or, in other words, the general signification of the expression itself, have been confused and indistinct, those which it now awakens in us will have the same character. And whenever, on the other hand, our knowledge of anything is vague and deficient, we shall be unable to express it otherwise than vaguely and deficiently in words.

These few remarks may suffice to show the necessity of closely examining into the relation in which language stands to our questions and problems. The office of logic in the investigation of nature in general, and in our department of natural science in particular, is, to make us acquainted with all the ways and means which may aid us in the discovery of truth and of the exact state of things. And if words and language are an indispensable instrument for the expression of what we have seen and observed in our investigations, and further, for the formation of all the views and conclusions we draw from them, it must also be the office of logic to teach us the proper use of this instrument. For it will be of the highest importance that we should know how to make the best use of this auxiliary in the process of induction. We clothe our observations and descriptions in words, or become acquainted with those of others through words, and no less are our conclusions and whole train of thought an operation which is performed essentially in words, and indeed, as we have seen above, under the constant influence of them and of the impression which they exert upon us and upon our conceptions.

In addition to this, the observer, the investigator, and the physician, each finds himself at every moment necessitated to draw some conclusion or other; for each is compelled to form *some* opinion of what he has recognized and observed. Their conclusions, however, consist of certain phrases and assertions, these again of words, and words are the expression of our ideas or thoughts. If the latter be vague and erroneous, so will our words be also, and conversely, vague expressions will render our mode of judging more or less vague and erroneous. Hence it follows that while a clear and pertinent language and mode of expression will essentially facilitate our conceptions, the reverse of this will result from the use of inadequate language. And every one who does not know exactly how to employ language, and is not thoroughly conscious how far he is influenced by it in drawing his conclusions, will easily fall into error at every step in his observations, and still more in the interpretation of what he has observed, and in his inferences from it. He will easily incur the risk of setting a value upon his notions of a thing rather than upon the thing itself, and of believing that he is speaking and judging of the latter while he is, for the most part, describing only his own views. It is the office of logic, however, to call attention to these and similar dangers, which may arise from the nature of our language and modes of expression. It is further of the highest importance here to examine generally how we have arrived at our ideas in natural science, and especially in medicine, and why these have so often been obscure and erroneous. What has already been said might suffice to explain why language plays no inconsiderable part therein, and proofs enough of this will be furnished in what follows.

However this may be, language has ever been the medium for the communication of all knowledge and experience from the earliest times to the present day, as well in medicine as elsewhere. On this account it is an indispensable auxiliary, an essential means for the acquisition of knowledge. To attain our end, which is the discovery and determination of the actual state of things, and of the laws of the phenomena and occurrences which present themselves to us, that we may be able to advance sound propositions concerning them—*e. g.*, concerning a disease, or the mode of operation of a given influence or remedy—we must first have acquainted ourselves with all the co-operating circumstances, and have observed carefully all the cases of this kind. It will easily be understood, however, that it is never possible for any individual observer to do this to the necessary extent; he is necessitated to call to his aid the experience of others, and to have recourse to the stores of information laid up in his department of science. In short, like others, the physiologist or the physician, who seeks to acquire any knowledge of the things with which he has to do, or perhaps to investigate them himself, has need, at every step, of the experience of others for the development and completion of his own ideas, and for the guidance of his judgment. How much of all his scientific knowledge would, in the end, be left to an individual, if all that he has gleaned from others could be taken from him? But all that others can teach him they give him essentially only in words, and no less is our own individual experience, with the ideas and inferences we draw from it, laid up in the memory in words only, to be brought forward again in words when occasion may require.

Thus it is that language is the great repository or store-house of everything which has been observed, investigated, imagined, and discovered to the present day. In it our predecessors and contemporaries have recorded their achievements, and for that reason language is, to a certain extent, the sacred property of each science. For that very reason, again, language is not only an indispensable instrument for the education of the individual, but, at the same time, also the sole link by which he can connect his own experiences and achievements with the general stock of knowledge in his science and art. Language thus forms the intellectual bond between all men who understand and employ the same tongue, as well as the technical medium of communication between investigators in each science and art.

That the language of science, in particular, may render the important services just alluded to, it must first of all, like language in general, fulfil certain requirements. It must, above all, furnish words and expressions for everything which we observe and discover, for everything concerning which we form notions, and which we wish to express and communicate to others: in short, wherever we are in need of special words and expressions, these ought to be at our disposal. We require, therefore, a terminology—*i. e.*, an expression for every phenomenon which we recognize through the senses, and for every object, with each of the properties and modes of manifestation which we meet with in the observation of it, and in the comparison of it with others. In like manner we require special expressions for each individual series of such objects and phenomena as agree, or at least appear to agree, in certain properties or essential points—*i. e.*, which are of the same kind and belong to one and the same group or class. In short, we require a special and complete nomenclature as it exists in the greatest perfection in botany, chemistry, and other descriptive natural sciences, and approximately in pharmacy and even in pathology.

It is, perhaps, more essential still, that each of these words or expressions should convey exactly what we wish to assert concerning the things designated by them, and that they should render perfectly clear to others, what we intend to express. Each individual and definite expression must, therefore, point out a definite object, or phenomenon, or a certain property in it; every word must have its exact meaning, its precise and definite value. No expression ought to be obscure, or admit of being taken in more senses than one.

It is, therefore, in the end, not of so much importance to possess a language generally,—*i. e.*, to be able to communicate in words all that we wish to express, as to do so correctly and consistently. Our language must, therefore, not only be complete, but also clear and definite enough. In fact, this precision may be regarded as the most important requirement, for upon it depend especially the clearness and certainty of our ideas and judgment, and the correctness of our inferences. And this again is the first condition, the indispensable means for the attainment of our final aim—*i. e.*, the discovery and determination of the true state of things.

All this, however, as is well known, does not hold good of our technical language as it exists at present; and the reasons why this is so are intelligible enough, for they lie entirely in the nature of the subject itself.

We must especially take into consideration here, that the technical language of medical science, like that of any other science, has not been constructed with a full consciousness of the requirements it ought to fulfil, or with an adequate knowledge of the ways and means which can best lead to that end. Like the whole science of medicine, it has rather arisen as the offspring of necessity, almost unconsciously and instinctively, and has reached us much in the same form. It sprang out of the practical wants of the moment, not out of an even approximately correct understanding of all the things it was intended to express. Men sought to propound something by its aid, not only concerning the existence and occurrence of various things, but also concerning their conditions and effects, their connection and essential nature, and were thus necessitated to form a language as best they could, long before they had become really acquainted with the things themselves.

Our predecessors in medical science especially, who were compelled to use this language because they saw and had to treat the sick, were far from being scientifically educated—*i. e.*, from possessing a knowledge of the things they observed and wished to designate. They were priests, or shepherds—quacks —and their observations and traditions have formed the first groundwork of medicine and its language. They could not be conscious what phenomena and properties, what things and processes they gave names to, because they had no real knowledge of the things themselves, and were unable to form other than very deficient notions of life and nature, of diseases and their causes, of remedies and their effects, &c. This was as impossible for them as it would be for an inexperienced child to describe anything consistently and correctly, or to give it a fitting name.

The earliest physicians were unable, and did not attempt, by their language and expressions, to do more than make certain superficial assertions concerning various diseases, injuries, remedies, &c., as they appeared to them. And if they afterwards met with anything which seemed to agree in certain points with what they had previously observed, they gave it the same name, without knowing whether and how far the agreement was real, or only apparent. In like manner they instituted comparisons between such things and certain other natural phenomena, but of necessity also in a rough and general way, without any real knowledge either of the one or the other. They further formed from what they had observed, certain general notions of the causal connection, conditions, and effects of those phenomena, and even advanced general propositions concerning their ultimate causes, laws, and intrinsic nature. But since, in all these cases, they were compelled to confine themselves to the observation of various phenomena, circumstances, and occurrences, or to what happened to make the most powerful impression upon their minds, and appeared to them most important and interesting, and to observe all these things just as they presented themselves, without having, in reality, ascertained anything definite about them, it is easily to be understood why their success should not have been greater than it actually was. It is also intelligible enough why their language should be so full of crude and vague conceits, of general and obscure allegories, for their whole world of notions and views itself, bore essentially the same character, and we find that this is more or less the case in the language of every science.

The words and forms of expression employed by our predecessors have, however, been transmitted to us. We have been necessitated to accept their language as it stood, with all its deficiencies and errors. Like other relics of earlier times, the expressions once formed and taken into use have remained, and with them lie dormant, stratum upon stratum, whole series of views and theories, which had gradually sprung up and partly died away again. These have, however, continued partly active, and still exert more or less influence upon our language and modes of expression, and consequently upon our notions and views. For since they have ever been the symbols or expression of the doctrines of a science, they have been, as it were, identified and incorporated with it, and with it have come down to us.

If every language is, in a certain sense, rather figurative and symbolical than substantive and objective; if words express our notions and views of a phenomenon or object rather than the latter, in itself—*i. e.*, than what we may have been able to recognize in it by means of the senses—this applies especially to the technical language of medical science. Instead of the things actually observed and their properties, it gives us, for the most part, only symbols and the crude allegorical comparisons of our predecessors, such as corresponded to their imperfect notions of the things themselves. Almost every term or word in medical language shows this: for example, "inflammation," "fever" (*ferveo*), "hypochondriasis," "obstructions," "hemorrhoids," "consumption," or "irritation," "reaction," "antagonism," and "sympathy."

It is, however, easy to understand that expressions of this kind convey no clear meaning, nor could any such be connected with them, simply because none existed. Moreover, every attempt to effect this is rendered more difficult by the circumstance that those observations and descriptions, those notions and views of earlier times, have not been transmitted to us in our own, but in foreign languages, and not only in foreign, but, for the most part in dead languages—*viz.*, in Latin and ancient Greek. All those expressions were adopted and came into use under the title of *learned* or *technical* language; and, valuable as they may be in a scientific point of view, it is no less certain that a large number of traditional expressions have thus come down to us, whose foreign character, in itself, renders them more obscure and inexpressive than they naturally are.

The well-known half-Latin jargon, with a mixture of Greek, &c., was thus, from a certain innate necessity, adopted in medicine; and this was favoured by the further circumstance that the man of science was better able, by such means, to seclude himself from the mass of the people and their language, and felt himself greater and more secure in his technical wisdom. And while Göthe says, "that every schoolmaster, when speaking Latin, appears to himself more distinguished," similar notions have exerted more or less influence down to our own times, and perpetuated the use of such a form of language. With German physicians, for instance, it has continued to be employed in spite of everything, although the German language in particular, from its richness in root-words, is better adapted than any other to the formation of technical terms. Nevertheless, even in our own days, we see teachers in certain countries necessitated to convey their doctrines in medical science in Latin—the best means, doubtless, for the concealment of their own want of knowledge, and for preventing the acquirement of it by their hearers.

Those expressions of earlier times have further lost in clearness of signification as our own notions of the things they were meant to designate have, in the course of time, become changed. But it has been less from our inattention to their original signification than from the increase of our experience and knowledge, and from the contributions of entirely different sciences to the latter, that the primary meaning has become partly lost to us. And this happened the more easily because those expressions were from the first, and in themselves, fluctuating and indefinite, and admitted of various interpretations. We can thus understand how their original signification might not only become changed, but even lost altogether.

Notwithstanding all this, we continue to employ them, just as many potentates still bear the titles of countries and dignities they have long ceased to possess. It is also self-evident to how great an extent the daily employment of words, whose proper signification had become gradually changed, or more or less lost, could not fail to interfere with the clearness of our notions and views—in short, of our whole knowledge. How could empty formulæ, for the most part without meaning, act much otherwise? But the intellectual elements which first called them into life, and imparted to them their meaning and usefulness, have, in the course of time, almost entirely disappeared. The notions and views, of which they were the product and the expression, have made way for entirely different ones; and, whether we are fully conscious of it or not, we no longer cherish or partake of them; but the words and forms of expression have remained, without root in our scientific consciousness and knowledge, and hence, again, without any real value for these, but rather impeding than promoting them. They are opposed to each other because our knowledge and views stand in a similar opposition to those of our predecessors, who first brought them into use and employed them in a sense which it has become impossible for us to adopt.

It thus happens that we give names to most of the things with which we have to do—diseases, the effects of the influences of the external world, remedies, &c.—whose primary signification differs entirely from that now attached to them. By these names we attribute to them certain properties, relations, or causes—a certain behaviour—which we are now convinced do not belong to them. In short, we no longer mean to assert what the words we use convey; their literal signification involves a meaning which we no longer regard as the true and correct one.

Thus, in the case of such words as inflammation, fever, rheumatism, melancholy, or absorption, resolution, and many others, we cannot possibly entertain the same views as those who employed them some centuries, or even twenty years ago. Here, therefore, as in almost every other instance, we are constantly necessitated to change and modify the original signification of the terms we employ. With those terms we nevertheless often assert one thing or other concerning the properties, conditions, or mode of operation of the objects we are seeking to investigate, which we neither intended, nor could with truth assert. And, even under the most favorable circumstances, our terms express only vaguely and superficially the notions we form, and the assertions we are able and anxious to make.

We are often told, indeed, that we must not adhere to words in themselves, but to their present more correct meaning; not to the shell, but to the intellectual kernel. But to carry out this exceeds, in many respects, the powers of man; it is requiring of him things more or less opposed to his whole intellectual nature; for, in spite of everything, it follows from the employment of such words, that their former signification mixes itself as a vague reminiscence with our perhaps more correct notions of the phenomena and occurrences designated by them. Whenever we think, utter, hear, or read such words, their former signification is always, as it were, re-echoed with them. For this reason, also, the continued employment of such half-allegorical, figurative, or even meaningless or false expressions, often exerts a much more dangerous and disturbing influence upon our notions and views than we are willing to believe. Moreover, our own insight into the processes going on in the living body, and all the other things which we have occasion to express in words, are so imperfect, and the views and notions of physiologists, and yet more of physicians, concerning them, still show, in many respects, so little harmony, that on this account, also, the meaning of these terms must be more or less fluctuating and indefinite. Not only has it undergone constant changes with the advance of our knowledge, but it varies also according to the peculiar views of the individual. Dissimilar ideas are often called forth in different minds by the same word or expression, or the latter are, at least, employed arbitrarily, and have various meanings attached to them. It may suffice here to instance the varied and fluctuating signification of such words as irritation, excitement, crasis, poison, and many others.

Some remedy for these evils might, doubtless, be obtained by the employment of such words or expressions only as designate things simply as they are, or rather as we observe them in nature, and not as we represent them to ourselves. But this is impossible, especially in our department of science, because, with the exception of outward forms, we can recognize next to nothing directly through the senses, least of all the occurrences and processes going on, as well in the living body itself, as in external nature around it. And, while our observation and knowledge of these are essentially rendered more difficult by that circumstance, the whole precarious and fluctuating condition of our technical language is intimately connected therewith. It expresses, well or ill, the generally obscure notions we entertain at the moment concerning those occurrences and processes rather than the latter themselves.

These considerations are, perhaps, sufficient to show how far our technical language is from fulfilling the requirements above mentioned. Not only, indeed, does the confused and doubtful sense of the majority of its expressions disturb, in a greater or less degree, our comprehension of the objects thereby denoted, *e. g.*, diseases, &c.—and still more, the description and communication of them to others,—but, on the other hand, the clearness and correctness of our ideas, of our own inward conceptions, and consequently our whole intelligence, is thereby impaired in an equal degree.

For this reason, also, our language must be regarded, not only as a somewhat inconvenient, but as a decidedly faulty means of expressing our thoughts, conceptions, &c. Many persons, perhaps, imagine that our right use of language depends essentially upon ourselves; that, in short, it rests entirely with us to use it correctly or not. And thus we easily arrive at the conclusion that we rule our language, and forget that, on the contrary, we—that is our conceptions, and therefore, our judgment—are generally more or less ruled by it. Too little stress is, perhaps, laid on the fact that by the constant use and reception of certain words from our earliest youth, a mass of disturbing and indistinct images has gradually infused itself into our ideal world.

From this state of things, we may clearly see how it is that, not only in every day life and conversational language, but likewise in medicine, we rarely know what we exactly mean and wish to express by the use of such and such words. But the cause of this deficiency is not solely or exactly that these words do not in themselves express the precise meaning which we wish to convey, or that, being originally derived from indistinct, if not erroneous views, they can only excite ideas of a similar nature, but rather that, in the actual state of our knowledge, it is impossible to attach to these words a clear and definite meaning. For to this day we are very far from knowing enough of the objects of our science—such for example as functions, diseases, or the action of any influence or remedy on the living body—to enable us to say clearly and distinctly what we mean by the expressions used respecting them, or what, in fact, is the exact import of our words. We may, in fact, almost literally apply to ourselves the observation of Seume, "that we ourselves seldom know our own real meaning."

Or can any one say clearly, what he exactly understands by respiration, conversion of tissues, and nutrition, or by sensation, innervation, mind, and similar expressions? Is any physician able to define positively what he pictures to himself by the terms fever, inflammation, congestion and stasis, rheumatism, hemorrhoids, dropsy, or by rheumatico-inflammatory complication, and scrofulous diathesis? Or again, what meaning he attaches to such general expressions as irritation, crasis and dyscrasis, plethora, spasms, tone, excitement, and erethism, or to words like miasma, contagion, taking cold, errors in diet, and the like? We daily hear our remedies spoken of as alterative, solvent and deobstruent, tonic, sedative or antispasmodic, and certain general conditions of the body designated by the terms excitement and re-action, sthenia or asthenia, weakness, constitutional tendency, disposition to disease, &c. And yet no one can say what images these expressions actually call up in his mind, or what peculiar meaning he attaches to them.

But if no one is in a position to say so much as this with regard to the words he uses, it is clear that the things denoted by those words cannot be exactly known,—that our understanding of them cannot be regarded as even approximately correct. Every one who uses these words is, perhaps, impressed with the idea that he wishes to express such and such a meaning by them, and that, consequently, he uses them in a determinate sense. And such, indeed, may generally be the case. But it is a sense void of reality, *i. e.*, of clearness and precision, and cannot at present be otherwise. For the sense or conception attached to such words has itself arisen in a confused and indistinct manner, and is founded on complicated and exceedingly defective, if not altogether erroneous, views. For it is ultimately the offspring of all that we have heard, read, or thought of from our earliest years, but whose correctness neither we nor any one else have yet been able to demonstrate. Continually do we see ourselves reduced to the necessity of observing, comparing, judging, and finally of denoting by words, various phenomena and processes which we understood but partially or not at all; concerning which, therefore, we do not know whether they really take place in the manner in which we represent them to ourselves, and whether we are justified in saying with regard to them, that which our words are intended to express. Nearly all the expressions which we use in medicine are, therefore, rather to be considered as short descriptions, or pictorial representations, of our existing and always defective and indistinct conceptions of this or that matter, than as a real and truthful delineation of the things themselves.

It may, indeed, be safely asserted that our technical language is but, as it were, a reflection of the imperfect state of our knowledge; that, in short, we have no true scientific language at our disposal, simply because we have no science; that we wander about in words and phrases, just as astrology and alchemy did some centuries ago, because we are at present at about the same degree of advancement as those sciences then were. Should we accord to chemistry and physics the rank of sciences if they designated their phenomena and processes as we designate ours?

But our language, instead of contributing anything to the advancement of medicine to the rank of a science, and at least opening the way to clearer conceptions by providing definite expressions for our thoughts, does, in fact, produce an effect almost contrary to this. So far, indeed, from expressing by it the real nature of the objects to which its words are applied, it does not even enable us to say with correctness and precision what we ourselves—in default of a better understanding of those objects—think and imagine respecting them. To expect our language to express all the objects of medical science correctly and absolutely, as they exist in reality, would, according to the preceding remarks, amount to exacting impossibilities from it. For that which is beyond ourselves, that which is not present to our own ideas, or contained within the circle of our knowledge, can certainly not be expressed in words. But so much at least may we expect from our language, that it shall represent, without obscurity or distortion, that which we think and know respecting those objects; that it shall not supply us with words which, for the most part, do not even say what we wish to express, and consequently introduce a meaning quite different from that which we intend, or which, according to the actual state of our knowledge, have no meaning at all. Or if they have a meaning (such as the expressions for anatomical, chemical, and physiological objects), this meaning is nevertheless seldom sufficiently fixed and definite, but rather admits of extension and contraction according to individual fancy; and the greater number of our expressions are exactly for this reason calculated to favour obscurity rather than understanding, or perhaps to afford the means of rambling in all directions, and to arm us in our controversies with a weapon adapted equally well for attack or defence.

A language like ours always holds out the temptation of supplying, in a certain degree, the want of insight and knowledge of facts, by a free and unconstrained, if not reckless use of words, and by expressions and phrases which mean anything or nothing. And this state of things is especially promoted by the use of foreign words so frequent in German science. These words, indeed, more than any others, afford a hiding place and cloak for our ignorance or defective knowledge, whereas by the mere use of the corresponding words in the mother tongue, the unmeaning and often absurd character of our foreign words would at once be brought glaringly before our eyes. Thus, like all technical terms, they expose us to the danger of imagining that we understand any matter much more clearly than we do in reality, and thereby stand in the way of the improvement of our knowledge, indirectly at least, and again in a much more direct manner, inasmuch as, with such a language, we can never attain to such improved knowledge. Let any one read the first work on diseases and their remedies that comes to hand, provided it does not give merely the appearances actually observed, but attempts also to explain them, and he will soon discover that nearly every word is a mystery, an allegorical picture, perhaps a bold comparison or hypothesis, or perhaps, again, totally devoid of meaning.

It is true that most writers,—iatro-anatomists and physiologists, for example, as well as the iatro-chemists—now clothe their language more and more in the garb of a certain exactness or scientific precision. But it is merely the shell, the form, and not the kernel of a scientific language, and cannot indeed be otherwise, so long as we are destitute of science and sound knowledge. With all our *stases* and *crases,* and *-æmias,* indeed, we have not gained anything very satisfactory.

Nevertheless, in the actual state of things, we can do no more than accept our language as it is. It is not, indeed, in the power of men to change their language altogether, and still less is it allowable for individuals to introduce into our technical language words which differ from those in general use, even though they may be much more correct. Moreover, it is tolerably certain that we shall always have a number of more or less indefinite and ambiguous words, without clear and well-established meaning, because this uncertainty lies, to a considerable extent, in the very essence of the matter; because, in fact, the things denoted by these words are themselves sufficiently ambiguous, doubtful, and variable, and therefore our knowledge of, and ideas respecting them, cannot easily assume a more determinate character.

All that our logic can do under these circumstances is to teach us to use even this instrument, with all its errors and imperfections, as correctly and as consistently with facts as possible. Its province is, in general, to show how these words have come into use, how they have gradually acquired their actual signification, and no other, and in what sense they must be used at the present time. Only so far as all this is clear and distinct to any individual, will he be able to make a right use of his language, in spite of all its defects, to think clearly and correctly in its words and phrases, and to express his thoughts and conceptions by means of it, with the highest attainable degree of perfection. And to facilitate this end as much as possible, logic must further point out wherein these defects, ambiguities, and obscurities in our language especially consist, how they have arisen in course of time, and how they may in future change their character. With equal distinctness, too, must it warn us against every application of its expressions in an erroneous sense, and thereby remove, as far as possible, every tendency to error on this side, and in general, every danger to which our understanding is liable from the use of our language.

These considerations confer peculiar importance on certain words and phrases, which aim at expressing more than others, which have a more general and comprehensive meaning, a higher current value in the language, and whose more particular examination possesses, therefore, a twofold interest for our understanding.

The words in question are those which are called *collective words:* they play an important part in *our* technical language, as well as in all others. Thus, for example, the names and expressions for the different functions of the body (such as respiration, digestion, diuresis, organization); or for our several maladies and their causes (*e. g.*, typhus, fever, scrofula, or cold, contagion, poison, malaria,); or the words by which we usually express the mode of action of many of our remedies (*e. g.*, tonic, solvent, astringent, caustic, &c.)

Such expressions have always been necessarily used, because both observation and the deduction or formation of our ideas and conceptions therefrom, as well as the practical necessity of expressing them in words, equally led to their employment. Moreover, certain species or series of phenomena, processes, actions, and other things have been designated by the same name, because they seemed to agree more or less in certain essential qualities or circumstances—as, for example, the several diseases, influences, remedial or poisonous actions, &c. The observation of these phenomena in a certain number of particular cases, and the more exact comparison of them one with another, have led to the conviction that, by virtue of this agreement, they are in several, perhaps in all points, regarded as essential, either perfectly similar, or, at all events, allied and analogous to one another.

Whether and how far this conclusion is correct in individual cases, may for the present be regarded as indifferent. It is sufficient to know that the observation and comparison of these phenomena has led to their being regarded as essentially and completely identical, or as nearly related and similar, or generally as connected together. Phenomena thus related have been designated by the same name, and thus have arisen the collective terms of medicine and physiology, as well as in ordinary language—*e. g.*, the words "weather, vegetables, fruit, corn;" or "noble, aristocrat, gentleman, citizen;" or "intelligence, virtue, vice," &c. By a process essentially similar, also, the descriptive natural sciences, such as botany, zoology, and others, have arrived at their names of species and orders; and chemistry at such expressions as metal, alkali, base, acid, salt, &c.

In thus designating certain series and instances of phenomena or objects with one and the same name, we at the same time express our conviction that they are more or less of the same kind, or even perfectly identical. For in our ideas, in our mode of representing these phenomena to ourselves, and thinking about them, they are actually regarded as such. At the same time we give a sort of provisional definition of them, and establish a kind of grouping or classification of the things themselves; or at all events, we make the first step towards it. We do, in fact, declare respecting them, that they are related, and perhaps identical, and that, on account of this resemblance or identity, we denote them by the same name.

Moreover, this use of collective words is perfectly correct and well founded, provided always that the objects thus designated be really, as we suppose them to be, similar, and related to each other, which is certainly the case with the species and genera of the zoologist, the botanist, or of the chemist, as exemplified in the words vegetables, fruit, food, &c.; more or less, also, in the names of certain diseases—*e. g.*, syphilis, small-pox, intermittent fever, &c. The correctness of collective words of this kind requires, therefore, that such things only shall be classed together, and designated by the same name, as actually agree together in all, or at least in certain essential points. And, on the other hand, all those must be excluded which differ in these particulars, and, perhaps, on account of their properties, or their internal relations, or from any grounds whatever, belong altogether to another species.

But it is precisely this most essential requirement that, in the present state of our knowledge, as formerly remarked in speaking of comparisons and analogies, we are, comparatively, but rarely able to satisfy; so soon, at least, as we have not to deal with objects, processes, &c., which are exactly of the same kind—*e. g.*, with one and the same function, disease, or external influence, or, in general, with things which are united by nature in a *group* or *ensemble*, and precisely for that reason may be recognized as such by an ordinary observer. And if this proof of the actual agreement of things which we classify together is seldom enough possible, even at the present day, it is easy to understand that such proof must have been still more difficult to obtain at the time when these collective words and names first came into use. Those, indeed, who originally employed them, far from being acquainted with all the points, qualities, and circumstances, which the objects included in the collective name or idea might possess in common, and consequently designating those objects by a common name by reason of such knowledge, were guided rather by a confused and uncertain imagination and supposition of their resemblances, or by an arbitrary *a priori* assumption of them. Such supposition or assumption might indeed be right, but it might just as well be wrong, and completely opposed to the actual state of the matter. For, inasmuch as we can never attain to a correct understanding of the equality or similarity of our objects, processes, &c., from any *a priori* considerations, but solely by fixing their common points from observation and experience, it is clear that, when this experience was wanting, there could be no solid foundation for any theory or classification: at all events, no actual proof could be given, and so, indeed, the matter has essentially remained up to the present day.

Such, for example, is the case when we designate certain classes of morbid states or attacks by the same word, such as scrofulous, rheumatic, gouty, &c.; or as epilepsy, inflammation, leprosy, plague, dyspepsia, hemorrhoids, and the like. For in thus giving the same name to states and disturbances of various kinds, we likewise attach to them one and the same predicate; we declare, in fact, that in certain essential points, especially in their mode of production, their conditions, and their internal organization, they exhibit a general agreement. And yet our understanding does not readily supply us with the proofs required to establish this agreement; indeed, we rather arrive at our idea of their similarity or identity by certain resemblances of external appearances and symptoms, or of their origin, progress, and the like. In like manner have our names for the similar or analogous modes of action of our remedies on patients been formed; such, for example, as "antiphlogistic, alterative, solvent, tonic or strengthening, stimulating, purgative," &c. It has always been imagined that the favorable alterations and gradual cures, in inflammation, tumours, indurations, &c., or in various states of weakness, &c., were more or less brought about by the different remedies as they successively came into use. For that reason they were called antiphlogistics, antispasmodics, tonics, &c.

These words, however, could not be the symbols of any clear, definite notions, because they had not been arrived at by the road of sure experience; because, in fact, it had not even been demonstrated that those remedies would generally act in the manner assigned to them, and still less that the mode of action in certain essential points would be the same in all, or in many of them. It was rather imagined that their mode of action was of such and such a character, because they were found to cure certain diseases; and as this conclusion rested on no other grounds, it became, as it were, necessary to regard the mode of action of these remedies in such a manner, and to make such suppositions respecting it as were most consistent with the actual views entertained regarding the cause and so-called essence of the disorder which they were used to cure. And as, in the course of time, the views upon these latter points changed, so likewise did the theories entertained regarding the action of the remedies change, for better or for worse, with them.

So one, for example, who regards hemorrhoids as essentially produced by obstructions, and the like, in the hepatic circulation, by infarctus, &c., the action of remedies employed against that malady will appear in quite a different light to that in which it will be regarded by another, who looks for the origin of the disorder in a disturbance of the nervous system or of the digestion. For, while the former ascribes to them a "dissolving purgative" action, the other thinks, perhaps, on their favorable action on this or that province of the nervous system, on the process of digestion, &c. The former would perhaps describe his remedies as "resolving;" the latter as "antispasmodic," sedative, or strengthening.

Thus the followers of Hippocrates and the Arabs entertained quite different notions upon all these matters to those of the Galenians, for example, and consequently used totally different names and expressions; and, in a similar manner, a disciple of Broussais differs from a pathologist of the present time; a so-called nerve-pathologist, again, from a humoral pathologist; and a modern chemico-physiologist talks, perhaps, of the antiseptic, anti-fermenting, or oxidation-stimulating action of his remedies. But that our remedies act exactly thus and not otherwise, and that they have therefore an exclusive right to this or that predicate, this or that name, has not yet been demonstrated by the moderns any more than by their predecessors.

It is otherwise with the action of many substances on healthy persons, provided these actions take place with sufficient distinctness and constancy; as, for example, in the case of laxatives and emetics, or of caustic, acrid, or narcotic substances, and the like. Such substances form definitely bounded groups or species of essentially the same action; they possess, therefore, essentially equal or similar properties, and are rightly called by the same name.

If, now, our knowledge is to be furthered by the use of such words and phrases, we must, in the first place, be able to say, from positive experience of the groups or kinds of phenomena, actions, or objects thereby denoted, that they do actually agree in all the points and qualities which their collective name expresses or involves. This, however, according to what precedes, we are seldom able to do completely. Experience rather shows that we often enough regard totally different morbid states, actions, &c., as essentially the same, taking them for a kind of unity, and accordingly denoting them by the same word, merely because, at the first view, and from a superficial or inexact observation and comparison of their symptoms, progress, and so forth, they appear to agree more or less in certain properties and circumstances. Moreover, we have habitually laid the greatest stress on such of their properties, and on such points and relations as appeared to us to be the most important, and best capable of affording a standard, in short, as the most essential. But we have scarcely ever been able to give any proof of this—that is to say, whether and how far the qualities and relations so selected were in reality such as we supposed, and whether and how far, therefore, the objects themselves were rightly classed together and denoted by the same name. And because in this, as elsewhere, there has always been a want of a sufficiently sure basis for our notions and judgments, and of a well established understanding, and consequently, also, of fixed principles for the union or separation of our objects, each one has gone his own way, according to his own preference and inclination.

That our collective words must from this cause suffer much greater loss of clearness and precision than any of our other expressions is perfectly evident, and the confusion of our ideas of the things thereby denoted is likely to be increased in an equal degree. Names of diseases, such as nervous fever, plague, consumption, dropsy, rheumatism, epilepsy, are used sometimes in one sense, sometimes in another, and consequently the same states are denoted with this word by one person, with that by another, so that no one knows exactly what he has before his mind, and wishes to designate, and why he designates it by a certain expression and no other. Similarly we daily use such words as " strengthening, tonic, solvent, purgative, astringent, stimulating, exciting, &c., in the most various ways. Almost every one accepts them with peculiar limits and in a peculiar sense; and even one and the same expression is used, according to circumstances, in anything but a uniform sense. Thus one may say of good wine, of cold bathing, or fresh air, that it exerts a strengthening action, and the same may be said of nutritious diet, of cinchona-bark, and various bitter substances. They may, indeed, all and severally, in a certain sense, exert an action of this kind; but it is equally certain that in each one of these means and influences, the action must be of a peculiar kind, distinct from that of others. We use then the same word in different senses, as applied to wine, to fresh air, to cinchona, and to roast meat. And the same may be asserted more or less of all collective words of this kind used in medicine.

To this it must be added, that these words, in consequence of their wider and less defined sense, are much more liable than others to change in course of time. Their meaning has been sometimes extended, sometimes restricted, according to the increasing experience and insight, or to the theories and caprices of individuals. Thus we now apply the terms tubercle, inflammation, typhus, paralysis, cancer, or narcotization, tonic treatment, &c.—in the former case to morbid conditions, in the latter to certain modes of action which were formerly not referred to the same classes, and therefore not denoted by those words, possibly because they were not then sufficiently well known, or that the views entertained respecting them were totally different from those of the present day. And, on the other hand, we now no longer apply to many diseases such terms as nervous fever, epilepsy, paralysis, and other so-called idiopathic nervous affections, but rather call them pneumonia, phlebitis, tuberculosis, or softenings, pressure, &c., upon the brain or spinal marrow, because we have become acquainted with certain anatomical changes connected with those diseases, and these changes are regarded as the essential conditions of the diseases in question. In like manner, many substances and remedies which were formerly spoken of as tonic, astringent, diffusive, &c., are now no longer designated by such terms, because further experience has shown that their action is of a different character.

But the chief misfortune connected with these and other expressions of technical language is, that they have long been used in a certain, though obscure and ambiguous sense. And, however ill-adapted, or even faulty and confusing they may afterwards be found, it is very difficult to alter them, or even to get rid of their signification—*i. e.*, of the images and notions formerly connected with them.

But here, again, arises further opportunity for their confusion and obscurity, there being frequently a direct contradiction between what we mean and wish to express, and that which the words we use actually convey.

Far more important, however, is another consequence to which these collective words and their use have led almost of necessity—viz., to a kind of materialization or embodiment of the ideas and images therewith connected, making them into an actually existing thing (Ontology, Realism).

Thus from all the appearances manifested by patients suffering under the same disease, a certain homogeneous picture of this disease has been put together; or, in other words, the idea of typhus, intermittent fever, scrofula, &c., has been abstracted from these appearances. In like manner, from the actual or so-regarded effects of all sudorific, diuretic, or antiphlogistic remedies, the idea of diaphoresis, diuresis, antiphologosis, &c., has been constructed and embodied. Thus, on the one hand, in place of the individual concrete, patients exhibiting such and such phenomena; and, on the other hand, in place of the individual actions of certain remedies, a certain idea and abstract image, applicable to all, has been substituted—a kind of self-existent thing or being (Ens) with its particular name. For, in our imagination, we have gradually accustomed ourselves to regard this idea as in a manner separated from the phenomena exhibited by certain patients, which phenomena originally led us to that idea and its construction.

A precisely similar series of changes has been exhibited by the formation of our language and ideas with respect to other matters, in ordinary life, as well as in other technical languages, because, in fact, it is necessitated by the nature of things, and more especially of our intellectual capacity. For example, just as we have arrived at the ideas and names of Beauty, Virtue, Conscience, Honour or State, Law and Opposition, the physicist has also arrived at his abstract conceptions and names of Galvanism, Magnetism, Electricity, Light, Heat, Cold, Wind, Storm, &c.

In this, as in every case, partly for the sake of convenience and brevity, partly in consequence of a certain carelessness, we do not think clearly enough how we have arrived at these ideas and modes of expression. To the names themselves (*e. g.*, typhus, diuresis) we now attach certain qualities or predicates, which belong, in reality, not to them, but rather to the phenomena, processes, or actions, from which these ideas have been derived: of the latter, in fact, we predicate that which should really be said of the former. Thus, we speak of a typhus, an inflammation, tuberculosis, or epilepsy of a patient, and not of a patient suffering under certain disturbances comprised in those ideas and words. We now speak of symptoms and qualities, of certain peculiarities of the disease, not of the patient;

of certain properties and actions, of antiphlogosis, diaphoresis, or tonic treatment, and not of the substances or means which have been used and observed. In like manner the physicist speaks of electricity, heat, wind, and weather, and ascribes to them certain properties and effects, just as, in ordinary discourse, we speak of virtue, conscience, honour, or of art and science, law, and state, as something actually existing, and of what the one commands, the other forbids, &c. Thus, instead of saying that a heated or cooled body behaves in such and such a manner —*e. g.*, expands or contracts—we say, it is a property of heat to expand bodies, and of cold to contract them.

Here, as in the former case, a certain ideal picture, an abstract conception, of heat, for example, in the one science, or of typhus, &c., in the other, is formed and perfected, and this image we then represent to ourselves as clothed with the attributes which, under certain circumstances, have been observed in a substance or in a patient. In our thoughts and imagination, the two have become so united and amalgamated, that we now attribute to our abstract conception, all that in reality belongs only to the substances, men, &c., wherein these phenomena, processes or properties are exhibited. And just as the physiologist, for example, says of respiration, digestion, &c., that it goes on in such and such a way, is abundant and in order, or scanty and disturbed, so likewise does the physician speak of the origin of a disorder, of its first appearance, character, progress, &c., that they exhibit certain peculiarities. In short, he treats his idea of the disorder as if it were an actually existing thing, instead of being the creature of his own imagination and mode of thinking upon it. He makes his disease increase and decrease, become complicated, change, exhibit periods, and intermissions, &c. Thus it is said to be a property or peculiarity of "inflammation" to arise and progress with fever, to have or to produce the so-called buffy blood, to form such and such exudations and products, to be malignant or non-malignant, to come to a crisis, or continue long uncertain; perhaps even to become nervous or gangrenous. And if a patient has become ill from any action, direct or indirect, of patients of the same kind, if, for instance, he has contracted itch, syphilis, plague, or small-pox, then the disease itself is said to be of contagious character, and we speak, moreover, of a certain something or principle that originates it—*i. e.* of its "contagium." Similarly again, we speak of the treatment of a disorder—for instance, of measures directed against inflammation of the lungs, typhus, &c., and how this or that remedy is required or "indicated" by them. The remedies are, in fact, supposed to act upon the disorder, not upon the patient, and we speak of specifics against syphilis, gout, intermittent fever, &c.

In short, the idea which we have formed from the observation of certain phenomena, actions or processes, and from our conclusions and theories respecting them, identifies itself to a certain extent in our imagination with the observed phenomena and objects themselves; we confound the two from habit, and because we do not clearly enough remember or consider the actual relation of the matter. And this due consideration, again, we generally omit, because the very names and forms of expression which we apply to those phenomena, &c., entice us to such omission,—especially our substantive-words (such as typhus, electricity, heat, or astringency, antiphlogosis, diaphoresis, &c.), in which our ideas have been, as it were, clothed and embodied. The idea once separated and embodied—that of a disease or a remedy, for example—we can no longer imagine it without certain properties, without such and such kind and peculiarity of appearance or action, &c., any more than we can make such abstraction with regard to any other object which we actually perceive, or of whose existence we have otherwise any knowledge. For our knowledge and idea of an actual object is likewise arrived at by the perception of a certain kind of appearance and relation, and of certain properties belonging to it. A botanist, for example, cannot think of a rose or a pink, a zoologist of a raven or a monkey, or a chemist of an acid, metal, or salt, without ascribing to it certain properties, such as form and structure in one case, composition in another, &c. And just in the same manner a physician cannot think of typhus, diaphoresis, and the like, or a physicist of heat, light, or electricity, without connecting with them certain qualities or attributes.

There is, however, this essential difference, that the objects with which the botanist, chemist, &c. has to deal, are actual bodies, certain really existing, and definite forms or substances, which is not the case with those into which the physician has to inquire. And because states of disease, such as typhus, and scrofula, or modes of action, such as diaphoresis, antiphlogosis, &c., never actually exist as such, and because the very phenomena and properties which lead to the idea of those conditions, effects, &c., cannot even be directly observed, and moreover, are anything but constant, it is not possible that the words above mentioned should denote a uniform, homogeneous object. For the same reason, it has hitherto been impossible to connect with the word "typhus," &c., any clear, definite, and constant meaning. For the abstract idea of typhus, &c., has itself been arrived at in an indistinct and generally uncertain way, and by a kind of confusion or ignoring of the real state of the case.

It is true that in the daily use of these words and ideas, the uncertainty in question is easily overlooked, and we use them as if their meaning were always the same. We think, perhaps, that we always use them in the same sense, whereas, in reality, different persons use them in different senses. Thus, accordingly as one man has seen, read, or heard of this typhus-patient, another of that, each will thence have deduced his own particular idea of typhus, and therefore each will use the word in a different sense from the other. Many an attack of disease may be regarded as typhus by the one, while the other may apply to it quite a different name. And one who, with respect to inflammation, lays the greatest stress on certain symptoms, such as fever, pain, or a certain state of the blood,—an increase of fibrin, for example,—or on the formation of certain exudations, &c., will deny the applicability of the term inflammation to many states of disease in which these symptoms are wanting, whereas another may consider it fully applicable.

Moreover, our experience and knowledge with regard to such diseases, and the effects of various influences, medicaments, &c., are constantly progressing, and consequently our views thereupon must undergo continual variation. For example, in typhus-patients, in inflammation, certain processes and peculiarities (*i. e.* anatomical, physico-chemical, and microscopical) have been made out, which formerly were unknown. And, on the other hand, it has been ascertained that many phenomena and processes, which were regarded by our predecessors as characteristic, essential, and determining, are rather of accidental and subordinate character, or at least, that they occur in the same manner in patients of a totally different class (*e. g.* fever, certain disturbances of the nervous system, the formation of certain exudations, &c.).

Just as likely is a case to arise in which this or that appearance and property, which we had hitherto regarded as constant and characteristic, is altogether wanting. In our imagination we have always regarded these characters as essential members and indications of a disorder such as typhus, for example, and now all at once we miss what we have been accustomed or had expected to see. We are thereby thrown into perplexity and doubt, as to whether these anomalous cases should or should not be designated by the same name as the rest.

Any one, for example, who from previous experience has been led to believe that in typhus-patients, diarrhœa and intestinal ulcerations, or a certain exanthema, violent headache, and delirium, &c., are constant, and even essential phenomena, would not know exactly whether to include in the same class, other patients, who, in many respects, exhibit the same symptoms, but without the diarrhœa, ulcerations, or exanthema observed in the former. The same difficulty would occur to the physician on meeting with a scrofulous patient without swollen glands in the throat, &c., a patient attacked by some inflammation without fever, pain, the so-called buffy blood, &c., or in general with patients without those peculiar symptoms and changes to which he had previously attached the greatest importance, and without which he had not been accustomed to suppose the particular malady to exist. In the same manner, a zoologist, previously acquainted only with black-feathered ravens, having a peculiar form of the beak, toes, &c., might, on seeing other ravens with grey feathers, or a somewhat different beak, be in doubt whether to include them in the species "Raven" or not.

If, however, it should appear, on further examination, that even these latter patients, in spite of such and such deviations, agree, nevertheless, with other typhous, scrofulous patients, &c., in many other and perhaps essential points, they must, notwithstanding all those contradictions, be designated by the same name. For we have but one word for such patients. When, however, we apply the same words also to patients exhibiting the anomalies just noticed, we feel that we are not using it in the same sense as before, because, in fact, the picture presented to us by such patients contradicts, in many respects, our former and accustomed notions. And just as the zoologist, botanist, &c., under similar circumstances, forms his species together with varieties and sub-species, so likewise does the pathologist establish many sub-species and transitional forms of his diseases. He speaks, for example, of exanthematous, contagious, and abdominal typhus, of scrofulous, rheumatic, gouty inflammation, of catarrhal or typhoid pneumonia, of syphilitic and scrofulous eruptions on the skin, of nervous and pituitous consumption, &c.

But in course of time the variously modified and variable sets of phenomena exhibited by such patients become amalgamated, and grow, as it were, in our imagination, into *one* picture. We abstract or form to ourselves, out of all these varying and manifold forms and accidents, a more general and comprehensive idea—*e. g.*, of typhus, inflammation, &c., just as the zoologist or botanist forms the idea of a genus.

Since, however, this general idea, still more than that of an individual disorder, is developed in an undefined, if not confused manner, from all the varying appearances, changes, &c., exhibited by those patients, taken together—from all, in fact, that has successively been seen, heard, or read concerning patients suffering from typhus, scrofula, &c.—our conception of it must be more or less confused and variable. Moreover it is seldom, if ever, possible to make our observations on all the patients designated as typhous, gouty, scrofulous, epileptic, &c., or to examine and compare their several conditions in such a manner and to such an extent as to discover that which is constant and common to them all, much less that which is actually characteristic and determining. On this account, we are often unable to form a positive judgment as to whether and how far such and such phenomena, changes, &c., regarded as constant and essential, occur in all these patients, whether such phenomena arise or may be determined in the same way in all, and possibly have the same signification for the whole course of their disease.

For this reason, every one must, for better or for worse, confine himself to those appearances and circumstances whereby he himself has been led, according to *his own* experience or theoretical views, to designate these attacks of illness as typhus, scrofula, &c. But these phenomena and circumstances themselves are, as already observed, of exceedingly variable and uncertain nature, may sometimes be present, sometimes not, or may exhibit themselves under the most various modifications and combinations. And yet, in our ignorance of the particular conditions, of the internal connection or conformity of those states of disease, our observation and comparison, and therefore our judgment, can rest only upon their external appearance, or, as it were, on their upper and exterior surface, *e. g.*, on certain symptoms of general or local character and their progress, on certain anatomical or chemical changes, on the constitution and general character of the patients, &c. Hence it follows that no one has a comprehensive, clear, and exact notion of those maladies—*i. e.*, such a notion as would have been derived from actual understanding of them. And just as little, as above noticed, can any such expressions as typhus, inflammation, scrofula, &c., be used by all in one and the same sense, or excite in all the same image, the same idea, because almost every one has his own ideas on the matter, according to his particular experiences and views.

Unless, therefore, the case is perfectly distinct and well characterised, two physicians will scarcely agree in their diagnosis and consequently in their denomination of it, and in the several points relating to it. Even the same physician may at different times arrive at different conclusions respecting it, according to his further experience, his age, and the theories and points of view which he successively adopts.

The same is true, indeed, with respect to all the influences which act on man, as with respect to the action of our remedies and their denomination, so long as we are deficient in a sound understanding of them; in fact, our ideas, interpretation, and judgment of them, must vary continually with our knowledge and experience.

It is, as already observed, scarcely possible to make any radical improvement or alteration in the form and manner of our expressions, and their use. They are bound up with the very nature of the matter, and ultimately with the entire conformation of our intellectual nature, and especially with the origin and derivation of our ideas, as much as with the defective state of our knowledge.

Hence, also, we may understand how it is that many opponents and sharp critics of these modes of expression and representation nevertheless make use of them, and thus, in spite of all their criticism, ultimately fall into the very error which they condemn. All that an individual can do under existing circumstances is, to make out clearly the real meaning of his expressions, how he has arrived at them, and, finally, how he is to use them in their true sense, and in a proper manner.

But, on the other hand, it follows from the very nature of the thing, that general terms and ideas, such as ours, the exact meaning of which no one is able to determine, cannot possibly admit of precise or rational definition. We can say, indeed, what view *we* take of the things to which we apply those terms, and in what sense *we* employ them and wish them to be employed by others; but we are unable to say whether and how far we are right, so long as we are deficient in a clear and definite understanding of the things themselves.

Nevertheless, neither science and its language, nor any individual for himself, can avoid making an attempt at such limitation; and this attempt at a more exact fixation and limitation of the sense of our words constitutes *definition*. We thereby state, in few words, our theory, our confession of faith, on the objects and phenomena thus designated—*e. g.*, on a disease, on the properties and effects of a medicament, &c. Very many words, indeed, contain a kind of definition within themselves,—*e. g.*, "disease, poison, malaria, food, remedy;" also "intermittent and nervous fever, melancholy, tonic, astringent," &c. It is likewise self-evident that the possibility of such a definition implies a previous examination and comparison, a certain knowledge, in fact, of all the objects denoted *by the same word*. We must first determine their more important and characteristic qualities, and all the more essential circumstances relating to them, before we can define them in such a manner as to say what they are, and wherein they consist.

This end is most easily attained in the descriptive natural sciences, such as botany, zoology, anatomy, &c.; for all that these sciences require for the definition of their objects they are able to observe more or less directly and comprehensively. With nearly the same certainty, and partly for the same reasons, the physicist and chemist may form their definitions; partly, also, because they are more or less acquainted with the causal connection and order of succession of many of their objects and phenomena. Thus they can state in a few words what they mean by heat, light, electricity, gravitation, or by oxygen, carbon, alkali, acid, or by oxidation, combustion, fermentation, and the like, and are therefore able to define these words with more or less clearness and certainty.

But it is quite otherwise with our science, because we can give no correct and admissible definition of things which we know but inexactly and, as it were, from without. For this reason, indeed, we can scarcely flatter ourselves that we are able to give a precise definition of any process in nature, but least of all is this possible with regard to the phenomena and processes of the living body. For we have never been able to observe these things either in healthy or in diseased subjects, directly and in their whole extent, but have rather been compelled, from the little that we have observed, to draw conclusions respecting the rest, and, in fact, the greater part. Hence our definition, like our knowledge, can only relate to and rest upon so much of a function, disease, medicament, &c., as is known to us, or as may appear, in the imperfect state of our knowledge, to be the most constant or determining and essential. But whether, and to what extent, this is true, we know not, and we are unable to prove.

We are, therefore, not in a position to give a perfectly correct and admissible definition of anything in the whole circuit of life, either a function, a disease, or any individual process, or of the effects of any external influence on the living body, or of any medicament upon a diseased person. In the one case, as in the other, we know far too little of their conditions and laws of origin, or action, to enable us to extract therefrom any exact definition.

For example, we have as yet no precise knowledge of all the phenomena and conditions, or processes, which a patient must exhibit to justify us in designating him as typhous, scrofulous, arthritic, epileptic, &c. And still less can we state with certainty in what essential points all the patients or disorders agree, which we have denoted by the same word, typhus, scrofula, gout, epilepsy, &c. We do not know with certainty what kind of conditions, what peculiar processes are present in them all, or whereby, and according to what laws, they have been produced and have progressed. Our idea of them is based rather upon a sort of general and more or less superficial impression of resemblances in these several patients, on their agreement in certain points and symptoms which appear to us as the most important and essential, and perhaps are really so, more or less, but cannot with certainty be established as such. And if, in our diagnosis, we would designate a new case by such and such a name, there is nothing left for us but to fix the attention on those resemblances, to follow out the appearances exhibited by the patient before us as well as we can, to compare, and lastly, to make out in what points he may agree with this or that earlier conception and picture of a disease. We then include him under that one among them which he appears most nearly to resemble, especially in those points which we regard as most important, and give him a name accordingly. But with actual understanding of the matter, and consequently with characteristic definition, we are here not much concerned.

The preceding observations likewise point out and explain why we are less able to give satisfactory definitions in proportion as the objects, or our ideas of them, which we wish to define, are more complicated and less distinct. Least of all can we attain to any rational and correct limitation of ideas respecting those collective words whereby we are accustomed to include entire and manifold series of phenomena, processes, actions, influences, &c.,—*e. g.*, a definition of typhus and antiphlogosis, or of organism and organization, of life and disease, cure, medicament, or of contagion, miasma, malaria, constitution, and the like. We are as yet acquainted only with separate elements and parts, with particular sides of this vast group of effects or causes ; we know but fragments of them, and these by no means perfectly. It is therefore impossible for us to determine anything with certainty and perfect correctness; what, for example, is true with regard to all the processes going on in and peculiar to the living body, that is to say, with regard to life generally and in itself; or, again, with regard to all diseases, medicaments, and their properties, &c. We can, indeed, predicate this or that universal or common property respecting them, and perhaps, under the most favorable circumstances, may make some degree of approximation to the truth. But we cannot give a definition of them founded on a comprehension of their laws and all essential circumstances relating to them, and therefore worthy to be regarded as convincing and satisfactory, and to remain for ever undoubted and uncontested.

Such a result may perhaps be attained, at least approximately, in the purely descriptive natural sciences, and even in physics and chemistry, but certainly not in medicine. Instead of actually defining our several varieties and series of aggregates of effect or cause—*e. g.*, of all vital processes, diseases, &c., and thereby predicating something respecting the laws which they possess in common, we find, on making the attempt, that we can define or describe the former only in short words. We must not, therefore, think of an exact fixation or statement of our ideas respecting them, but only of a more or less correct but limited description of what we have observed in connection with them, that is to say, of those phenomena, effects, and circumstances, so far as they may be known to us, and regarded as the most constant and essential.

But, on the other hand, we find it impossible to renounce altogether this wish to define the objects of our investigation. The requirements of our intellectual existence, as well as of language and communication, lead every one to this as well as to a general wish to understand the operations of nature. And accordingly we make out our definitions in the best way we can. As in other cases, we thereby express shortly and plainly our confession of faith, our theory respecting a disease, function, morbid influence, action of a remedy, &c., and accordingly rest our definition on all the points and circumstances connected with it which appear to us to be the most constant and essential, in short, as the pith of the matter.

But our view of these circumstances changes continually with the point of view from which the objects themselves are regarded by us. It changes, therefore, according to the particular end, the particular science that we have in view. Thus, to the zoologist and botanist, and even to the anatomist, the parts and aspects of a living body, of an organism, which appear the most important, are different from those in which the physiologist, chemist, or physician is chiefly interested. While the former, in his own special capacity, is concerned only with the recognition and description of external form and properties, structure, &c., the attention of the latter is directed to totally different objects—*viz.*, to the study of the apparatus or organs, the fluids and solids of which the living body is ultimately composed, and still more of the mode of action of each upon the other, and their perpetual changes and indications of activity; in short, of all the occurrences and processes in the living body itself. For a similar reason, also, the point of view taken by the latter in forming their definitions will differ altogether from that selected by the former; and to the physiologist or physician, again, it will be different from what it is to the chemist; and consequently the several definitions thus arrived at will exhibit corresponding differences of expression.

Moreover, the expression of a definition will of necessity vary continually with the progress and development of the particular profession, or science itself, of physiology or nosology, for example, as of medicine in general. And because those most constant and essential points, the laws which regulate the phenomena and processes of life, are precisely those about which our knowledge is the least satisfactory; and because our views respecting them must constantly change with our knowledge and with the progress of our intelligence, the definitions in our science must also, even more than in others, be subject to continual variation. Older definitions are constantly being recognized as more or less incorrect, narrow, and one-sided, and are therefore overthrown and abandoned to give place to others more exact. And the same fate attends our present definitions, and must necessarily do so till we shall have attained in our science to a sure and reliable understanding, and to well-established laws of vital processes, diseases, &c.

Thus, for example, the terms disease and remedy were formerly understood and therefore defined quite differently to what they are now; so, likewise, are the meanings and definitions of inflammation, pneumonia, typhus, gout, lithiasis, &c., different from those which were attached to them thirty years ago, now that we have become acquainted with certain alterations of the organs and their texture, composition, &c. And whereas, in fever-patients, for example, the ancient Greeks and Arabs regarded the increase of bodily heat as the most essential circumstance, and therefore restricted their appellation of the disorder (πυρ, πυρετος, πυρεξις), as well as their definition, to this particular symptom, Boerhaave also looked upon cold, horripilatio, and the *pulsus velox*, as constant and essential disturbances, or as *causa proxima* of the disease, and therefore included them in his definition of fever. We, on our part, have advanced somewhat further, we have discovered that those phenomena themselves depend upon certain other processes and circumstances—*e. g.*, upon certain alterations and processes in the circulation of the blood, in the process of respiration and internal oxidation or conversion of tissues, in innervation, &c. And as these latter are now included in our definition of "fever," the symptoms above-mentioned, *viz.*, the increase of heat, the pulse, &c., have come to be regarded as conditional, variable, or accidental, and, as such, excluded from the idea of "fever."

Essentially the same change, and from the same causes, has, in course of time, affected all our definitions of diseases, of the manifold influences and agencies of the external world, and of our medicaments and their effects. And these changes have been farther accelerated by the fact, that the objects of our definitions have themselves, within certain limits, gradually become different from what they were; men and nations, for example, and their diseases, as well as the whole relations of their lives. And since, from this cause, from century to century, new phenomena and new objects have constantly been presented to our notice, their import, and our judgment and ideas of them must in like manner have undergone progressive change. Thus men and nations—at least in civilized countries—are, in their bodily and hygienic condition, as well as in their intellectual and moral nature, no more what they were in the middle ages, or even 100 years ago; and the same is true with regard to their disorders, such as leprosy, venereal disease, typhus, small-pox, and the like, and for this reason our present definitions are by no means applicable to those diseases as they formerly existed, neither will those of our predecessors apply to the diseases of our time.

However disturbing, if not melancholy, these external changes may be in many points of view, showing as they do the variable and unsatisfactory state of our knowledge, they are nevertheless encouraging as proofs of our progress. For, after all, our definitions vary and alter solely because the old ones no longer agree with the improved state of our knowledge. The more defective these old definitions are, and the more clearly we see their deficiencies, the sooner shall we feel ourselves impelled to correct them by farther investigation, and substitute better ones in their place. We must, however, as appears from the preceding, beware of falling into the illusion that we can give a perfectly correct and satisfactory definition of an object, process, &c., in our science, as regards either the healthy or the diseased body, before we have attained to thoroughly accurate and satisfactory ideas respecting it—that is to say, before we have become acquainted with its actual and peculiar laws.

Till then—therefore doubtless for centuries to come—we shall have to content ourselves, as we may, with instalments and approximate definitions, and shall not be entitled to believe that *our* definitions will have much greater permanence than those of our predecessors. And, even if it were so, the case would in many respects be so much the worse,—it would indeed afford a proof of intellectual laziness and impotence on the part of our successors. But as long as investigators and thinking heads are to be found in the field of nature and of life, we need never apprehend for our definitions so unlikely a fate as stability.

The very constitution of the mind of man and the whole course of his thinking and investigation cause him to direct his attention sometimes one way, sometimes another, and to occupy himself, in succeeding times and generations, continually with new objects, and with new aspects and portions of his great question. This direction varies also even with each individual inquirer, not only according to the speciality of his profession, objects, and position, but likewise according to his temperament, age, and experience.

It follows, therefore, as a necessary consequence, that new knowledge and new ideas of our objects, and especially of their laws and so-called essence, will continually arise—therewith, also, new definitions of those objects; and, in consequence of the intimate mutual relation between thought and language, new expressions for them. The meaning conveyed to us by these expressions is indeed continually changing, just as the word "love" has one meaning in the mouth of a young girl, another in that of a matron, and is understood in different ways by a child, a youth, and an old man. While one part of our knowledge rises, and the sum of our intellectual acquisitions on the whole increases, another portion rather perhaps sinks into oblivion; while one side, one direction becomes prominent for awhile and occupies the minds of inquirers, another retires into the back-ground. Thus, for example, in medicine, till recently, the old controversies between the humoralists and the solidists, and about juices, blood, and nervous system, were constantly shifting about, like Whigs and Tories in England; and in our own time, since investigations relating to the living body, both in health and in disease, have been directed chiefly to the chemical point of view, the anatomical and microscopical have excited less attention. As now the one and now the other portion of our variegated field of inquiry presses on the scale, the other scale, in which nothing new is laid, at the same time becomes lighter, and is neglected, till a revolution takes place in the entire direction of the intellect.

Other minds now, perhaps, become all at once aware of the one-sidedness, the extravagances of their time, and on the other hand, perhaps, perceive, more clearly than their contemporaries, the portion of truth contained in many older though forgotten facts and modes of representation. They become convinced that by following the path hitherto traversed, the whole, the nucleus of the great enigma can by no means be solved, and still less has the solution been actually obtained. They therefore dig and search the more in other directions, in other shafts and galleries, which have perhaps remained for years covered up and neglected, and, if successful, discover new answers to questions old as well as new. Moreover, in all such inquiries, the human mind generally proceeds in two opposite directions,—the one tending to the universal, to farreaching comprehension of resemblances, agreement, and inward relations of the greatest possible number of things, and to their collation and generalization,—the other more adapted to the comprehension of particulars, and therefore tending towards specialization, and chiefly occupied with predicating this particular, more limited matter by itself, in its own relations, and according to its own laws.

But as these simultaneous or alternating currents and counter-currents of our modes of thinking and investigation produce a continual oscillation of our understanding, ideas, and representations, they must necessarily be equally reflected in our expressions—that is to say, in the sense which we attach to them. But this sense is likewise subject to continual variation, even the meaning of our words being thus liable to be altogether lost and again recovered—as is the case, for example, with such expressions as nervous fever, gout, miasma, poison, oxidation, and the like—and, similarly, in common life with words like virtue, honour, religion, law, state, &c. And whereas in course of time certain expressions become used in a continually wider and wider sense, and increase in comprehensiveness (*e. g.*, salt, alkali, oxidation, inflammation, exudation); others, on the contrary, may become more limited in meaning and lose more and more of their original comprehensiveness. Thus, for example, the old term "consumption" has gradually been reduced to pulmonary tuberculosis; the old "fever" to nervous fever, typhus, &c.; the old "bark" to cinchona bark, &c.

For this reason, also, there have always been found persons who busy themselves, in spite of all difficulties, with continual attempts to give greater clearness and stability to our conceptions. In other words, their endeavour is to give more correct definitions of the several objects of our inquiries, perhaps to substitute, in place of confused, limited, if not erroneous ideas, their own more exact notions, and thereby at the same time to give to certain expressions which are undefined, ambiguous, and perhaps have become almost meaningless in daily use, a well-defined, exact sense, at least according to their own opinion.

So far as such attacks upon the deficiences or errors of our conceptions are limited to the field of science itself, and greater clearness is sought by means of positive investigation, and above all by conscientious personal labour, the attempt is doubtless praiseworthy enough. In fact, there can never be too much of such increase and illumination of our understanding by the continual addition of new matter, by new and more trustworthy facts and discoveries.

But such endeavours become faulty and even dangerous, as soon as they are directed solely or principally to the formation of more exact definitions for their own sake, or to a mere purification of our forms of expression, and a fixation of their particular meaning. For all such attempts at definition and explanation add nothing to our knowledge, do not make our ideas in the least degree more exact or more stable, and so far contribute nothing essential to the matter itself. Many an inquirer or "savant," who busies himself in this way, thinks, perhaps, that he can thereby add something positive to our knowledge, and possibly, also, under certain circumstances, defend his own views and pretensions better. But all his definitions and explanations, in themselves, bring us no enlightenment, unless they give greater width or steadiness to our view of the disputed matters. And inasmuch as such definitions and explanations are ultimately nothing but words, they can, as Bacon observes, call forth nothing but words, similar attempts at explanation, and further dissertations on the part of others, and the result in most cases is a mere war of words.

Moreover, no individual has a right to use our words and phrases according to his own pleasure, and to attach to them only that meaning which, rightly or wrongly, may appear to him to be the only correct one. Our medical language, like every other, is the property of all and of the science, not of any individual. Nearly every one of our technical terms has, in course of time, acquired a certain if not definite sense, and has been used, sometimes in one way, sometimes in another, by all members of the profession; and to these uses they adhere, and for the sake of their own understanding and the intelligibility of their communications to others, must adhere. And if any one thinks proper to use such an expression merely in his own particular sense, according to his own peculiar opinion, even though he may have the most valid reasons for so doing, he thereby attaches to it a much more definite—that is to say, a narrower and more confined sense, which no longer predicates and includes all that was formerly understood by that expression. He takes from it part of its significance, part of the sense which others are accustomed to attach to it. For these reasons, there is but little utility in attempting, by subsequent definition, to give greater clearness and fixity to the sense of long established, though perhaps defective words; but still greater confusion would certainly arise if individuals were at once to use old words in a new sense or to invent new words for old objects and ideas. For the words as well as the ideas exist, as it were, with a certain sense, with a certain signification in the minds of thousands, and, by such transformation, would lose more or less of their capability of serving as carriers or communicators of the existing amount of experience and knowledge. To an equal extent, also, does the use of certain modern technical words not only for the most part fail to add anything to the clearness of our insight, but rather clouds it, and introduces confusion into our ideas. Thus to speak of "Hyperæmia" instead of congestion, or of "Stasis" instead of inflammation, is to say something very different from that which was formerly understood by congestion and inflammation. The two by no means agree,—as little indeed as pulmonary tuberculosis agrees with consumption, Bright's kidney-disease or albuminuria with dropsy, abdominal typhus with nervous fever, &c.

The tendency to such attempts at explanation and purification, and even a certain justification of them, is certainly greater, in proportion as the usual forms of expression are more ambiguous and variable, more widely diffused and more altered and modified by use. In like manner it is greater, as our insight into the objects thereby denoted is less perfect, and the more our knowledge has changed in course of time, and is even now, perhaps, changing from year to year. And if these causes conspire in any science, they certainly do in our own. But, on the other hand, it follows from the same considerations that in a branch of knowledge so enigmatical as ours, in the midst of objects and processes so many-sided, ambiguous, and complicated as those with which medical science has to deal, no individual can be in a position to investigate, even a single question on all sides and in its whole extent. He will not readily succeed in determining any single process in the living body in all its conditions, effects, or laws, with sufficient certainty to enable him to understand it thoroughly, and therefore to define it with perfect precision, and in a manner which shall hold good for all time. He will never be able to show that his view of the matter is perfectly correct, or even superior to that of others antecedent to or contemporary with him. And even if he is convinced of this, he will, in the most favorable case, scarcely avoid the danger of taking a part or a side for the whole, and much that is subordinate and secondary for the principal part.

It is evident, also, from the preceding considerations, that great mischief will in most cases ensue if, in such attempts at definition and explanation, greater importance is attached to a clear and determinate, than to a sufficiently complete and comprehensive understanding of the objects and questions before us. For however useful and valuable the former may be for the understanding of the individual, its value is but doubtful and transient when considered with reference to the whole science and to all its cultivators. In a field like ours, clearness can in general be purchased only at the expense of completeness and therefore of truth. To an individual, a mode of representation, an idea or expression in this confined and narrower sense may be clear enough, and therefore of a certain value. But to the understanding of others and still more to the whole science, the gain thence accruing is but small, at least for a continuance.

Every one is therefore at liberty to follow out our questions and enigmas as much and as well as he can, and by his investigation and his newly-discovered facts to throw as much light on them as possible—the more the better. Moreover, this end can nowhere be attained more directly or more usefully for the science than precisely among those objects, the idea and name or denomination of which appear to the inquirer, as well as to others, doubtful, or unsuitable, if not entirely false. Here, even more than elsewhere, is there need of further elucidation and active research. But in the names and expressions themselves, provided they are to a certain extent endurable and derived from the mother-tongue, there should be as little as possible of attempts at alteration, subsequent explanation, or improvement,—less, indeed, in proportion as they have been in longer and more general use; for, after all, no one has the right to make such alterations, and still less, the power—that is to say, the requisite knowledge of things.

And since, for all the reasons above adduced, our language and forms of expression scarcely admit of any further fixation, explanation, or definition, without risk of greater danger and confusion than utility, it appears more advisable to give a clear, searching *history* of each of these expressions—*e. g.*, of fever, inflammation—of the names of the several disorders, and the like. In this way, our ideas of these matters might be most thoroughly fixed and elucidated, by showing how these expressions were first arrived at, how their original sense had altered in course of time, and why they must now be received in a sense different from that which was originally attached to them, and why in this sense and no other.

The above remarks leave little to be said concerning DESCRIPTION in our science, and the conditions which it must fulfil.

Like language itself, description is to us an indispensable medium for the representation and communication of all that cannot be otherwise presented, exhibited, and brought to the knowledge of others. Such, however, is the case with almost everything with which our science has to do. For we have to deal with phenomena, processes, and objects which, for the most part, cannot be directly perceived, much less laid hold of and exhibited; as, for example, is the case with anatomical preparations, animals, and plants, or the substances with which the chemist has to do. Description is therefore the only help that remains to us, and the best description must be that by which, according to an Arabic phrase, "the ear of the auditor (reader) is converted into an eye."

The requirements or conditions which we have to satisfy in our descriptions are simple enough. Every description should, in short, set forth and reproduce what we have observed, or, in general, the things to be described, just as they really exist. We must therefore know how to find for each of our phenomena, for everything that we observe in our processes and objects, the expression which is most exact and best adapted to it, so that others may receive therefrom the best and most complete explanation of which the matter admits. The images and conceptions to which they are led by our description should correspond exactly with the real state of the matter, as we ourselves have observed, and now wish to represent it. And this representation itself should convey to others only that sense, and excite in them only those ideas which we wish and ought to excite.

Every such description, therefore, presupposes, in addition to a correct observation and apprehension of our phenomena, &c., a suitable language and terminology, especially, also, the power of handling it in the most appropriate manner, so as to avoid all unmeaning or ambiguous and obscure expressions.

To satisfy all these requirements, however, is, as experience shows, a very difficult matter. That it should be so will not excite much surprise if we consider how, in every description, those difficulties of our language, which we have above endeavoured to depict, are raised to a much higher degree, because the difficulties of our observation in general, and the defects of the individual observer, together with all the dangers thence arising, tend to increase them. In short, if we require all that such a description in our science ought generally to furnish, and all that it is within our power to furnish, and if we examine more closely the peculiar circumstances and difficulties surrounding the objects to which our description applies, we shall readily understand why we are here opposed by greater obstacles and more formidable rocks than in any other department of nature.

For the same reason, it is clear enough why good descriptions are so rare, and good describers even much rarer than good observers. But it is rarest of all to find a man who is both at once, at least in an equal degree, because, in a certain sense, description and observation require totally different qualities and dispositions. If observation especially requires knowledge of facts, together with circumspection and a cool and unbiassed, as well as searching and comprehensive grasp of all the phenomena and circumstances, description, on the other hand, requires in addition, not only knowledge of language, and the requisite practice and facility in its use, but more especially the power of representing the observed phenomena in a sufficiently clear and, as it were, plastic manner, and yet with perfect accuracy. But to fulfil these conditions there must also be fancy, a certain almost poetical or pictorial talent, such as is rarely found in good observers, but rather appears to be at variance with the peculiar disposition required for correct observation. And if the description of the one appear too short and dry, if not obscure and difficult to understand, that of the other is apt to be too wordy, broad, often, indeed, inexact, and somewhat too fantastic or poetical. If the former, in a certain sense, confines himself somewhat too closely to the given details and objective view of the observed phenomena, the latter, on the other hand, often allows his subjectivity, his personality, to play too conspicuous a part.

In all cases, however, the truth and credibility of our description are most endangered by our ready disposition to express our own personal apprehension and combination of the thing observed, rather than to present it plainly and clearly, neither more nor less, nor different in any respect from that which it really is. Here, also, our nature and intellectual constitution lead us only too easily to give to our views and temporary theories greater scope than they deserve. In a certain sense, indeed, it may be asserted that such a thing as a pure and thoroughly objective description, without any admixture of our own subjectivity, is for the most part not even possible, just as our observations are never strictly confined to that which we perceive by our senses. We rather express by our description more than we have actually observed, and thereby present, not merely the objective, the pure matter of fact, but necessarily, also, something more—namely, our own interpretation and judgment of the matter. For good or evil, then, we must, in every attempt at description, no less than in our observations, express more than the thing actually observed—if only for this reason, that we cannot otherwise present it in a manner which shall be clear and intelligible to others, and convey to them a distinct impression. Our actual observation, indeed, is made only on certain individual phenomena, whereas in our description, if it is to be intelligible, these phenomena must, as it were, represent something, they must make their appearance grouped and combined, with a meaning previously attached to them by ourselves, and explained in one way or another.

Exactly, therefore, as (for reasons given under the head of "Observation") we never actually see a typhus patient, a typhoid ulcer, a scrofulous gland, or inflammation of the eye, &c.; so, likewise, are we unable to give of any such objects a purely objective and thoroughly matter-of-fact description; for in the very act of representing them and declaring them to be such (*e. g.*, typhoid, scrofulous, &c.) we say of them that they belong precisely to this species or series of phenomena, diseases, &c., and to no other. But, to be able to do this, we must previously have interpreted or judged them—that is to say, we must, by comparison with similar objects, have connected them with these, and so far reduced and classified them in a certain manner, and finally pronounced them to be of such and such a character. This, indeed, is the case to a certain extent, even when, in our description, we speak of such a thing as a "hot, dry skin," a "red or furred tongue," a "smooth or tumefied edge of a sore," a "rattling noise or percussion-tone in the chest," &c. For, in either case, we have already, even at the moment of observation, consciously or unconsciously, compared that which we actually perceive by our senses in these phenomena, states, &c., with others which resemble them, and recognized their similarity or complete identity therewith. And the discovery thus made, the result of that almost instinctive interpretation and comparison,—in short, our deductions from what our senses perceive and our conception of it,—must likewise be transferred to our description. This description, if it is to be intelligible, must therefore contain these results of deduction and comparison in addition to that which is actually observed.

Every description, then, even the best and most unadorned, contains a sufficient quantity of our own additions, of our own subjectivity; and only in that way can it be rendered appropriate and intelligible. To avoid, indeed, even to a limited extent, this interference and addition on our own part, we should be obliged to make an enormous circuit in endeavouring to describe each individual phenomenon, and all the properties and constitutions of our objects, each by itself. The result would be a description which no one could listen to, read, or understand. It must be remembered, however, that this unavoidable addition, this subjective element, may play much too conspicuous a part in our description, and easily obtain the preponderance over its objectivity, as well as in the observation itself. To us, moreover, this danger is much more imminent than to others, because we can observe directly only a very small part of our objects and phenomena; and therefore, also, are seldom or never able to describe simply that which we have actually seen and observed.

From this, and all that has been previously adduced, we may readily see how it is that descriptions are, on the whole, so seldom perfectly correct and admissible; and therefore, also, seldom really useful or appropriate, and nowhere more rarely than in our science. This, also, for reasons already given when speaking of language, will in general, under otherwise similar circumstances, be still less the case as the description itself—*i. e.*, its date, is more remote from our time. Apprehension and ideas, as well as the modes of expressing them, were in former times more or less different from what they are now; and moreover, in the descriptions of our predecessors, the subjective element predominated in a much greater degree than in later times; so that, in fact, they often gave views, arbitrary assumptions, and theories, rather than actual observations. The first-mentioned circumstance was, in itself, quite sufficient to render their descriptions for the most part doubtful, if not unintelligible, and but too frequently adapted to excite incorrect images in us, rather than such as are really accurate and true to nature.

In spite of all this, however, there has never been a dearth of good describers or of good observers, and from the communications of a Hippocrates and a Celsus, as well as of a Sydenham, Boerhaave, Van Swieten, Stoll, and others, we may, within certain limits, even at the present day, extract a sufficient amount of instructive matter. In fact, even one-sided and fragmentary descriptions, whether of ancient or of modern date, are often of great value, if only they contain something that is true and in accordance with fact. For in truth such descriptions are often our only sources of information respecting many new, rare, or important matters. And if the information thus obtained respecting these matters is but approximate, at all events our attention is directed to them; their description renders them a possession of the science, and the gaps may be afterwards filled up by further observation and description.

But we ourselves shall, in general, be able to avail ourselves of such and of all foreign descriptions with greater security and better effect upon our understanding and our independent investigations, the more we have previously learnt and understood concerning the things described. For we cannot otherwise be in a position to judge whether and how far a description is accurate and complete, or, on the other hand, faulty, or perhaps utterly false, and therefore to be regarded as worthless, or at least as improbable and doubtful. Hence we may plainly see the absolute necessity of a certain scientific cultivation and insight, of a certain knowledge of facts and ripeness of judgment, not only for our own observations, but likewise for making use of the experience and observations of others as they are communicated to us. It is only in this way that we can successfully avoid the numerous deceptions and errors to which others may expose us by the defective or erroneous character of their descriptions. On the other hand, the smaller the amount of cultivation and knowledge that a man possesses, the more readily will he believe all he hears, even though it may be utterly unworthy of belief; or, on the other hand, he will doubt, and perhaps altogether reject what he ought to believe. The one or the other he will do without valid grounds, and without clear and sound judgment, because without knowledge and experience; and whether he believes or doubts, he is always in danger of doing so in the wrong place. But inasmuch as such sufficient knowledge and judgment is to us, from the almost entire want of sound understanding of our objects and questions, nearly impossible, or at least is rarely found, we may understand why even educated physicians, physiologists, and others, are often beset by the dangers just mentioned. Examples of this are to be found in abundance both in ancient and modern times, and nowhere more frequently than in the field of our ætiology of diseases, and in therapeutics properly so-called, especially when bias and interest, or prejudice, party views, and similar personal motives likewise come into play.

Finally, there must be added a circumstance which does not always receive the attention it deserves. Every foreign representation or description may make very unequal impressions upon others, according to their several experiences, ideas, and tendencies, or according to their temperament, intellectual constitution, and education. Descriptions, no less than any other sentient impressions, or the *vox viva* of an orator, may perhaps awaken in every one, even in a cultivated, impartial, and circumspect person, different conceptions and associations of ideas, according as they agree with his mode of conception and judgment, his previous views, his habits, wishes, and aims; just as, for example, the description of a pretty girl may produce different impressions upon a youth and an old man, or that of a "beautiful" case of disease on the physician and a non-medical person, or the representation of a battle on a soldier and on a woman.

If, for example, a person has conceived this or that notion of an object or a question, or if he is already engaged in its investigation, he will have quite a different and a sharper eye for all that relates to it, for all even half-concordant descriptions given by others, than one who is interested not in this particular point so much as in another, perhaps even in the very opposite; while the former perceives even the remotest and obscurest analogies, the most ambiguous allusions and hints of others, because he can make use of them—the other, perhaps, receives no impression from them, and consequently overlooks them.

In this, however, both may go wrong, and make an unjustifiable use of the description of another. And precisely for this reason is the circumstance just noticed worthy of the attention of every investigator or physician, and especially of the younger and less experienced.

IV. CLASSIFICATION.

By an almost instinctive impulse, similar to that which leads to the use of language, we are induced to collate or group together the things which we observe—that is to say, to classify them. Our intellectual nature and requirements compel us, in fact, not merely to observe the objects and phenomena with which we have to do, and designate them by this or that name, but also to imagine them combined or grouped in a certain order—at least if we wish to take a complete survey of them. Accordingly, every science and art has endeavoured to classify as completely as possible the things belonging to it; hence, in our field of inquiry, the objects classified are the phenomena and processes of the living body, diseases, remedies, the hundred influences and agencies of external nature, &c.

Even when we observe any object or phenomenon, and certain properties belonging to it, and declare it with our words to be this or that (*e. g.*, red or hard, a cutaneous or pulmonary inflammation), we have already compared it with all objects, phenomena, &c., of the same kind that are known to us, and thereby recognized it as of such and such a kind—that is to say, we have already, in a certain sense, grouped or classified it. But in this case it is only, as it were, an accidental or unconscious, or at all events an unpremeditated classification that we make; whereas, in classification properly so-called, the objects and phenomena are, with a clear understanding of the matter in hand, united into groups and classes, according to their more essential properties, or reduced to a certain system.

The grand problem, the true end of our classification, therefore, is by no means to arrange objects and phenomena in such groups, well or ill, in any way whatever, but rather, by this assemblage of certain things in one and the same class, to place ourselves in a condition to regard the objects thus united as actually belonging to one another, and therefore as essentially of the same kind, or at least as nearly related and analogous. That which we, by our classification, declare to be so—*i. e.*, that which, by being placed together in one and the same class, is presented to us as a more or less homogeneous, and essentially concordant assemblage of objects, might likewise be so in reality; for herein consists altogether the benefit which classification can and ought to confer upon our understanding.

It is a consequence of our very nature that we cannot observe anything—in the living body, for example, or in a patient—and cannot pursue our reflections upon any observed phenomenon, without a whole train of other phenomena and objects occurring to us, which, from previous observation, appear to be analogous or essentially similar to the one in question. In order, therefore, that our judgment respecting the latter may be facilitated by this comparison, it is essential that the objects thus presenting themselves be really similar or identical with the one under examination. Equally dependent upon this condition, also, is the result of our reflection on the matter, and of our inquiry into its conditions and its causal connection, for a correct comparison of objects constitutes the first step of our whole inductive process. This presupposes that all the series of individual objects comprised within our province are already present to our consciousness, slumbering therein, as it were, in due order and connection. For we cannot well form a distinct conception of any one of those objects or of any individual case (of a disease, for example), unless the other objects and cases belonging to the same class are also present to our minds. Neither can we form a correct notion of any separate series or group of such objects (*e. g.*, phenomena or things of the same species, genus, or class) without comparing it with the other groups or series, and therefore without having these others present to our consciousness, to our ideal world. And it is only when our conceptions on these points are as accurate as possible—that is to say, when those groups are formed of things really similar and connected together—that we obtain a clear and correct survey of all the individual objects which together make up the contents of our province (*e. g.*, physiology, nosology, or therapeutics). Far more important is it, however, that we be thereby, as it were, placed in a position to apprehend and judge more correctly that which we observe in any particular case: for example, in any process taking place in the living body, or in any action on the same in a patient; for we can then assign to this case, or rather to that which we observe in it, its proper place in the entire mass of objects and phenomena; inasmuch as we collate it with precisely that group or series of phenomena which are of the *same* kind, which agree with it in such and such principal points.

This may suffice to show that every classification is of the utmost significance to our understanding, to our entire method of comprehending and judging every individual case that falls under our notice, and likewise to our further inquiry into its causal connection and laws. And as this end is essentially facilitated by a good and rational classification, so, on the other hand, may it be impeded by defects and errors therein. To obtain, then, all these important advantages, it is plainly necessary that every classification be of such a kind—that is to say, that the objects in question (*e. g.*, vital processes, diseases, remedies) be arranged in such groups or series and classes,—or generally in such order,—that we may be thereby immediately reminded of their more important or essential properties, of their peculiar nature and relations to others. For, by such a classification, not only would our ideas and views respecting the individual objects thus connected be placed in order, but in a *correct* order,—in an order adapted to assist as far as possible our judgment and understanding of those objects. Moreover, it is self-evident that this desideratum is of two-fold utility in our province, and especially for the theory of diseases and remedies,—for our so-called ætiology and hygiène. In this even more than in other fields of inquiry, we have to deal with the most varied and complicated mixture of phenomena, processes, or influences; and at each step, at each individual observation (of a patient, for example), all possible analogies and comparisons with various others come into our minds, and with these we perhaps unconsciously connect the new case. How much, therefore, must our judgment, our understanding of this latter be assisted, if at this time we are reminded precisely of those objects which actually agree with those before us in all essential points, and the classification, therefore, supports us in this first important step! The chemist derives this assistance from the grouping of his substances—*e. g.*, into alkalies, acids, or into organic and inorganic substances—the physicist from the grouping of his phenomena into those of electricity, heat, gravitation, light, &c. And in a still higher degree is this true of the descriptive natural sciences, in which the union of certain sets of phenomena or characters, perceptible by the senses, and established once for all, serves for the formation of genera, groups, and classes, and the division of all animals, for example, into the great sections of vertebrate and invertebrate, or of vegetables into monocotyledons and dicotyledons, of vascular and cellular plants, &c.

It is then the especial business of each branch of science to classify the things belonging to it (*e. g.*, the vital processes and influences, all diseases, remedies, &c.), according to their essential or distinguishing qualities and characters, and, moreover, according to those particular characters which are of the greatest importance with reference to this branch of science and its points of view or interests. This arrangement and our classification-titles should also be such as to induce and enable us to think exactly of those things which really agree, in certain decisive and important points, with that which we observe or would speak about in the case before us, or, at least, resemble it as closely as possible;—such allied or similar objects should alone, therefore, be included in the same class or series. It is true that we can, in a certain sense, select as the basis of our classification any aggregate of properties, and ultimately even any single property which we observe in the objects under examination. And so far, our power of forming groups and classes is almost unlimited—that is to say, the number of them that we can form is equal to that of the properties which we observe in our objects and phenomena, belonging to one and not to the others, so long, therefore, as we discover differences, which may serve as the basis of their division or classification into so many species, genera, groups, or classes. It appears, however, from the preceding considerations, that those several properties and differences are far from being equally advantageous for our classification, and that consequently our main object—the understanding and clear survey of all the objects under examination—cannot possibly be promoted in an equal degree by them all.

Least of all is this the case in the older attempts at classification, and in our department of science as little as in any. For here especially it follows from the nature of the matter, that in the beginning, in the infancy of science, those objects or combinations of phenomena can only be distinguished and grouped according to those particular properties, circumstances, &c., which present themselves at once to the ordinary observer, and can generally be recognized with but little trouble. We find this, for instance, in the older classifications of plants and animals just as well as in those of diseases, remedies, &c. In both cases, the existing knowledge of all the objects which it was desirable and necessary to classify, was much too small to admit of any other method of classification. Well or ill, therefore, one and the same group was then made to include phenomena, influences, and events (*e. g.*, diseases and remedies), which resembled one another only in comparatively external and subordinate, perhaps altogether accidental, points and circumstances. At all events, the characteristic and more important properties or circumstances were then far from being sufficiently established, and the objects included in the same class were, for the most part, far too little known to render it possible to predicate anything of a general character respecting them with truth and certainty. Hence, also, it is evident that such a classification could by no means promote the knowledge of the things classified, but, on the contrary, must often have impeded and disturbed it, and consequently that the above-mentioned conditions of a good classification were far from being fulfilled thereby.

To attain this end as completely as possible, we must obviously rather form classes or groups respecting which we know and can predicate the greatest number of general properties—that is to say, the greatest number common to all the united objects, and, as it were, available for them all, and at the same time the most important, or determining and essential. We must, therefore, be in a position not only to indicate the points and circumstances in which, especially, the assembled objects agree with one another, at all events more than those included in other classes, but these points and circumstances must be as numerous as possible, and, more especially, as important and essential as possible. In so far, then, as our peculiar and principal object may be regarded as the comprehension of our phenomena and processes, and therefore the insight into their causal connection or laws, we must, as far as possible, know how to select those moments and circumstances which especially relate thereto. Thus, instead of classifying diseases according to variable and accidental symptoms, or the so-called locally attacked structures and anatomical lesions, &c., we should rather base our classification on their necessary conditions, and on the manner of their origin, progress, and result; in like manner, with respect to the agencies of the external world and to remedies, we should look to the conditions and laws of their action in general; and again, in the living body, and especially in man, to the properties on which those actions depend, &c. In making such classification, also, the greatest stress must be laid on those properties and attributes of the things to be classified, on those circumstances of their origin or action on which the others more or less depend, or, at least, are only their external manifestation (as, for example, the so-called morbid or remedial symptoms are the manifestations of the process going on within, of the continual changes and conditions of the human body). Those properties or circumstances which determine and serve as measures of a number of others are generally called, for shortness, the *essential* properties.

In favorable cases we might then succeed in uniting into *one* group or class exactly those series of phenomena, events, or objects, whose common and general examination would, on the other hand, throw the greatest light on each individual among them, and would necessarily lead us to the observation of its most important relations—*i. e.*, of its conditions and laws, and the manner of its origin, progress, or action. So far, also, a classification of this kind would be best adapted, not only to afford us a certain insight into the whole of these groups, and therefore into all the objects of research in our science, with respect to their essential points, but also to promote and facilitate the investigation. For this very classification would support us in our judgment and understanding of the individual case, and would likewise point out the roads which would soonest lead to a further investigation and a better understanding of our processes.

In short, a division according to causal connection and the internal relations and laws of things, is, perhaps, the best adapted to the further progress, at least, of all those sciences and branches of knowledge which are not merely occupied with the observation, description, and enumeration of their objects, but likewise aim at a thorough insight into them. But, on the other hand, it is evident that in our science we are far from being in a position to carry out such a classification with certainty. A classification, according to those considerations, presupposes a comparative knowledge of vital processes, diseases, and their laws, and of all external influences and remedies, and of their effects, which we are at present, and shall be for some time to come, very far from possessing.

For good or evil, therefore, we confine our attention to certain external phenomena, properties, or effects, which we find most easy to apprehend, and use them as our principle of classification. If, now, by such a method, objects are grouped according to the whole of their resemblances or differences in external appearance, form, progress, &c.; the classifications and systems thus formed are called NATURAL. On the other hand, they are called ARTIFICIAL when only one or a limited number of properties and characters is taken as the basis of the distribution (as, for example, in Linnæus's system of the organs of fructification), and more or less arbitrarily, accordingly as one or another character is regarded as the most important and convenient.

From another point of view, also, all attempts at classification in general, and in our science in particular, may be divided into those which rest merely on known and given EXTERNAL indications (*e.g.*, on phenomena or symptoms of diseases, on the manner of their progress, or on the form, composition, and similar properties of our medicaments), and those on the other hand, in which rather the INTERNAL—that is to say, the laws and mode of origin or action, in short, the more essential characters, are adopted as the principle of the distribution. The former are certainly, at present, the POSSIBLE, or, at all events, the easiest; but unfortunately they are of little use excepting in the purely descriptive natural sciences—such as botany, zoology, &c.—which aim, not at a thorough comprehension, but merely at the external characters—such as the form and structure of the objects with which they are concerned. The others, on the contrary, are, for the present, IMPOSSIBLE; but it should be our constant endeavour, for all the reasons above adduced, to render them possible and available.

Every classification, however, presupposes, in the first place, a fixation and limitation of the individual objects which are to be classified. For every class or group comprehends a certain number of such individual things and unities, which, with reference to this their class or series, are called SPECIES. The first step towards their classification must therefore be a certain knowledge of them. Before we can think of rightly comparing or distinguishing these species or elements—that is to say, of classifying them—we must make ourselves acquainted with them, each by itself, in all their more important properties and relations. Now all the sciences and branches of inquiry which have been more successful in this respect than ours, have fortunately had to do with well-defined objects or phenomena which present themselves to the observer as something complete each in itself, well defined and isolated, or at least admitting of being circumscribed and isolated with facility. Thus every species, every element with which the chemist has to do, whether simple or complex, has a certain number of peculiar properties; each individual is a kind of unit, with its peculiar mode of manifestation or of action, of combining with others and being altered by them. The botanist, zoologist, or comparative anatomist, when he proceeds to classify the objects of his research, finds them already presented to him by nature as distinct and circumscribed unities. They are, in fact, individuals with determinate properties or characters, which, moreover, are produced according to types peculiar to themselves, and as such, develop, reproduce themselves, and pass away. He has merely to observe and compare them, and make to himself an abstraction of the type of each species or genus, and he is immediately in a position to arrange them in groups and classes according to the characters which they possess in common.

But it is quite otherwise in our field of research, inasmuch as neither in the processes and functions of the healthy body, nor in its diseases, can there be any approach to the formation of species and genera in a similar sense. The physician in particular has to deal with events,—*i. e.*, with changes in the so-called normal or physiological existence and state of our body, which intersect each other, modify, develop, and unite themselves in a variety of ways, and this in a different manner in almost every case. All his species and genera of diseases, so far, indeed, as they can be said to exist, are therefore nothing but ideas or pictures deduced and constructed by himself. They are more or less arbitrary abstractions formed by our intellect from that which we have observed in individual cases, in order to bring their unlimited variety into something like order, to obtain a more convenient survey of the objects actually before us, and, lastly, to be able to predicate certain common properties respecting them.

It is true that there are states of disease in which a certain kind of unity may be supposed to exist, particularly in acute and so-called specific maladies, such as scarlet fever, small-pox, syphilis, cholera, and intermittent fever; and in a great number of skin diseases forms are presented to the eye which might perhaps lead to the assumption of peculiar species or genera, although neither in this nor in the other case do such species and genera, in the strict sense of the words, exist. Be this as it may, it has led to the mistake of classifying in a similar manner diseases of a totally different character, without the determinate ensemble of phenomena and processes exhibited by the former, and without the same definite origin, progress, &c. Nosology now includes not only such disorders as inflammation of the lungs and typhus, but likewise dyspepsia, gout, scrofula, epilepsy, and the like, as units in its classification, regarding each of them, therefore, as a uniform essentially identical essence, whereas, in reality, almost every particular case *of the same species* differs from the rest. To the abstract idea of the disease, then, there is no individual disease actually corresponding; and so far the supposed nosological unity has and can have no existence, because it relates, not to any essentially uniform essence, but to mere phenomena and their accidental and varying forms. But these phenomena themselves are continually changing, because they originate merely as effects or results, as secondary products, as it were, from other and more deeply seated processes. For similar reasons, also, every attempt at classifying diseases according to their so-called "seat" in this or that organ, &c., resting, therefore, on an anatomical basis, or according to certain changes of composition, and, therefore, based on chemical grounds, cannot lead to a much better result. For, independently of the consideration that separate elements or fragments of the disorder are thereby erroneously regarded as the whole, these anatomical or chemical species and units of disease are not in themselves more reliable, and generally possess no higher value than the old symptomatic species. For these material alterations, "lesions," &c., as well as other phenomena or symptoms, are, after all, merely results or members of the entire process of disease, and, under certain circumstances, a portion of its necessary conditions. It is often said that such and such diseases had at least the same seat in an organ or texture in other cases, and that this is a decisive characteristic circumstance. But, in reality, we cannot speak of the seat of a disease; we may perhaps speak of the seat of a so-called morbid product, of an alteration of texture, after-formation, and the like, but not of the seat of a process which travels to different parts of the living body. We cannot even make use of the alterations of individual functions or processes in diseased persons as our ground of classification, because we rarely, if ever, know which of them are first, or chiefly disturbed, still less in what manner and why. In so far, then, as, in these anatomical and chemical changes, their causal connection and relations to other changes or states in the same patient, and to the whole of his disease, are far from being satisfactorily known, our scientific knowledge of them, and therefore their applicability to classification, are very slight. For, so long as we are more or less unacquainted with the essential character, that is to say, with the causes and connection of these changes, we shall be constantly in danger of regarding comparatively subordinate, dependent, or accidental disturbances, as characteristic and essential, and consequently of placing together diseases which differ in essential characters, and, on the other hand, of separating others which are essentially similar or related. Exactly the same is true with respect to the action of the external world, and of our remedies, on the human body.

Moreover, as already observed, a comparison of all the separate objects—*i. e.*, of our so-called species and unities one with another, is indispensable, when we wish to combine or separate them according to their resemblances or differences; that is, to classify them. We require, therefore, for this comparison a certain type or model, a single species (of disease, for example), which, by exhibiting the impression of all the more important properties of this series of objects, is peculiarly adapted to serve as a type of comparison with others of the same species and series. Such types readily present themselves to the zoologist, comparative anatomist, botanist, &c., but not to us; because, for example, the several vital processes, diseases, and actions of remedies, so far as we know them, are too variable, too dissimilar in kind, and altogether more or less unknown in their more essential points. Finally, in classification, no single group or class can be rightly formed or circumscribed by itself, but must rather be viewed in relation to all the rest—*i. e.*, as a member of the whole collection of groups and classes. We ought, therefore, for reasons above detailed, always to be in a position to compare the things assembled in one group with those of all other groups. But up to the present time we are not acquainted with all the diseases to which man is subject, even so far as is possible by ordinary observation; of his diseases over four-fifths of the earth's surface we know very little, and still less of those of the lower animals, and of plants.

On the whole, then, it appears that it would be equally vain and unsuitable to endeavour to proceed in our classification on the same principles as in the descriptive natural sciences, or in chemistry. Nevertheless, such attempts were formerly made, and are even repeated at the present day, perhaps because the completeness and excellence of the classification in those sciences entice us to make them. We suffer ourselves, in fact, to be misled by this false analogy, so far as not only to discover and make out the existence of species and genera of disease, but even to augment the number of such species and genera as much as possible—and merely because this division has its value in botany, zoology, and chemistry, though in our science it has no value whatever. It is just as if a meteorologist should endeavour to divide into so many hundred species, and further into actual groups or classes, all his phenomena and states of the weather, which are continually varying and passing one into the other, according to season, situation, and climate, and even on the same day! In making these attempts, it has been forgotten that the final aim of our classification is by no means comparable with that of the purely descriptive natural sciences, because the chief interest of the physiologist and nosologist is directed to the understanding of the vital processes and their laws and internal relations, and not, as in the sciences just mentioned, to a mere acquaintance with external appearances, form, &c. It has been forgotten that our end, as well as our fame, can only be promoted by searching for the laws and relations just mentioned, and thus, by the discovery of the causal connection and more essential characters of our diseases, medicinal actions, &c. continually reducing the number of those species and genera, rather than seeking to establish new and more extended ones. And if we are not at present in a position to assist our understanding and insight by our classification, in the manner already indicated, that classification at least should not render the matter more difficult, or introduce additional confusion into our notions, already so doubtful and indistinct, respecting those diseases, &c. All our attempts at classification, however, ultimately agree, as experience shows, in this respect only, that they either arrange diseases, remedies, &c., according to phenomena, or properties, and circumstances, which for our purpose are destitute of positive value; or that they represent properties, causes, and effects as actually present, which are far from being demonstrated; in some instances, not even investigated or probable, but are rather hypothetically assumed, and then used for the purposes of classification.

It is true that the latter state of things cannot be avoided, if we wish to form such a classification according to hitherto received principles, and in the ordinary manner. For, in fact, our knowledge of all vital processes, diseases, remedial actions, &c., is far from being sufficient to enable us to judge in what principal points many of these agree, and in what they differ; and for this reason, all the higher or universal, and at the same time more reliable, principles are wanting to our classification. In other words, we are not at present, and probably shall not be for some time to come, in a position to form large groups or classes with certainty, and in accordance with the actual state of things, or to predicate anything that can rightly be considered as applicable to them all. And so long as we are unable to do this, there is nothing left but to form SMALL GROUPS of those objects (*e. g.*, diseases and remedies), which agree in all their more important properties, and in the more essential points and circumstances, so far as they are known to us. But our view of these more essential and characteristic properties changes with the progress of our knowledge, and with each period of its development, and to an equal extent, as already observed, according to the particular department of inquiry in view, and its peculiar interests. For example, those properties of a substance or of a remedy, which perhaps appear the most important to the chemist and naturalist, are not so to the therapeutist and physician. The latter, in his professional capacity, is interested in those qualities only by virtue of which substances act upon and do good to a patient; and a classification according to these effects —*i. e.*, according to the changes induced by those agents and substances in the state of his patients, would be the best adapted to his wants. The hygeist and toxicologist, on the contrary, would attach the greatest importance to those properties and modes of action of the same influences or substances by virtue of which they keep man in a healthy state, or are capable of hurting him. In the study of diseases themselves, the physician will regard, as the most important, those aspects and circumstances which relate to their causal connection, and to the manner of their origin, progress, and cure.

Hence, also, it appears that anatomical changes can have but little value as a principle of classification of diseases, because, in any division founded thereon, little or no attention would be paid to the very points which to the physician are the most important, because in fact, the classification-titles could not teach or suggest anything respecting those points. Is it indeed to be expected, when we place together the diseases of the same organ—(*e. g.*, of the brain or stomach, commencing with simple irritation, congestion, inflammation, and proceeding to extravasation of blood, softening, cancer, &c.), that much that is common and applicable to them all can be predicated respecting them? The same remarks may be applied to attempts at classifying diseases according to purely physiological or chemical principles, because the nosologist, if he would be true to his own interests, must not look to foreign sources for the principles and points of view of his classification. The scientific problem of the physician, as such, is solely, as already observed, to attain to a knowledge of his morbid processes, remedial actions, and their laws and causal connection; and for this reason, the phenomena and processes in patients which bear directly thereupon, are alone adapted to form the basis of his classification.

In such a classification, therefore, we must place together those diseases, remedies, &c., which exhibit such phenomena, processes, or states in common—that is to say, those which are to us the most essential and characteristic. We must therefore endeavour to discover those which probably possess these characters, and seek in the long list of our diseases, remedies, &c., for those which most nearly agree with each other in these respects. We must, in the first place, examine the cases and circumstances in which these phenomena, effects, or processes, stand out in the strongest relief, exhibit the highest development, are least complicated by the presence of others, and therefore present themselves to our notice with the greatest clearness. Supposing, for example, that we had recognized the increase of bodily temperature as such a characteristic phenomenon of a series of morbid states, its relation, origin, and progress, &c., would perhaps in the next place have to be more precisely established by observations on patients suffering from fever and inflammation, because in such patients the observations would be most easily made; and those diseases in which this (together with other characteristic phenomena) exhibited itself most strongly and consistently, would then be placed together in a group. Thus much having been established, we must then look for the same phenomenon or element of disease in its less distinct and prominent development, under circumstances and in patients in whom it exhibits itself in a lower degree, is more complicated, masked and impeded by others, and perhaps has undergone certain modifications. And if, in course of time, we succeed in discovering the laws, the determining conditions of this series of phenomena, and in showing that the same process or circumstance is characteristic of a whole series of diseases, we shall then have made a nearer approximation to another object of our science—the progressive generalization or reduction of our processes of disease. We shall then be more and more in a position to refer differences in them, which are apparently qualitative, and regarded as essential, to mere quantitative differences. Lastly, it is self-evident that neither in such cases, nor anywhere in nature, can we safely use *any single character*—*e. g.*, any single phenomenon or process—as a ground of division in the formation of our groups, but must rather avail ourselves of the greatest possible number of characteristic and essential circumstances —that is to say, if we wish our groups to be natural, in accordance with the actual state of things, and at the same time instructive.

In the present state of our knowledge, these small groups will at first, as above observed, include only a few diseases, remedies, &c.: it is possible, even, that a group may in some cases be made up of not more than a single disease. At all events, we should hear no more of such classes as febrile and inflammatory diseases, neuroses, dyscrasias, and the like, or of a division of maladies into acute and chronic. It would rather be necessary at first to divide such classes into small groups, and put together typhus, plague, and cholera, dysentery, acute exanthematous and syphilitic diseases, &c., in their several forms, each in a group by itself. And in the theory of remedies, instead of forming classes, such as organic and inorganic substances, or diuretic, exciting, tonic, and astringent medicines, resolvents, alteratives, &c., we should rather, perhaps, form groups containing only mineral and vegetable acids, alkalies and their salts, certain metals, narcotic substances, &c. On the other hand, the farther our knowledge extends in various directions, the sooner shall we be able gradually to enlarge these groups, or cautiously to unite with them other groups agreeing with them in certain essential points, and thereby to form more general and comprehensive groups or higher classes. Moreover, the formation of these classes would be further promoted by previously arranging these small individual groups amongst themselves in our system, according to their degree of relationship or analogy. We must therefore seek to arrange them in such a manner, that those groups which agree with or resemble each other in the most numerous and important properties, processes, and circumstances, may be placed nearest to each other; and those which present but few and subordinate points of resemblance, the farthest. This mode of classification in natural groups, and according to the degree of relationship or agreement, has perhaps been carried to the highest pitch of technical perfection in comparative anatomy and physiology. The mode of classification adopted in these sciences exhibits to us, thus far, the most complete example for our own, and we shall therefore do well to proceed in our field of inquiry according to their principles, but self-reliant, and with due regard *to our own interests and points of view.*

It is not, however, the province of our Logic to enter further into the mode of carrying out such a classification, or into its technical details. Its task is sufficiently accomplished in setting forth the universal principles of classification, and the conditions which every classification must fulfil if it would satisfy the requirements of our science, and therefere, also, of its Logic.

SECTION VII.

ON FALLACIES, THEIR DIFFERENT SPECIES, SOURCES, AND IMPORTANCE.

Experience shows us, that errors and false conclusions are of constant occurrence in our department of science, and, indeed, are more particularly frequent in medicine, strictly speaking, than perhaps in any other sphere of human knowledge and investigation. We are hence led to infer that some circumstances must exercise so preponderating an influence as to cause us to observe and interpret the same thing in many different ways, whence deductions are drawn opposed to truth and fact. And, again, many things are by this means made to appear correct and proved without such being really the case. Upon these chances of error, offering themselves at every step of an investigation, we have sufficiently dilated on a former occasion. That we should be constantly misled in observing and attempting to explain phenomena and agencies, whose internal conformity and order have as yet been so insufficiently defined, can by no means be regarded as wonderful. One may comprehend how even the most gifted and highly talented men have been induced, under such circumstances, and excusably so, to examine attentively some very complicated matter,—perhaps even to attempt to unravel and explain it,—before they have learnt to understand its separate portions, or are in a position to answer with certainty even the most simple question concerning it. As in every branch of human knowledge where there is a want of solid foundations and of fixed principles, so in ours, only too much depends upon the peculiar views and conceptions, upon the tact and, so to say, instinct of the individual; and since every one is free and unrestrained in his faith, doubt, or rejection,—in short, perfectly uncontrolled in his whole proceedings,—he is the more prone to error in this particular branch than in any other. However precious, therefore, liberty may be in other matters, so as "to make up half the value of man's existence," as Homer expresses it, yet in science it is the reverse. Being then so circumstanced that we are all liable to error, and doubtless have already erred scores of times, the necessity of first learning thoroughly to understand and carefully to watch over ourselves,—that is, our mental processes,—the creation and addition of our *ego* in connection with our every observation and experiment, and more particularly with our conclusions, is obvious. But since this is the peculiar province of logic, we shall enter into its consideration with more minuteness. It would be of small avail to dismiss the subject with some commonplace phrases or trivial advice, as for instance, that one ought to guard against all error, that one ought to observe correctly and prove everything, and that experiments should result only in such conclusions as really follow, free from all prejudice, and so forth. How much more useful to prove in detail that everybody, even the most acute and sober-minded among us, may be deceived, at the same time stating the causes and reasons! Thus will he not only be convinced that he himself, together with others his predecessors and contemporaries, is exposed to the danger of error at every step, but he will the sooner learn how that danger is to be avoided. Although, from a knowledge of the rocks which threaten him, his own feeling of security might be diminished, still he would derive an advantage in estimating his own achievements and the faults of others with due modesty and precaution.

Instead of enumerating the various errors, and the apparent show of reason by which they are supported, it will be far more important for us thoroughly to examine into the cicumstances by which we are misled, and how we are in consequence erroneously induced to consider observations to be correct, facts to be proved, and deductions to be true. Without some foundation in reason, no one could conceive even the most erroneous views, neither could he have drawn his conclusions, however false; his error must have lain in his estimation of the convincing power of those reasons, and of the reliance to be placed upon certain experiences and assumed facts. But even if ready access to the comprehension of our subject be denied, we ought, at all events, to possess a sufficient knowledge of it to prevent our believing that we understand a thing when we really do not; we should, at least, have made such progress as not to accept a doubtful thing as a decided fact, or downright error as truth. And since it appeared that physicians were even less upon their guard against this delusion than investigators in other departments of science, especially in ætiological and therapeutical questions, an explanation of the circumstances likely to lead to errors on either side was deemed the more indispensable.

In short, though the chapter of fallacies—the pathological part of our logic, as it were—be bitter enough in many respects, it is nevertheless a very useful one. Even correct observations and accurate modes of investigation will be benefitted by this knowledge of their reverse, just as the laws of the formation of tissues, and the functions of the body in a state of health, are better understood by observing their deviations in disease. Thus, had our faults and errors been applied to their proper use, they would frequently have shown themselves to be the first defective edition of the truth, or at least its preface. For alas! it is only in the old myth that Minerva bounds at once out of the head of Jupiter, whilst in reality severe and protracted labour-pains are generally required. Error, itself, however, has in nearly every instance paved the way to a better understanding: "Ex errore citiús emergit veritas quam ex confusione," says Bacon.

It is probable that in every case it is through our judgment that, in the end, we err. Thus something appears to us as real or proved, at least as very probable, without being either the one or the other; or, on the other hand, we doubt or wholly reject correct and real facts, or perhaps consider a thing as erroneous which, nevertheless, is in perfect conformity to truth. The sources of these errors of judgment, the causes and circumstances which have induced them, are most numerous and complicated, sometimes being only of a remotely disposing, at others more nearly of a determining character. On a former occasion mention was made of these, in so far as they depend upon the special nature of our department—that is, upon the peculiar difficulties which, for instance, the external world, as well as vital phenomena, diseases, &c., oppose to our observation and interpretation, and to the formation of correct opinions. These may even be looked upon as comparatively extrinsic circumsances, and are essentially always the same; they are, moreover, unavoidable in a certain sense, inasmuch as the means at our command for their more perfect exploration and correct comprehension, often appear insufficient. Of far more importance for us at present, is the consideration of such sources and motives of fallacies as lie within ourselves, whether they be owing to imperfect comprehension or wrong points of view, or to desires and aspirations resulting from our peculiar individual tendencies. They are all mutually and most intimately connected with each other, and possess a higher degree of interest here, since they may be avoided more or less, or, at all events, more so than in the former; for, in the end, we and our *ego*—that is, our manner of contemplation, and the relation in which we stand to nature in general, and to the objects of our investigation especially—are more at fault than the latter *per se*. The fact, likewise, of our knowledge and powers, in regard to the grand problems of our subject, being necessarily defective, is of less significance to us in this case than the circumstance that we do not even know how to comprehend, in a correct and clear manner, things which ought by no means to be beyond the horizon of our faculties. And the precise circumstances or causes which impede, if not altogether prevent the attainment of what is possible for us, and lead us into error where we might have found truth if we had sought for it more skilfully or more earnestly, require to be specially considered here.

That a powerful influence is exercised by the susceptibility and moral condition of the individual upon observation and judgment, and that hence an abundant source of error arises, requires no proof. Everything which makes a very vivid impression upon our feelings, particularly if it captivate our imagination, which keeps our interest in suspense, which rouses our will and energy, or excites our fears in an unusual degree, influences our whole intellectual being, and at the same time clouds our perceptions or warps our judgment. In this state we are less fitted for a calm and circumspect investigation and correct judgment; and whatever has in this manner once taken hold upon our minds may be compared to a pair of spectacles improperly adapted to our vision. The one is of necessity almost as certainly allied to the other, as that a person under the influence of wine or passion observes, examines, and judges quite differently from what he would in a sober and calm condition.

The relation in which the observer is accustomed to be placed in regard to the object of his research—that is, to nature and reality—continually favours the operation of this cause of error, and this in proportion to the influence of the former upon himself or his intellectual being; whilst it is only by the slow road of research that we can obtain an insight into nature, into vital phenomena, disease, &c., and, above all, by the help of the cool and sober operations of our reason: on the other hand, much of that which is submitted to our observation acts less upon our reason and judgment than upon our feelings, imagination, and vague recollections, and more upon these than upon the exercise of our will. Everybody has perhaps experienced the excitement produced by a new discovery, or a new and striking idea, whether his own or another's; and the judgment and approbation of the bulk of mankind are more prone to be influenced by that which acts upon their feelings and imagination than by that which merely appeals to their understanding. It is, in short, exactly that part of our nature which is influenced by external circumstances, and attuned to vibrate to their touch, and in the naturalist or physician not less than in others. Our feelings thus wrought upon, we are no longer what we were, and what we ought to be for a calm investigation. Now, whether the stimulus be no more than an ardent zeal to find out, prove, or accomplish anything, slightly tinctured, perhaps, with ambition, vanity, or rivalship; or whether it be fear, excited by supernatural events or religious belief; or lastly, whether it be the apprehension of pain and disease, or anxiety to alleviate it in others, after all, in substance, it remains the same. For all these are but gradations of passionate excitement; and let them, as we advance, manifest themselves in different directions, still their influence upon our observation, conception, and judgment, after all, will only prove different in degree and not in kind. No sooner do certain feelings or tendencies preponderate, than we cease to preserve that calmness of examination which we may possess in reference to other matters, and which is so essential in our observations and researches.

This effect is the more likely to be produced the greater the natural vivacity of the individual, the more confined the extent and solidity of his knowledge, the lower his horizon, the more limited his education, perhaps, and lastly, the more he is deficient in an intimate knowledge of his subject, and in discrimination and clearness of judgment. The earnest desire, too, of finding something corresponding to our own views and expectations permits of our seeing such correspondence where it does not exist; or this may happen from our being under the dominion of some authority. In the former case we overlook the arguments on the opposite side because we do not wish to see them, because they are not consonant with our own notions, wishes, and tendencies—or, at least, we do not view and judge of the matter according to the actual state of things.

In both cases our state of excitement and mental tendency, whether we be conscious of it or not, our interest and narrow-mindedness have blinded us to the defects of our whole argument. In the one case we do not ask for the same strict proofs in accepting a thing as true and evident, no more, for example, than does a partial, although perhaps a just judge, or a favorably disposed jury; we are satisfied with half-evidence, or with such as is false, whilst in the other case we may, perhaps, be blind to the most clear and valid reasons. In the one case we perhaps merely notice that small part of the whole which may be correct and proved, or at least probable enough, because it just happens to answer to our ideas and wishes, and overlook that which is wanting in the evidence, or which would require quite a different interpretation. In the other case it is just the reverse, although we may not be aware of it, perhaps because we really have no hidden intention, or because we believe ourselves to be free from all peculiar tendencies, narrow-mindedness, or partiality. The history of all times furnishes us with examples of these sources of error; and if we justly wonder at the errors and blindness and great superstition of our ancestors—for instance at their alchemy and astrology, their philosopher's stone, or their faith in so-called signatures, witchcraft, &c.—our successors will probably have little difficulty in discovering similar aberrations and fallacies in us.

But the above further shows how, in the end, it is our judgment and reason—that is, our interpretation and examination of the things we observe, and especially the conclusions we draw from them—by which we err, even in the case of such fallacies as are wont, proximately and especially, to have their origin in our sentient and moral being. It is always from one or other of these causes that seeming arguments appear to us to be real, and false conclusions to be correct. We shall fall into such an error much more easily and directly if its remoter sources, and the circumstances which dispose us to it at the moment, are on the side of our reason and judgment. For faults and defects in this quarter naturally exercise an immediate influence upon our whole mode of proceeding, especially in respect to what is most important here—namely, the general character of our arguments and conclusions, so that they likewise may become defective, if not utterly wrong. But as soon as our judgment has once become influenced in this manner from any quarter, so that the freedom of its action has become disturbed, and that in future it can only be looked upon as an erroneous conclusion *pro* or *contra*, it becomes a prejudice, and in so far one of the chief sources of all our errors. But the manner in which we arrive at it, and the remote motives intrinsically connected with our prejudice, may also be manifold.

The influence of our *ego*—*i. e.*, the great and inevitable part which it plays in all our researches, observations, and conclusions—must be regarded as the most determining and important cause of such prejudices, because, lying essentially in the whole nature of man, it manifests itself, more or less, in all of us. This, also, has been sufficiently alluded to on a former occasion. All things external to us,—nature, with all her phenomena and processes, whether they be called animate or inanimate,—only exist for us in so far as we have some conception or idea of them. It is not so much the things in themselves which have become intelligible to us by all our observations and researches, as rather our own conception and views of them, as these have arisen in one way or another from those observations and the deductions drawn from them. Those phenomena and things exist, as it were, for us only in so far as we have formed some conception of them; and on that account we are so easily induced to believe that they can only exist in reality, precisely as we happen to conceive of them. Moreover, we are wont to take an interest in them, or occupy our minds with them, only in so far as they correspond to our own ideas and views of them; and whatever we observe, hear, or otherwise ascertain concerning them at a later period, must, as it were, adapt itself to our former ideas or views upon the subject before it can appear correct to us. Here, as everywhere else, we have, as investigators, hardly any other alternative than to try to bring things into such relations to each other as we regard as the most probable ones; but these will most frequently be such as best correspond to our former conceptions and views. In natural science every explanation and hypothesis is readily accepted as the correct one, provided it only harmonizes with our previous ideas and notions.

From this we may likewise understand how it is not easily given to any one, and least of all in a department of science like ours, to adhere strictly to what is considered the first condition of every observation and research—namely, to be entirely unbiassed, and to take, in every case, a free and impartial view of the subject. But as these preconceived ideas and views of ours may be false, and especially as they are, at best, but narrow, defective, and vague, our judgment, at any later moment, will almost necessarily contain more or less of prejudice. At all events, it would be above the nature and powers of man to guard against all errors from that quarter, and no illusion is more ridiculous, none more dangerous, than to imagine that it is nevertheless possible to avoid them.

For even the honest investigator, the most sober thinker, cannot so far emancipate himself from that influence, and from the consequent obscuration of his views and judgment, as would be requisite, or as he himself may wish or believe. Whilst he cherishes the belief that he confines himself to what is objective, and to facts, he easily overlooks the circumstance that it is never his senses which observe and discover, or combine and explain anything, but rather his mind and its interference which, as it were, render those facts what they are. And it is precisely this his mode of conception, which renders it impossible to free himself from the influence of his intellectual being, his whole train of thought and feeling at the moment. But these, again, are most intimately connected with the whole world of experiences, ideas, and notions which he has gained in the course of time. Whether we may wish and approve of it or not, there have long existed in us a certain knowledge and manner of viewing the things around us, and a certain way of conceiving and representing them in one relation or another, as well to each other as to ourselves.

And all this, however correct or incorrect it may be, exerts a constant influence upon us all, and becomes, more or less, a measure of our judgment—an authority. We can neither render it ineffective, nor do away with it, any more, for instance, than we can strip off our skin (D'Alembert). But if this happens in the solution of ordinary problems, of which, among others, the well-known tale of Homer and his fishermen affords a striking instance, it will take place in a much higher degree in the case of the problems of nature and our efforts to solve them.

Moreover, our whole world of ideas, our habitual manner of thinking, conceiving, and judging of all the things around us, is continually taking new shapes, according to the mental constitution of the individual, his peculiar abilities and tendencies, the extent and clearness of his horizon, the degree of intellectual development he has attained, and the nation he belongs to.

Everybody, for instance, is aware of the difference which exists in this respect between the Germans and the English or French, between the North and the South; and how, in natural science, as well in medicine as in other branches, the propensity of the former for a no less grand than vague and obscure mode of conception and generalization, for premature and groundless, because too comprehensive and profound systematizing and theorizing is regarded as characteristic. Our mode of reasoning and judging of a given case will further vary continually with the society around us, and with the circumstances and times in which we live. And as we are always influenced by the remembrance of what we experienced and thought on former occasions, and by the manner in which we have previously been wont to observe and reason, the importance of another circumstance becomes evident at the same time—namely, of the period at which we first became acquainted with certain things.

Much depends, also, upon the point of view from which we first learnt to observe various things—*e. g.*, the problems and phenomena in our department of science—and upon the methods which we adopted in our observations. For all this cannot fail to exercise a powerful influence upon the views that we may take of all our future experiences and observations during our whole lives, as well as upon the conclusions we draw from them. Thus each of us may find, upon a more minute examination, that he has arrived, perhaps unconsciously, at one inference or another, and consequently at certain general ideas or notions, by things which he observed or experienced on some former occasion. Perhaps we have never given utterance to them, nor even arrived at a clear perception of them ourselves; but they may, nevertheless, influence our judgment on a future occasion, as readily, for instance, as the likeness of one person to another, long ago become repugnant or endeared to us, will more or less determine our opinions, our sympathies, or antipathies in regard to the former.

But all this gives a certain embarrassment and partiality to our judgment, by which we now may be led just as readily to a rash, perhaps totally wrong belief in the truth of certain things or views, as to unbelief or rejection of others. This danger threatens us because, if we observe, in a given case, anything which agrees with our former views and expectations, we are not wont to seek, with anything like the same zeal, for further causes and proofs, as when it is contradictory to them. In the latter case we are apt to be too critical and sceptical, and even ask for proofs which cannot be furnished to such an extent, and with such accuracy and strictness, in a department like ours; while in the former case we are too credulous, and are satisfied, perhaps, with the most equivocal arguments or facts. If, for instance, we once assume some condition or causal connection as the true or most probable one, in a given function or disease, we continue to observe, search, and reason on in the same track, without taking other and quite different possibilities sufficiently into consideration. We treat our view and conception of it at the moment as a fact about which there can be no further doubt, and which now only requires to be explained, or employed for the explanation of other things. From these our general ideas or principles we now form our opinions or explanations of individual phenomena and processes. If any one, for instance, has assumed some anatomical lesion of the lungs, the brain, or one of the valves of the heart, or some peculiar "crasis" of the blood, as the essential and determining condition of a given disease, he looks upon everything else—all the other phenomena and processes which may be present—as the effect of the same cause. He explains and arranges everything to his own satisfaction, just as it appears to him as alone possible or probable, according to his anatomical or chemical point of view. If the belief has once possessed him that he has cured a patient by a given remedy, nothing remains for him to do but to inquire or to explain in what manner this result may have been effected.

And he who assumes the atmosphere of marshes, or "miasmata," to be the essential cause of intermittent fever, will not hesitate to consider and explain all the other concomitant circumstances, relations, and influences—*e. g.*, the higher temperature of summer, of the tropics, &c.—in exact accordance with this point of view—that is to say, as promoting the formation and intensity of action of those marsh-miasms, and, consequently, the development of intermittent fever. He perhaps takes too little into consideration various other cooperating circumstances, which may be modified in different ways, and act more or less according to the season of the year, the temperature and climate, the quality and culture of the soil, &c.

In short, here as little as anywhere else, can we consider ourselves guarded against the danger of such errors of judgment. On a more calm examination, it becomes sufficiently obvious to every observer and investigator, especially in our department of science, that in the study of natural phenomena we are little better off than children, and that in this immeasurable labyrinth we wander about well nigh as strangers. We therefore necessarily look about for some support, and for a certain guidance; and what first naturally presents itself to us is exactly what we formerly knew, thought, and remember, concerning various things, or certain views, ideas, or theories, which are the prevalent ones among our contemporaries or with ourselves. We constantly reason, and form combinations and conclusions, under the influence of some authority or other. And we shall be governed by this authority the more, the less we possess a clear and self-acquired knowledge of our own and a thorough comprehension of our subject, and the greater is the general uncertainty of knowledge in the particular department of science—as, for instance, in ours. For the feeling of our own uncertainty, and the want of a trustworthy basis for our judgment, of firm ground beneath our feet, make us submit the more readily to every foreign authority, whether it be such as is old, or such as is novel and in vogue at the time.

In a science such as ours, in which the objects to be investigated are so manifold and complicated, and at the same time so little understood, we may arrange everything nearly to our own liking, as well, for instance, as in politics. We may bring whole series of phenomena and processes, or influences, into certain new relations to each other, and thus explain them in a manner which is novel and peculiar to ourselves; we may turn our attention in different directions, and to different things, which, perhaps, had not been noticed previously, or at least not in this light. And if we possess not only acuteness of mind, with a certain amount of knowledge and faculty of combination, but likewise sufficient eloquence and reputation, we shall find many who believe us, and many who do not. They will have faith in anything novel the more readily, if it agrees more or less with their own former notions and points of view, and if they believe that it furnishes further confirmation of them, whilst, in the reverse case, they reject it. Even in every day life we are easily influenced by the words and opinions of others, provided they correspond to our own, or perhaps even confirm them; while, in the opposite case, we doubt or reject them. For some everything which is novel, striking, or piquant, has a much more irresistible charm than for others; they view nearly everything in the exact light which the new doctrine and fashionable theory of the day throw upon it.

Thus, in the end, it will depend entirely upon concomitant circumstances and upon the peculiarities of individuals, how far we are accessible to the influence of any authority, of novelty, or fashion—*i. e.* whether we believe in it, perhaps are enthusiastic about it, or reject it altogether. In many respects, indeed, every new point of view, everything which draws us away from our old fashioned, perhaps narrow and false views, and shows us things in a new light, may, for that very reason, further our knowledge, or at least prepare the way for its acquisition, and ought, therefore, to be welcome to us. But, in reality, such new views more rarely influence us in this way than we ourselves are perhaps aware of. For, as we have shown already, we do not easily, at a later period, relinquish the notions and views which we have once adopted, and incorporated, as it were, with our whole intellectual being and consciousness, either in any essential points, or for any length of time. Perhaps we could hardly do otherwise, however earnestly we may desire to do so, any more than we can feel quite at home in a foreign country. We are all young but once, we can only once go through the whole cycle of experiences and all the processes which have brought our inward being into connection with nature and men, as well as with the facts and doctrines of our science; and indeed into the exact kind of connection which has now become our own, or, in other words, which forms our whole world of ideas, and peculiar mode of thinking and judging of everything around us. How rarely, upon the whole, this happens, is proved by the circumstance that the world is wont to wonder at every really novel mode of viewing things, and at every discovery or original idea in individuals, and to attack or do homage to them according to circumstances. And at the same time it shows how rarely we are able to free ourselves from common and predominant notions, and from the influence of what we have learnt ourselves, or of traditional knowledge and opinions. Thus, for instance, the so-called mediocrity even amongst the learned and men of science, will not easily give up such traditional opinions; their intellectual horizon, and the influence of the views already adopted, as well as interests of an entirely different nature, perhaps, are opposed to this. From this it also follows, that in no science can the views and the approval of contemporaries be looked upon as a proof of the correctness of a theory, or an opinion, any more than their condemnation can be regarded as a proof of the contrary. "Anticipationes," says Bacon, "satis firmae sunt ad consensum. Quandoquidem si homines etiam insanirent ad unum modum et conformiter, illi satis bene inter se congruere possent." Without ever seeing with their own eyes, or standing upon and walking with their own legs, they remain throughout their whole lives subject to a foreign authority, and it is only this latter which changes, whilst they perhaps believe that they have sufficiently emancipated themselves from it.

Others, again, are prejudiced against all that is novel, and confirmed in their dislike of it from other causes. The more experienced and cooler thinking man is perhaps aware, from his own experience, how difficult it was even to arrive at his present state of knowledge, and how easily it may happen that by one false step in advance or sideways, he may lose more than he can gain. But especially in a department of science, which, like medicine, is at the same time a practical one, this is a point of double importance: and not without cause may he now reject everything novel, which threatens to disturb, if not to destroy this comparative accuracy of his knowledge and practice; especially as he does not know how much, perhaps even of what is good, may be lost at the same time. But setting aside these and other reasons, which may be in themselves legitimate, his opposition to all that is new may spring from prejudice, narrowmindedness, or total error, and may thus extend so far as to degenerate into the well-known plodding spirit of the multitude. If one party, the empirics of the old school, for instance, doubt much that is novel chiefly because it does not correspond to their views, its advocates, on the other hand, are frequently not less mistaken, and that in consequence of their predilections, and partiality for their own prevailing view or conception of the subject. This, for instance, applies just as well to the representatives and followers of our so-called modern exact medicine, to pathological anatomists, microscopists, and chemists, as to the physicians of the old school. For what the former cannot see nor lay hold of, is said, according to many, not to exist at all; and if others hold a different opinion upon any subject, it is pronounced to be erroneous or obsolete, if not wholly absurd. And, on the other hand, all novel views and explanations, even the most correct experiences and facts, may be doubted, ignored, or attacked by the latter, only because they happen to be contradictory to the notions which have become habitual to them. If, for instance, some stubbornly adhere to the ancient routine, and perhaps boast, as empirics, of an experience which they do not and cannot possess, others build theory upon theory, which, in a department like ours, so frequently may have the appearance, but cannot bear the real character, of scientific explanations. Both parties are confirmed in their mode of viewing things by the circumstance, that hardly in any other department of science is it so rare, upon the whole, as in medicine, to find a more universal education, a certain philosophical and clear insight into the sources of our knowledge, and into the origin as well as the nature of our notions and their connection with each other. This may also serve to explain, why, in medical science, so much may be believed, and, on the other hand, so much doubted, without there being adequate reasons for the one or for the other.

Many may be of opinion that a discovery or assertion of A. cannot possibly be correct, or else certainly many others must have found it out previously. In this they overlook what is peculiar to genius, namely, to have a preconception of one thing or another, to make combinations of various kinds, and now, under favorable circumstances, to find out and to demonstrate what others did not surmise and therefore did not look for. With some, this doubt of everything new and unaccustomed, of everything which does not agree with their own ideas or views, grows to a perfect spirit of contradiction. And great minds are, in the end, no less exposed to errors of this kind than mediocre ones, and in the case of both it frequently enough happens that some motive of a personal or moral kind is added to those which are strictly scientific. Thus in a case in which we perhaps reject the most certain facts and established truths, because we do not understand them, or because we are too much attached to our previous notions, it not unfrequently happens that some private interest, some rivalship or other, or at least the desire to uphold former views and assertions in spite of everything, is the inducing cause. And certainly every one who, for instance, has written a comprehensive and complete work on any experimental science, on any subjects or problems of natural history, whether on physiology in general, or on diseases and their cure in particular, has been necessitated to state and maintain many things without having had sufficient and absolutely valid proofs at his disposal. He has, perhaps, assumed things as probable which were only remotely possible, or accepted as established truths, what ought, at most, to have appeared probable to him, and has, indeed, to a certain extent, been justified in so doing. For he himself, equally with his readers, frequently had no other alternative than to accept much in good faith, were it only because they themselves were not able previously to investigate and to examine everything accurately. And consequently future corrections, doubts, and contradictions will never be wanting, because there is never any lack of hazardous and partial views and assertions, and even of decided deceptions in a science like medicine. But he who is once possessed by the spirit of contradiction, and of stubbornness of opinion, will try to maintain his former views, even in spite of the most valid reasons and refutations, and often will rather doubt the most evident truth, than their correctness. And thus it happens, in the end, that any one may so easily, almost from a certain innate necessity, continue to be deceived and mistaken so soon as he has once fallen into error; and this nowhere more readily than in reference to matters and questions, our knowledge of which is still so fluctuating and obscure, and our mode of conceiving and judging of them so arbitrary, as in medical science.

Moreover, the very nature of man induces him, to a certain extent, to advance the most rash and premature opinions concerning the precise things which he understands least of all, and to which he has given the least amount of mature and calm thought. For here, least of all, is he acquainted with all the details of a given question, with all its conditions or complications, and because his knowledge and experience do not give him any certain grounds for one view or another, he now the more readily draws an arbitrary conclusion suitable to his own wishes. And again it can hardly be doubted that errors from this source occur more frequently in medical science than in any other. For even to explain, in some measure, the most simple phenomena in the living body, or the most frequent occurrences in our patients, we should require an amount of knowledge which we are far from possessing, and still less does our knowledge suffice for the construction of comprehensive and correct theories. And yet, on the other hand, we almost daily find ourselves induced, and even compelled, in our capacity as teachers and writers, to give some explanation of various things, and we do this, as well as we are able, in accordance with our views at the moment. No one can well be blind to all these gaps and defects, and misapprehend or ignore them altogether; and the wish to see them filled up will become the more ardent, and our love of science, as well as our necessities, induce us to help to effect this, the more fully we are conscious of their existence. We attempt it as well as we can, and, in doing so, are easily led out of our depth. We strive, by hypotheses or arbitrary conjectures, by calculation and imagination, to supply what is wanting in sound and positive knowledge. We attempt to gain, by a bold *coup-de-main*, what could only be taken by a regular and protracted campaign, and frequently fail altogether. But if, in departments of science which have been least illuminated by the torch of research, the wish to see and find one state of things rather than any other be superadded, there will be no end of *a priori* combinations, of arbitrary, even paradoxical assertions and attempts at explanation. And as these now fall into the hands of men of essentially the same class, and of not much greater knowledge, their fate, that is the approval or opposition they meet with, will depend less upon their correctness and real merit, than upon their agreeing with, or being opposed to, the views or tendencies of the majority, and especially to the opinions of the highest contemporary authorities. Moreover, a danger is wont to arise from such an unrestrained use of hypotheses and conjectures, from such arbitrary and hazardous attempts at explanation, a danger which is the greater, as they not only chiefly affect that part of our being which is most accessible, namely the imagination, but likewise because by these means two wants or wishes of the great mass or perhaps of every individual are more or less satisfied, at all events more so, than by a logical, sober, and deliberate research and judgment. These are, firstly, the desire of understanding and becoming acquainted with as much as possible in the most easy and convenient manner; and, secondly, of obtaining something comprehensive, and complete for their science, and rules and points of support as certain and fixed as possible for their art and practice; and the great majority are not wont to inquire closely whether all this has been done in a more or less arbitrary and uncertain or even false manner. That which is held out to them is sufficient to satisfy them, at least for some time, and this the more, the better it suits their own ideas and wishes, and the less of the latter they see themselves compelled by it to relinquish. We eagerly accept, almost without examination, the often too confident assistance which is proffered us against so many doubts and misgivings of our own, whether it be in regard to our science, or to our art, very much, for instance, as the beggar takes what is offered to him.

Now it is evident that in a science such as medicine there is a double opportunity for errors of every description from all those quarters which we have been considering, whether it be in consequence of our doubtful, fluctuating, and unreliable knowledge, or owing to our private interests and efforts. And yet one of the most important possessions of man, his health, is at stake. The dread of danger and the instinctive impulse to guard against disease and pain, or to see ourselves delivered from those which have once befallen us, equally come into consideration, as well as the efforts of the physician to satisfy this desire as far as possible. For this is what his profession and his conscience demand, and not the less upon its fulfilment do his own advantage and profits, as well as his reputation depend. And thus, lastly, it happens, that numerous inducements and sources of error offer themselves to us at every step.

But if such errors frequently remain within the limits of harmless illusion, so that we merely find and see exactly what we are so fond of seeing—in short, if we frequently err from mere credulity and self-deception—they may, in others, increase to actual superstition or mysticism, the part played by which, in medicine and even in some physiological matters, needs no further explanation here. Men deeply skilled in human nature pretend to have found out that certain tendencies to error are to be found in every, even the most enlightened, individual. But experience teaches, how these germs are first brought to full development by an imperfect, if not decidedly wrong observance of the laws which regulate our thoughts and judgment, whether it be in consequence of a deficiency of education, and of insight generally, or in our particular department of science, or in consequence of religious points of view, favoured, perhaps, by peculiar experiences and so forth.

But in order to make this entire mechanism or process of erring, which arises from all those different peculiarities of disposition, more clearly understood, and to bring it before us according to the actual state of things, Logic must proceed with a certain knowledge of the subject and of human nature in general. The individual investigator must not only be well acquainted with his department, but must likewise have sufficient knowledge of mankind in general; and the old proverb, "Learn to know thyself," is especially applicable to the natural philosopher and to the physician. For only by this means can he hope to arrive at a better understanding of the mental processes going on in others, and at a better distinction of the subjective from the objective, of the additions made by individual observers and investigators from the actual state of things and from that which is really perceivable by the senses. It ought to be generally known that the natural philosopher is in the same position and proceeds in the same manner as others in regard to his observations and explanations, his opinions and conclusions—*i. e.*, not only as a man in general, but also as a man of a peculiar disposition and character, according as his mode of viewing things happens to be at the time; that even in his researches he can never divest himself of this, but that, on the contrary, he will view, comprehend, and judge everything differently according to his whole train of feeling and reasoning; for, in the end, it is of little use to ascribe those aberrations in an off-handed manner to onesidedness, prejudice, superstition, or mysticism, and the like. For these are not, in themselves, defined causes and levers, but are most intimately connected with the whole intellectual character, with the whole mode of conception in individuals, and subject, moreover, to the most manifold transitions into each other. Everything will here rather depend upon a more delicate psychical analysis, and our recognition of those errors in our science will only become sufficiently clear by our knowing how to trace in detail their so complicated and ever-varying sources, and the manifold combinations of cause and effect in them. Of especial interest to us in this place are such errors as spring, more or less directly, from our defective or otherwise faulty understanding and judgment. For in so far as they are wont to arise from the adoption of erroneous or bad methods of investigation, in themselves, and from false inferences drawn from their results, and consequently consist essentially in a wrong or defective application of logical rules, they fall more within the province of our Logic than others.

If, for example, any one arrives at a false view in regard to the mode of origin and intrinsic connection of a disease because he still believes in supernatural agencies, in spells and witches, and ascribes the falling ill of some person to these, this error does not come under our consideration nearly as much as if he has erred in his judgment about it, because he has made use of defective or erroneous methods and expedients in his observations, or because he has drawn wrong conclusions from them in consequence of ignorance, onesidedness, and such like. In the same manner it is of greater interest to us here that a physician should believe himself capable of understanding the whole of a disease and its causal connection by the pulse only, or by similar symptoms, by the use of the stethoscope or percussion, or perhaps by an analysis of the blood, or a postmortem examination, than that a quack pretends to find it out from the urine which has been sent to him. And if any one persuade himself that he can cure diseases, such as typhus, gout, tubercular disease of the lungs, mania, &c., by his remedies, because he desires to serve his patient as well as himself by this belief, Logic has likewise less to do with this than if he had arrived at the same error in consequence of imperfect observation at the bed-side or at his experimenting-table, and at wrong conclusions drawn therefrom. In the following part, therefore, more especial mention will be made of these sources or motives of error, that is to say, in so far as it arises essentially from a faulty application of the common rules and laws for the observation and establishment of facts, and for correctly appreciating and drawing conclusions from them.

But even among errors of this kind by no means all are of the same interest to us, as some much more than others have their source in the nature of man, as well as in the peculiar species and character of our objects of investigation, and on that account are more difficult to avoid than certain others. In the end, indeed, we are all but unskilful horsemen on our own ground, and "whichever way God and our horse wishes" applies more or less to all of us. And yet the one rides his horse not only with more grace, but likewise with better success, than the other, because there exists a great difference between knowing a thing and knowing it well, and still more between knowledge and understanding and the power of executing, between the will and the deed. We daily see natural philosophers and physicians mistaken in questions and matters where others did not err; where, therefore, it was likewise more or less in the power of the former to have guarded against error, provided they had better understood and more earnestly sought to do so. The chief sources of such errors are therefore essentially of a subjective, or personal character, whether it be a want of knowledge of the subject, and of practice, levity, a rash disposition, or prejudice and superstition, perhaps even the belief that certain opinions and views require no demonstration. All these motives of our errors, whether they be remote or near at hand, may be avoided by any one if he will but adhere to the common rules of logic, and apply them to his department of science; and since these have been explained connectedly on a former occasion, they require no further mention in this place.

It is otherwise with errors which arise from faulty or at least onesided and obscure general points of view in our science, in short, from faults and defects in the science itself, as well as from certain faulty methods of investigation in it. And precisely these causes of error are more frequent, and at all events far more serious than the former, for they depend essentially upon the doubtful and unexplained nature of our whole subject. If the mere comprehension and distinction of the individual vital processes or influences often appear difficult enough to us, much greater difficulties in regard to our judging them arise from the circumstance, that hitherto we have become acquainted, and that very deficiently, with a small part only of their extent, importance, and character, and of their relations to each other. It is impossible, therefore, that we can already have any clear and correct conceptions of them; and it is not given to everyone to reason clearly upon *anything* in such a doubtful and obscure department of science, or to give even to himself a satisfactory account of what he means or wishes to say. It may, perhaps, rather be assumed that it is, in general, impossible for the human mind to undertake any investigation, and especially any interpretation or appreciation of what it has observed, without being influenced by partiality or prejudice. For in all cases, and especially in the theory of diseases, and of their causes and cure, well nigh all positive and secure points of support, and bases for the investigation of them, are hitherto wanting. We are especially in want of that criticism of the improbable, or barely probable and possible, by our science itself, and by parts and doctrines of it which are reliable and once for all fixed, as it exists in physics and chemistry, and partly too in physiology. No sooner, therefore, do we attempt to go beyond simple experience and facts—beyond that, which we are taught by our senses—than, whether we wish it or not, our *ego*, our subjective opinions and intervention must play a preponderating part. But by this very means the way is opened for our falling into error, in consequence of wrong starting-points, one-sided conceptions, or preconceived notions.

Our task then is, to bring these errors before our readers, and, for the sake of a better view, to do so, as far as possible, in a certain order.

All research and explanation in matters belonging to medicine as well as to nature, in general, consist, in the end, in our drawing a conclusion from the behaviour of certain phenomena and processes in individual cases, ascertained by observation and experience, as to the behaviour of all phenomena, &c. of the same kind, and more especially as to their causal connection—*i. e.*, as to the laws of their origin and development, or of their action. By observation, and by the discovery of the general state of things, we have to prove the correctness of this conclusion—*i. e.* the existence of the causes or effects which we have deduced or assumed. In the end, therefore, all our errors and faults can only consist in the circumstance, that in some place or other, a link, an element, and even, in most cases, several are wanting in our investigation and demonstration. We draw a false conclusion from the things observed by us, whether it may be owing to our having observed them in an imperfect and wrong manner, or to our having interpreted them falsely and brought them into a causal connection which does not correspond to the actual state of things.

We shall now successively treat of our errors in the individual steps or parts of investigation and demonstration. But in this no strict classification can be thought of, because those errors themselves are most intimately interwoven, and because it is less our object to give a special description of them than to show their intrinsic connection. If, for instance, there is a deficiency of observation and determination of the true state of things, there will usually be a much greater want of comparison of them with others, of correct interpretation of what has been observed, and of conclusions as to the exact connection of cause and effect. And if we happen previously to have had some incorrect, or at least obscure notions in regard to the latter, if our whole point of view has been in any way imperfect, or if we had a special interest to find some particular state of things and not another, the observation itself will very rarely prove quite correct. It has, therefore, appeared suitable, first of all, to give a more close consideration to certain complicated errors—namely, to those in which the whole mode of conception, the general starting points and manner of investigation and interpretation have been wrong, or at least onesided and confused, which naturally could not remain without influence, as well upon the observation and establishment of facts, as upon their interpretation, and upon the whole method of demonstration in individual cases. In so far, such errors may be termed "Collective- or universal-errors," in comparison with others which we shall consider afterwards.

I. ERRORS IN THE WHOLE MODE OF CONTEMPLATION, OR CONCEPTION AND JUDGMENT.

(Collective or universal errors.)

History shows us how long it took the human mind, how many circuitous paths and indirect and erroneous methods it had to adopt, ere it advanced so far as to occupy that objective sober position in regard to nature and reality which now, at least, appears the only correct and possible one.

This lies, indeed, entirely in the nature of the subject itself. All that which we are accustomed to include in the term nature," and of this, again, especially the "animate" part, is of such a kind, that we can only arrive by successive steps at a more correct and profound understanding of it. Here the human mind cannot accomplish or discover anything by itself, and at once, as it has in its power to do in the sphere of imagination and art. For this, not only able heads and faculties, which always have been, and always will be, rare, are required, but likewise time, a certain solid advance of understanding,—and this, again, not only in every individual department of natural science in particular, but rather in all of them together. With all this, however, our whole conception of nature and life must, in the course of time, become a different one; further, the view we take of our relations to everything which exists and takes place externally to our bodies, as well as of its relations within itself; and finally, no less our opinion as to the mode by which we may acquire an insight into these relations, and into the so-called "laws of nature."

On that account, also, every individual has always been more or less dependent, in his views of nature and life, as well as of all other things, upon the degree of civilisation and knowledge attained by his nation, and even by mankind generally, at the period in which he lived,—the sincerely religious man of old times, as well as the sceptic and materialist of our own day. Everyone is and remains a child of his time, and however much an individual genius may be in advance of it, he will be so in a far higher degree by his grand and free conception, by his correct surmises of the mutual relations of various things, than by his penetrating investigation of them, and consequent sufficiently comprehensive and correct demonstration of the accuracy of his views. Many a one, indeed, discovers single important facts; but rarely or never will it be given to one man to bring them into their proper connection with all the things bound up with them, or even to determine with sufficient accuracy the conditions of their origin, mutual relations, and laws. This necessarily shows that even the greatest minds of all times will essentially remain within the limits of the general position which their period holds, and more or less under its influence. And this position, these general views and conceptions, have, as we have already observed, continually been changing, and fortunately, by degrees, always become more correct ones.

Thus it is well known that, even as late as during the middle ages, superstition of every description—*e. g.*, the belief in a direct agency and mediation of the devil and his adjutants, male as well as female, in the power of witches and sorcerers—prevailed universally. Now, at that time, the ascribing of all sorts of things,—for instance, of the causes of tempests, storms, earthquakes, and such like events in nature, as likewise of diseases and their cure,—to the devil and witchcraft, or perhaps, in deficiency of something better, to the Jews, may have been just as natural as our present disbelief of, and even aversion to, every explanation of this kind. And, even in the present day, the uneducated man, the obscure thinker and mystic, for instance, takes quite a different view of the subject from the natural philosopher or physician. We are likewise well acquainted with the part which that belief in supernatural agencies and processes, in the power of magic, and such like, has played in the whole field of natural science—in physics and chemistry, for instance, in astronomy and meteorology, as well as in medicine—and how it has been continued even to our times, especially in the latter science. In those times, as well as in our own, it was found more convenient, and, in a certain sense, even more natural, for man to speculate upon diseases and their cure, upon phenomena and processes in nature generally, and to attribute them to various hypothetical influences or powers, just as he happened to form a notion of them, than to examine carefully and laboriously into their intrinsic relations. Here, still more readily than elsewhere, men took their *ego* as the starting point for their conclusions, and easily believed, not only that all the *radii* of the world must necessarily meet in a person of so much importance as man, but likewise that he could and might explain everything out of himself by his views and ideas. The mental tendency and degree of civilisation of those times could, indeed, only lead to such hypotheses and notions about those things as can now be considered no better than superstition. At the same time this superstition, whatever form it might take, was, in the end, based upon the natural want or desire of man to interpret everything within and around him, and in all these cases to give to himself and his ideas a point from which he might start, or to which he might hold fast as well as he could.

Men only erred in regard to the appreciation of their knowledge and resources; and having once committed an error in their judgment of that which alone can afford us the insight so much longed for, they likewise erred in the selection of their methods of investigation, as well as in the manner of carrying them out. Even in the present day no one, and least of all in our department of science, can free himself from a certain degree of superstition, taking the word in its more extended sense, inasmuch as he is compelled to believe in many things without sufficient proof, and without a clear comprehension of what he does believe, and of his reasons for so doing. Although he admits the various inexplicable phenomena continually going on around and within him to be mysterious or extraordinary and surprising, still he is anxious for some solution, and in this condition is readily impressed by any interpretation, and assents to anything which may facilitate an explanation, or affords some compensation for a better understanding. It is from this condition of things that superstition derives its chief support, in whatever shape it may present itself, and in whatever direction it may offer its assistance. However wrong it may be, man always believes that he can, by its aid, satisfy some want of his nature. Superstition flatters him with the fulfilment of his dearest wishes, and not only holds out the prospect of understanding that which he has not yet learnt to comprehend, but likewise of accomplishing that for which he has been longing, whether this be gold and fortune, health and long life, or a foreknowledge of that which is to come. Now whether he anticipates such results and services from supernatural agencies, from the moon, stars, amulets, philtres, and elixirs of life, or from miraculous pictures of saints, from so-called sympathetic means, from impossible remedial or curative powers, &c., makes no difference in the main. It is likewise sufficiently well known that to this longing and want of man, even in our times, is to be traced his judgment of nature and its reciprocal relations to him, his opinion of the origin of many diseases, and also his belief in certain remedies and their *modus operandi*. And further, the transition from common superstition to modern mysticism may be thus demonstrated, and at the same time an explanation found for the contradiction which always exists between the true investigation of nature and such superstition, whether it present itself in its usual form or not.

A further kind of deviation from the right path, of not less extent and influence, has been termed "teleological," which, originating in man's nature like genuine superstition, arises from the limited character of our comprehension, and from a certain subtle selfishness almost without a motive. For since man, like every other created being, is dependent upon surrounding nature, he must learn to understand her for the furtherance of his own interests; and this is not to be accomplished from nature's own advancements and by her unveiling herself to him, but by his own endeavours, and the exercise of his mental faculties in exploring her. Everything existing and occurring external to him has only such an existence for him as is permitted by his consciousness; all those innumerable threads which bring him into connection with nature and the external world begin at his consciousness and *ego*, and at his notions concerning them, and terminate there also. Hence he is readily brought to look upon himself—*i. e.*, upon his *ego*, which alone has procured him such conceptions—as a sort of centre, and is led to identify all external things with it and his interests. Just as princes and their courts have believed that land and people existed for them alone, so may he at last be simple enough to imagine that nature and the world have been created solely for his sake, to supply his wants alone, and have been arranged and regulated merely to suit his understanding; that even the moon and stars were ordained to stand in a special relation to a being like man, to govern his fates, to rule over his prosperity, his sickness, and recovery, and lastly, to regulate the atmospheric phenomena and climate of his earth.

To the same source, in the end, may be traced the further error, that everything in nature not only exists and occurs for a certain purpose, but also for just such purposes as may happen to appear to *us* the best and most suitable. It was thought that nature must, so to say, go to work with design and judgment, and with such design and according to such laws and principles as we just happened to be acquainted with. Now a twofold temptation to an error of this kind existed at the first origin of medicine, more especially as an art. Led on and animated by the desire of curing diseases, &c., the practitioner was from the very first induced to bring everything existing and taking place in nature, into a one-sided, teleological relation to the welfare and prosperity of man, to his falling ill and still more to his recovery, and to observe and judge of it from this point of view alone. Thus, instead of looking upon the life and well-being of man simply as the necessary consequence of the processes in his body, acting in unison, according to fixed and never changing laws, and also as the consequence of the influences and agencies of the external world acting here as elsewhere according to their own laws, all this was thought to take place with the very purpose and object that man should prosper and continue healthy. And again, in the case of his falling sick, instead of looking upon it as a thing taking place and proceeding in his organism according to its fixed and never-changing laws, only under circumstances and conditions differing from the usual and normal ones, and consequently with other results than the usual one of health, every disease was thought to be a violation or contradiction of the above laws of our organism and life, or a deviation from its purpose. Thus diseases are said to have peculiar hostile intentions, or a desire of occasioning some mischievous disturbance, and one talks of the tendency of inflammation to dissolve and destroy the parts affected by it, of a disposition or tendency of articular rheumatism to affect the heart, and of scarlatina to affect the brain, &c. On the other hand, the healing tendency is attributed to those processes which, by an innate necessity, lead to cure and recovery. So, for instance, absorption and resolution, or the conversion of so-called products of disease, are said to take place for the sole purpose of removing the noxious matter and re-establishing the normal state.

Up to the present day medicine has taken an interest in the atmosphere and the weather, in food, in the properties, modes of action, &c., of drugs, only in so far as they bear upon the maintenance of health, the occurrence of disease, and the process of cure in mankind. And although, in a certain sense, with a perfect right of so doing, still we can neither comprehend and judge of this correctly, nor can we make use of those influences in the proper and most useful manner, unless we have previously acquired a sufficiently comprehensive knowledge of their peculiarities and effects.

The fault of attributing one's own views or ideas, and one's own desires or tendencies, to nature, lies at the bottom of the whole teleological error—in other words, just what one happens to think, and how one does so, is actually thought to exist and take place in nature. And since one is thus far on the high road towards playing the dictator—the arbitrary interpreter of nature's laws—by looking upon one's subjective conceptions as objective and possessing a real existence, there needed but one short step to the so called "ontological" mode of contemplation, and even to downright mysticism—errors and confusions, whose character has made itself well enough known in our department of science. These errors, however, rest essentially upon the same basis as the preceding, inasmuch as we set to work in an *a priori* and arbitrary manner, submitting ourselves to our own disposition and prevailing ideas, and interpreting and arranging nature's proceedings for the convenience of explanation or conception.

Now what is usually denominated "Ontology" consists essentially in this, that by abstracting all the determining circumstances and conditions of a certain event (for example, of life, of some natural phenomenon, or of a disease) and comprising them in one's mind as a something—as an object—a real existence is conferred upon it in one's thoughts, and it is transformed into a peculiar principle, existing and acting for itself. Whether one now conceives it to be a "force," or personifies it in a more palpable and allegorical manner, as possessing a body and substance, or even, like the ancients, as a species of god, and similar metaphors, makes but little difference in the essential motive and character of this mode of contemplation—man is induced to do this by his natural, almost instinctive desire to render things intelligible and to conceive of them in his own way and in a manner as convenient and easy as possible, and therefore corresponding to his own ideas and position; and thus it is that we see this error creeping in in every day life. From inattention, for the sake of convenience or brevity, and for similar reasons, we put in the place of the real state of things, our abstract idea of a thing or "being," even if it is only by bestowing upon it a substantive appellation. When speaking of technical terms, we had occasion to show how we are the more readily led into this error by our customary words and names, and especially by the number of substantives in the Latin language; since we easily believe that to every such particular name or word, and to the ideas which we connect with them, a certain something must correspond. Just as we say, for instance, "history mentions," or "the law, the state demands this and that," whilst it is individual men who mention it, or who have found the law suitable, so we likewise speak of memory, reason, will, or of heat, light, tempest, and their peculiarities, conditions, effects, and laws (as existing) in and by themselves. And if the savage attributes peculiar causes to all he sees or feels, and further personifies these as gods, demons, fairies, and the like, so the physicist, who has seen the electric spark, abstracts therefrom his idea of the electric fluid, and from the solar light and its phenomena and mode of manifesting itself, abstracts the notion of a light-material, beam of light, &c., and similarly the physiologist his nervous fluid, &c. In one case as well as in the other the real state and cause of things was assumed to correspond to the distinctions of all these phenomena, effects, and causes, as we just happen to conceive and arrange them. Thus we speak of electricity, heat, magnetism, and other imponderables, whether we imagine them as an *ens*, a something in itself, or as a mere "force" and so forth, instead of confining ourselves to the bodies and substances themselves, in which we have observed the phenomena or properties denominated warm, cold, luminous, electric, &c., we rather confine ourselves to the causes of them, which we have ourselves abstracted and more or less personified, and now speak of qualities, effects, &c. (*e. g.* of light, heat, electricity), as peculiar matters or forces in themselves. We proceed in exactly the same manner in respect to the *vital force* when we sum up the aggregate of all the circumstances and influences which may lie at the bottom of the whole process of life, as something existing by itself, as a unity. We now readily look upon this abstract *ens* as a kind of self-acting executive power, as an active and effective thing in itself, which is even endowed with a power of creating for itself a substratum, the living body; we speak of its rise and fall, its action in one direction or another, perhaps even with peculiar beneficial tendencies and motives: for the sick, for instance, we imagine this vital force to be disturbed and changed in various directions or ways; remedies and drugs are supposed to affect it by means of peculiar curative or medicinal powers of their own. But in itself already, this vital force—as an offspring of the kindly purposes of nature—has been endowed with a disposition to react against such disturbances of its regular action, as encroachments upon its legitimate executive power, just as an able statesman or general would do, and thus it has manifested itself as the natural healing force. Thus it was assumed that it was this force which led to curative efforts, to favorable crises, and finally to preservation and cure.

By the same method of abstraction, and of the personification of the abstract notions formed, what took place and was observed in patients, came to be regarded as a thing existing *per se*—*i. e.*, as "disease." Now whether we form a certain number of species of disease from the latter, or whether we speak of peculiar acrimonious matters, or poisons (for instance, the poison of gout), and such like, *per se*, or whether we attempt, even somewhat more palpably, to represent those diseases as a kind of organism and parasitical being, also makes little difference in the main point. In the one case, as in the other, we have combined together, as a unity in itself, that which we observed in a certain number of patients similarly affected; from single cases of a "disease" we abstracted the picture, the notion of this disease, as, for instance, we abstracted the notion of the colour "red" from all red things, or that of warmth, hardness, from what we observed in all warm and hard bodies. And as we now speak of the properties, effects, and laws, for instance, of a red colour, or of warmth, as of a thing in itself, we in a like manner speak of those of a disease. Thus one disease is said to increase the heat of the body, to weaken the person attacked by it, to spoil his appetite, or digestion; it is said to grow, ripen, and cease; to be capable of travelling and wandering, of being benign or malignant; to demean itself in a friendly or hostile manner towards colleagues, that is, towards other diseases; to change, be hereditary, or generate a poison, a contagion, by which it infects others and propagates itself. The physician, however, is said happily to have the wish and power of guiding it, driving it here and there, or expelling it entirely, or at least of fixing, neutralizing it, &c.

A further criticism of these ontological views would be superfluous in this place. We have here to treat only of their actual existence, as they happen, in various ways, to develop themselves in us from early youth, and to grow together with our notions, so that they now usually become more or less determining for our whole mode of conceiving and judging throughout life. It has likewise been shown, on a former occasion, how this species of Ontology lies, in a manner, in the nature of the case itself, being not only the nearest at hand and most convenient for our conception of all those natural phenomena, processes, &c., but likewise for explaining and imparting a knowledge of them to others. For exactly this facilitates a concise, and still more a comprehensive, as it were, plastic representation of them, on which account it is very much in favour, especially with teachers, orators, and writers. But that, nevertheless, a certain confusion, and even a decided error lie at the bottom of this whole manner of contemplation, representation, and expression, requires no proof here. We attribute a real existence to the views and conceptions of any phenomenon or object, which we have adopted for the sake of greater convenience, or for other reasons, and now proceed as if everything were exactly so as we have imagined it to be.

From this point also a natural transition takes place to that host of errors, which we are wont to comprise as purely arbitrary, *a priori dicta* and hypotheses, as random assertions and attempts at explanation, or speculations. For, in the end, the same circumstance is to be found at the bottom of these also, namely, that we take it for granted that certain things must necessarily be so, and stand exactly in that relation to each other which we have assumed for them, without, however, having ascertained the real state of things, and therefore without furnishing any positive proof of the accuracy of those our conceptions, attempts at explanation, and theories. But that this kind of self-deception and error has always been the bane of all sciences, and especially of medicine, is sufficiently well known; and it is to be foreseen, that such will continue to be the case so long as we attempt to give an explanation, or even a coherent theory of things, our knowledge of which is, at best, but fragmentary. On that account we likewise find such an *a priori* mode of proceeding, and similar errors, no less in certain branches of physics and chemistry, than in physiology and medicine. In all these sciences we frequently draw conclusions concerning individual cases and their laws, from certain general theories and views, the correctness of which is problematical—at all events not proved by experience—instead of ascending to those general theories through a previous discovery of the former.

But, on the other hand, this whole *a priori* mode of proceeding lies in the nature of our subject itself, and in our legitimate longing for knowledge. We know also that we should hardly ever discover *anything*, unless we had previously thought of its possibility, and that even the most able and sober investigator is wont to pass from his original, frequently hypothetical notions about certain things to the investigation of their real state, therefore from his *ego* to the thing itself, to reality. He now readily proceeds a step further, and takes that as proved, as a fact, which, after all, is so only according to his views.

Yet some persons have always been led into this error more than others, because their whole nature urges them to it. And if they err by overrating the importance and character of their subjective opinion, in itself, there are others again opposed to them, by whom these *a priori* views and mode of proceeding are regarded as entirely worthless, who erroneously believe that they have done all that is necessary by the simple observation of various things, because their capability and horizon find their limits here. For the former, the investigation of all the questions and details cannot proceed quickly enough, and they give too much scope to their *ego*; they mostly see and find too much, and build their theories and explanations upon a basis for which their experience furnishes no guarantee. The latter, on the contrary, adhere to individual and isolated points, and are incapable of putting to themselves higher and more productive problems in their investigations; they now see and find little enough, because they did not understand what and where they were to seek. They readily, however, doubt everything which has been found and asserted by the former, more so, in fact, than is right—*i. e.*, they even doubt what may be correct in their discoveries, or theories. They even despair of the possibility of any discovery or certainty in general, perhaps because it would not succeed in their hands, and could not, in consequence of the peculiar constitution of their minds. These, as well as the others, in a certain sense, confound their *ego* with reality, in giving themselves up to the mistake, that their subjective views and notions of the matter must necessarily be the only correct ones.

The spirit of doubt or scepticism, in general, is, therefore, no less a species of *a priori* error, inasmuch as it leads us to interpret the real state of things in some way, and to form some, though perhaps only a negative opinion about it, without having previously become acquainted with it in a sufficiently clear and comprehensive manner. We perhaps seek, by this means, to guard against arbitrary *a priori* abstractions, without considering, that "doubt is a good beginning, but a bad end." We ignore or deny what we are unable to explain, and reject an unexplained fact altogether rather than confess, openly and honestly, that we cannot account for it, or that it appears surprising to us, perhaps even disagreeable, because it is opposed to our former views.

Thus it appears, in the end, that all the errors which have been alluded to above are, in fact, nearly of one and the same kind. In this long series—from *a priori* conjecture to the ontological and teleological, or even theological mode of contemplation, and finally to downright mysticism or superstition —no distinct boundary can be drawn, because their principle, their fundamental source is, in the end, of the same kind, *i. e.*, because they differ from each other in degree only, or in their prevailing direction. No well-educated man, indeed, will now-a-days believe fully in a devil and his associates, but many a one does believe, more or less, in some personified "forces" and the like (for instance, in a sort of materiality of a disease, in itself), and perhaps hardly ventures to admit to himself his doubts as to others. The well-educated physician of our days may smile at the superstition of his ancestors, who looked upon gold and pearls as sovereign remedies, or expected great things from yellow substances in jaundice, and from red ones in hemorrhages. But if he would only examine more closely why it is that he himself believes in the positive services of a great number of his remedies, he would find no essential difference between his own reasons and proofs, and those of his predecessors. What was then looked upon as most important and determining was assumed to be so in reality; and if certain changes, or perhaps the most remarkable recoveries were observed in a patient, physicians did not long hesitate to look upon them as brought about by their remedies, because they had previously relied upon their having such effects and properties.

Like our predecessors, we proceed to our observations with certain ideas, with previously fixed views, and because we happen to be convinced of the correctness of these, the result, or rather our judgment of it, is such as corresponds to our previous ideas and expectations. Certain phenomena and events, certain agents and influences, which we have perhaps constantly observed to accompany or follow each other, which we have, therefore, always considered as allied, and standing in a causal relation to each other, are now believed to occur simultaneously in all cases, and even to bear the relation to each other of cause and effect. Because we are accustomed to imagine a certain state of things and particular kind of causal connection, and now, perhaps, are not capable of conceiving the possibility of any other, we take these for granted. Our error everywhere consists in our unhesitatingly attributing a real existence to our subjective views and conceptions, whether we further look upon them as active forces and influences, as laws, or personify them as real bodies and entities. Things, therefore, which, so to say, are facts only through and for us —*i. e.*, according to our notions—we in all cases look upon as real, objective facts. And, instead of previously deriving these our ideas and inferences from an adequate determination of the true state of things, and making them subordinate to the latter, we do exactly the reverse. We now judge of and explain the positive according to our notions concerning it; we arrange and deal with it just as our point of view, and perhaps our tendencies and wishes, require.

A thinking man, however, would hardly fall so easily into this error if there were not an entirely different error in the background—namely, the opinion and conviction that those views or notions respecting various things were in reality correct, and even the only possible ones; that they were, in short, self-evident truths, and therefore did not require to be further investigated and determined by appropriate research and experience, &c. From the very beginning we regard them as established truths or facts, just as much, for instance, as our feelings, sentient perceptions, propensities, passions, &c. The error, therefore, consists, in the end, in our confounding certain things, with which we can only become better acquainted by an examination into their relations, and by positive experience concerning them, more or less with such notions or things as are of themselves intelligible to us, because they are in a manner only originated by and for ourselves. But as such a notion, in regard to all the things at an understanding of which we can only arrive by the way of experience and research, is an erroneous one, we commit the greatest breach of all the rules of logic—*i. e.*, we regard such a demonstration of the accuracy of our views as wholly, or at least nearly superfluous. We neglect it altogether, or, even if it be attempted, it is necessarily insufficient and defective, because we neglect to furnish, and, indeed, cannot furnish, the exact proofs which alone could demonstrate anything—namely, the proofs taken from established facts, and from trustworthy experience. Nevertheless, we are satisfied with such, whether it be that we have, in general, no clear notion of that which, in the question before us, would be requisite for the proof; or whether it be that, from whatever reason—for instance, from prejudice, in consequence of peculiar tendencies, &c.—we are satisfied with half-proofs, or even with such as are altogether inadequate.

II. ERRORS IN THE DISCOVERY OF THE STATE OF THINGS.

It has been pointed out on a former occasion why, in this place, no further mention can be made of such errors and mistakes in our observations as have their source in certain defects of our senses or artificial instruments themselves, any more than of those which, in the end, can only be looked upon as consequences of awkwardness, negligence, and the incapacity of individuals. The former are always more or less unavoidable sources of error, and not so much the fault of the individual observer, as of the objects which he is called upon to observe, or of the auxiliary means which are at his disposal. The latter however, are too easily to be avoided to require any further explanation here; and, moreover, the necessary instructions in regard to them can only be furnished by each individual branch of science, not by logic.

Of greater importance to us, however, are such errors as spring from entirely different faults in our observations, especially those which consist in a false interpretation of the things observed, whence errors in our whole judgment and appreciation of the state of things necessarily result. Now in this we may err, either by observing things wrongly—*i. e.*, by believing that we have recognized a state of things, in a given case, which is not the real one—or by entirely overlooking certain things or circumstances in our observation. In reality, indeed, both these kinds of error frequently enough occur simultaneously; but the interest attached to the comprehension of them requires separate consideration in this place.

Now if we examine more closely, how it happens that we make the first mistake, and how we are led to take certain things, which were accessible to our observation, for something which they are not in reality, a circumstance presents itself as the most important and final cause, which has already been alluded to on a former occasion. We mix up what we have recognized through the senses with our view and interpretation of it; we confound or identify, as it were, our conclusions and conceptions of it with what we have really observed. We regard not only the latter, but likewise the former, as real and direct perceptions through the senses, and now record them as such.

A confounding and throwing together of these two elements in our observations is the more easy, as both everywhere occur together, and must naturally do so. Thus, we are never contented with merely seeing or feeling a phenomenon or object, but we wish and are obliged to form some notion of it, and this again only becomes possible through some further explanation or interpretation of what we have observed. The error, therefore, merely consists in our not having been acquainted with, or at least not having noticed, the true state of things, and in our believing and asserting that something has really been observed by us—*i. e.,* recognized through the senses, which is rather only our interpretation or explanation of what we have observed.

Even expert observers are exposed far more frequently than they are aware of to mistakes in consequence of this confounding of different things, because they are not always sufficiently conscious of everything which takes place in an "observation," or of their whole mode of proceeding in it.

We the more readily neglect to make a distinction between these two elements in our observations, because what we recognize through the senses is, in itself, without further interest to us as observers and investigators, but rather only in so far, as we may draw a further conclusion from it concerning the things themselves which we have observed—*i. e.,* concerning their real existence and bearing, their properties, and so forth. It has, in a manner, become a habit with us, to proceed almost instinctively, and without a clear consciousness, directly from our perceptions through the senses to the interpretation of the things observed, and, as it were, to melt down both into *one* act. But this interpretation may now be an incorrect one, even if the observation, in itself, have been completely correct. We know, for instance, how long it took to produce the conviction that it is not the sun which day after day moves through the sky above us. Everybody could see it with his own eyes rising in one quarter and setting in the other, and nothing, therefore, was more natural than the conclusion involved in this perception through the senses, that the sun moves round our earth from east to west. And it is well known that many circuitous paths and protracted researches were required to show that this conclusion was incorrect. No physician hesitates to assert, for instance, that he has seen an ophthalmia, a schirrous ulcer, or even a typhus, a dysentery, a critical sweat, or a sediment in the urine, in his patients. And yet this, as we have shown on a former occasion, is utterly impossible in the strict sense of the term, and in so far this assertion itself is a mistake, *i. e.,* a confounding of that which really took place with our judgment or interpretation of it, however correct the latter may have been. But that such a mistake still more easily and more frequently takes place in everyday life, and especially among less educated or careful persons, is sufficiently well known. Every physician may become aware of this, for example, from the reports of his nurses or midwives; every natural philosopher or physiologist from those of his assistants. It rarely enough happens that such persons are able to describe and report, for instance, what they have observed in a patient, simply as they have noticed it. They do not say only or exactly what they have seen, heard, &c., but at the same time, and especially, what they have concluded from it, and how they interpret it. In this manner every assertion perhaps contains a whole chain of conclusions and notions, if not entire explanations or theories.

But here, as well as in the case of the motion of the sun, this throwing together of observation and interpretation affords the most ample opportunity for error. And it is only a thorough knowledge in each branch of science, and practice in observation, which can protect us against it, and still more a constant attention to this point, namely, the accustoming ourselves to keep these two processes (observation and interpretation) sufficiently separate, and especially to distinguish them in our appreciation of that which we have observed, and in its application to further conclusions and attempts at explanation. But precisely in this point even physicians and natural philosophers fail much more frequently than they are themselves aware of, whence many of their errors may be explained. Thus it had long been observed that patients suffering from dysentery frequently pass no urine, or at least only a very small quantity, and that equally little bile or pigment of bile is to be found in their stools. And thus far this may pass as a fact established by our senses. But we are further wont to speak of a retention of urine or bile as having been observed, and now proceed to assume a spasm of the bladder, &c., as its cause. For, as physicians are accustomed to interpret a state in which no urine is passed as a retention of urine, they now assert that they have observed it in a given case, whilst probably, in most cases, it did not exist at all, but rather there simply happened to be very little or no urine in the bladder at the time, and consequently none could be voided. In a similar manner it has long been customary to speak of a caking together of the hair in *plica polonica* by a viscous fluid, which was said to be secreted by the scalp, though the fact itself had never been demonstrated. What really had been observed was nothing more than that the hair is entangled with dust, scales of epidermis and such like into one dense mass, and from this an inference was drawn as to its cause—*i. e.*, the presence and action of that viscous fluid.

But it is evident that we may still more easily fall into error in our observations, and that in a more dangerous manner, if a want of knowledge, actual prejudice, peculiar tendencies, or even superstition on our part, are present. An individual in a state of so-called nervous excitement, or delirium, may regard his visions and sensations as objective, another his nightmares or hallucinations—*i. e.*, may attribute a real existence to them, external to his consciousness and whole world of sensations. He has, from infancy, been accustomed to this kind of connection, this dependence of his sentient perceptions upon external and actually existing objects. In the case of nervous excitement, or delirium, also, his sentient perception has, in itself, been so far quite correct that he has, perhaps, really seen frightful animals, persons who have long been dead, or even the devil in person, or has heard their voices, and it is only his conclusion as to their real existence, externally to his inward world of sensation and his imagination, which is false. Essentially in the same manner might even those errors of superstitious and visionary persons and demoniacs find an explanation, who perhaps assert in good earnest that they have seen ghosts, saints, the Madonna, or God himself, appear in person, and even have held formal conversations with them, and that, too, without any intention to deceive. Their mistake, doubtless, consisted only in their assuming a material object as existing externally to them, in the place of what they perceived merely subjectively, and now being sufficiently superstitious, stupid, or exalted, to take their conclusion, and their interpretation of it, for an actual perception.

Still more numerous are the circumstances and causes which may lead us into the other chief error in our observations, *viz.*, that of entirely overlooking certain other phenomena and things which really exist and are present. Yet such an error is facilitated, not only by the great complexity of all our phenomena, but also and still more by our being able to recognise *directly* only the smallest part of what is submitted to us for observation. From this cause we easily overlook one thing or the other, and perhaps even judge of what we have really noticed or observed, in a one-sided and imperfect, if not utterly wrong manner. Setting aside, here, negligence, want of knowledge of the subject, and other deficiencies on the part of individual observers, a certain prejudice, and the necessarily limited capabilities of the human mind in a study like that of medicine, may be regarded as the chief causes of our imperfect recognition of various points. Even in the observation of the simplest and most accessible things, we often see our comprehension of them impeded to an extent which will scarcely appear possible to future generations. Hippocrates was so far correct when he said that observers must be free from all prejudice and superstition. But where there is a want of comprehension and conviction, founded upon sure knowledge and upon positively adequate proofs, as is the case in medical science, it is difficult, if not impossible to remain free from all prejudice and superstition. And by attributing such errors of the observer simply to embarrassment, prejudice and the like, we do not gain nearly as much as we should do by a closer examination into the ways and circumstances, which may lead us into them.

But we are led into errors, not merely by the peculiar difficulties of our subject, but also and still more by certain directions and tendencies of our intellectual being, the more subtle motives and threads of which cannot always be traced with sufficient clearness. Some of these have been spoken of already in the preceding chapter. One of the most important causes, however, of our overlooking various things so easily, consists in this, that what we have really observed and noticed makes too strong an impression upon us and attracts our attention too much, to allow us, at the same time, to notice and give their due importance to many other things connected with it, or at least to have the will to do so. The more remote motives or circumstances, however, which lead us to this, may again be of very different kinds, and sometimes are more on the part of our understanding and judgment, sometimes more upon that of our impulses and volition, or absence of the latter.

It lies in the very nature of our subject that everything which we ourselves perceive in a direct manner, everything which makes a vivid impression upon our senses, and, by this means, upon our minds and consciousness, not only remains more strongly imprinted in our memory, but likewise more immediately, and, therefore, more forcibly influences our notions and judgment, than other things. We now, in general, readily, even during our observations, lay the greatest stress upon this—more, perhaps, than it deserves—and at the same time overlook many other things, because we happen to attach more importance to the first, and confide in its being of a more important character and decisive influence,—whether it be, for example, the moon and stars, storms and lightning, a superstitious remedy, images of saints, or, perhaps, some pretended specific. In every case it is something which is capable of acting directly upon our senses, whilst we are not able immediately to perceive others which, in the case at issue, are perhaps of at least equal importance. So, for instance, we see the patient himself attribute the greatest value in his cure to such medicines as he sees, tastes, smells, and swallows; and he is not always so wise as that old Greek, who, in the temple, inquired also after those who had been drowned in spite of their offerings. And, in the end, the natural philosopher is as little able to free himself from the influence of this law of our nature as the physician. All those things, however, which have once made a deep impression upon us, whether it be by their appearance and appreciable properties and peculiarities, or by their being novel and surprising, appear to us from the first, to be of especial importance, and, on that account already, we easily regard them as the essential causes of certain effects and events. Thus anatomical lesions, which are visible and palpable, and certain chemical changes and products, which we observe in patients or corpses, make a far deeper impression upon our senses than many other things, and it can hardly be doubted that all this does not remain without influence also upon our opinion as to their importance and the part they take in the aggregate of a disease. Exactly the same thing probably applies to all those phenomena and states which are perceptible to our senses in a direct manner, by the aid of auscultation or percussion, the speculum, the thermometer, or the scales, &c. But for the sake of what we have found out by these means we the more easily overlook and undervalue everything else, because that which we have perceived by our senses, and which alone, perhaps, is accessible to us, on that account, also, frequently appears to us to be most essential and determining. A further source of error, which is in many respects connected with the preceding one, is to be sought in the circumstance that, in general, all that is positive and affirmative makes a much deeper impression upon us than anything else, especially what is simply negative, and that for this reason alone the former more easily remains in our memory, and finally influences our judgment, than the latter. Every one will remember, both more easily and more willingly, such experiences and cases, for example, as speak for his opinion, and which are considered by him as further confirmations of it, than those which are contradictory to it; and just as, for this very reason, every one in his successes only too easily overlooks that which is unsuccessful, so the physician is often led by the favorable cases, which he has cured by his treatment, to overlook those in which he has failed to effect a cure. And if a physician has used any remedy throughout a long series of cases, and has seen every one of them recover, he will involuntarily regard it as an efficacious one, at all events more than another, who has not effected with it so many cures. The positive—*i. e.* the fact of the recovery or cure of his patients—makes its impression upon the former as usual, and, on that account, he will be predisposed to attribute an active part to his remedy, whether it may have played such a part in those cases or not.

It must here also be taken into consideration, that anything for which we happen to interest ourselves, from one cause or other, or which we are striving or wishing for, likewise appears to us as the most important; or else we should not have sought nor striven for it. But as this now especially, if not wholly attracts our attention to itself—*e. g.* some aspect of a function or disease—we only too easily overlook, for its sake, everything else, and perhaps, in the one point, see too much, see something everywhere because we have the desire of seeing it. And if personal interests or desires come into full play; if, for instance, we have an interest in seeing one thing and not another; if, by our observations, we wish to prove former opinions and assertions, or to oppose those of others, there is no end of errors, especially in a science such as medicine. If, in the present day, a Piorry finds large spleens where nobody else discovers them, and where, perhaps, none such are present, it is well known that during the middle ages, and long before the discovery of America, many always believed that they could see land to the west from the Canary Islands, merely because they wished to do so, and that without ever being really able to see it, simply because none existed there. To errors from this quarter, however, every one will succumb most easily, if he has to act at the same time, (as, for example, the physician at the bed side), and if, upon his judgment, and still more upon the judgment of others as to his services, well nigh his whole existence as well as his reputation depend. The physician is here much in the same position with regard to his judgment of his own measures and remedies as the teacher with regard to his method of education, and the statesman with regard to his regulations and laws. All of them frequently see and find precisely what they wish to see and find. They perhaps compare themselves to such technical men, for example, as operate upon inanimate and passive objects only, and overlook that they have to deal with entirely different things, with processes which are self-acting, and proceed according to their own peculiar laws. But as physicians so frequently wish to demonstrate the positive services of their remedies, the most abundant opportunity for error is furnished by the circumstance that their observation, and still more their interpretation of the state of things, only too easily become one-sided, if not entirely incorrect. And this is likewise essentially furthered by the denominations of our remedial agents. Remedies, for instance, which from the beginning have been placed before us and praised as antiphlogistics, astringents, &c., we are now accustomed to imagine as acting just in this way and in no other. We are now not only disposed to regard something to which we had previously attached the greatest importance—*e. g.* a given remedy—as the really determining one among all the circumstances and influences which precede or accompany a cure, but we likewise interpret all the changes taking place in the state of a patient in the precise manner which best corresponds to our expectations and views of the effect of that remedy. We now, perhaps, even perceive so-called antiphlogistic, astringent, or solvent effects from them, where none such occur, where they perhaps have acted quite otherwise, or where such changes or curative effects were owing to entirely different circumstances. In short—in this, as in other things—if we once are upon a false track in our interpretations and conclusions, we proceed further and further upon it, and everything is now made to serve as an additional confirmation of our views, even if, in reality, it should prove something quite different, perhaps the exact contrary.

Those obscurations which our observation and perception may suffer from everything novel, and from the prevailing tendency or fashion, are too well known for it to be necessary to enter into them more deeply in this place. And the liability to them in every science, and above all in medicine, where that which is already given to us seldom, in the end, makes us feel perfectly satisfied, and where we now expect from new directions, doctrines, and remedies, what the old ones were not able to accomplish, lies near enough at hand.

Those portions of the external world, of life, or of diseases, to which at the time the attention, perhaps the whole bent of the mind is directed, will now exercise a preponderating influence upon our mode of observation,—whether it be the so called imponderables, electricity, nervous force, certain anatomical and chemical points, or pure *dynamism* in these. Every one hopes and believes that *he* will best be able to solve the grand problem; and the discoverer is wont, even more than others, to attach importance to what he advances, and *must* do so frequently, if it be only for the purpose of attracting so much attention to it as it deserves. And history likewise teaches us how, in this point, one view, one theory followed upon the other, just as in medicine one "infallible" remedy upon the other; and every one of them was more or less believed in and followed, in its time, because it appeared to correspond to facts and experience. They appeared so, however, because the observer looked upon and interpreted things exactly as his belief or the prevailing views warranted. For instance, after the Emperor Augustus had been cured by his physician, Antonius Musa, by means of cold baths, everybody, and especially the higher classes, used cold baths, and believed that they experienced the most excellent effects from them; even a Seneca praises his baths and swimming exercises in the Calends of January. In the same manner, St. John's stimulating liniment was, in its time, considered as excellent a remedy for consumption as cod-liver oil, for instance, is at present; and although St. John himself died of that disease, his admirers nevertheless erected a monument to his memory. A similar kind of embarrassment and confusion is daily seen to arise from the circumstance that the observer is not able to keep himself free from the influence of antiquated notions and views, perhaps of actual prejudices, to the extent which would be requisite for the freedom and perfect correctness of his conception. In each case he does not stand upon an entirely objective ground, but under an authority, whether it be that of the ancient or the novel, and now finds everything exactly as this authority represents it.

He may now overlook the most accessible and apparently intelligible things and simplest relations, because he does not at all think of their possibility or probability, and this, because he has always imagined them quite otherwise, and never doubted the existence of certain other relations between them. Thus, for instance, the worthy Sir Thomas Brown, who, animated by the zeal for truth, himself wrote about Common Errors in the year 1646, was yet so thoroughly persuaded that the sun moved round our earth, that he could never hear the contrary opinion without attempting to render it ridiculous. No less was the belief of many chemists in phlogiston so firmly rooted, that it could only with difficulty be overthrown by a Lavoisier, a Berzelius, and their scales, so long as it was preferred to attribute the circumstance of burnt bodies becoming heavier to some mysterious property of phlogiston of making bodies lighter. That many errors in observation in our department of science have arisen from the same cause, and continue to do so daily, needs no proof here. These are essentially favoured, not only by the natural difficulty and obscurity of all our problems and objects, but also by the circumstance, that a man must be a specialist in order to be able to observe *anything* in a sufficiently accurate and correct manner. But specialists, again, readily conceive things in a one-sided, or, at least, not in a sufficiently unbiassed and independent manner; they easily overlook many things because they frequently, for the sake of views and theories once adopted, look only in one direction, to one quarter. Even our expressions and denominations, as has been explained already, exert the same influence, the same authority. We do not readily adopt new views concerning remedies, for instance, which have long been considered as antiphlogistics, tonics, &c. Thus, we assume that we have subdued an inflammation by some remedy or other, because it is said to act antiphlogistically, and it is said to act antiphlogistically because it happens to be considered an "antiphlogistic."

III. ERRORS IN THE COMPARISON AND DEDUCTION OF GENERAL PRINCIPLES, ESPECIALLY IN RELATION TO THE CAUSAL CONNECTION OF CERTAIN THINGS.

These errors even much more than the others, are, in the end, connected with each other; and since, at the same time, their name is legion, an allusion to the most important of them, and especially to their sources, may suffice in this place.

If we inquire, however, how it is that we may so readily fall into such errors nearly at every instant, one circumstance shows itself as the chief source in all cases, which lies in our whole nature, and in a certain instinctive impulse and requirement of man. For all of us are apparently more or less inclined—frequently even compelled—to pronounce a judgment, perhaps to make some general assertion in regard to one thing or another, which we understand but half, or not at all, and consequently to conclude, explain, or assert by far more than we can correctly do—*i. e.*, than our experience or our knowledge warrants. Unfortunately, also, the temptation to such a venture appears nowhere greater than precisely where the greatest danger exists—namely, where our knowledge least enables us to declare anything with certainty as to the causes and connection of various things. Now, whether it be a justifiable thirst for knowledge, the exigency of the moment, or pure vanity and thoughtless zeal, &c., which urges us to do so, our imagination and arbitrary combination always lead us further than our insight or proofs will reach, and we overleap what we cannot accomplish.

But opportunity for such errors is, of necessity, hourly afforded by the manner in which we arrive at an opinion in general, at a judgment of things which are only half known, or entirely problematical. For here nothing remains for us, at present, but to draw conclusions as to their conditions and the whole state of things in them, from those of others of which we already possess more knowledge or experience. What we have thus found to be more or less true in individual cases (for instance, in regard to a disease, or in experimenting upon the mode of action of some influence, or upon the processes going on in a function), we not only extend to all cases, processes, &c., of the same kind, but likewise to such series and species of things as appear to us to agree more or less with the former in certain essential points. In other words, we generalize that which we have found in the former, and which, with regard to them only, is proved with more or less certainty. But so soon as the latter, which we have placed under the same category, and assumed to be essentially the same as certain others, have been, as so frequently happens, only partially known to us, we extend those general principles and assertions much further than we are justified in doing. For we compare things with each other, and even declare them to be essentially the same, without by any means having become sufficiently acquainted with them, and without, perhaps, having compared, much less determined, all the points and circumstances connected with them which ought to be carefully considered. It is manifest, however, that in a department of science like ours, in which so little has been clearly and comprehensively investigated and become known, especially in relation to causal connection, hardly anything of the above description can be asserted positively. On the contrary, we are always in danger of extending that which we have ascertained in regard to certain cases, and which is but imperfectly determined even for these, to others, to which perhaps it does not apply at all, or where it is at least doubtful whether and to what extent it may be applicable. Not even statistics, one of our best auxiliaries in this respect, can protect us against such errors, because here, also, cases and things are so readily and frequently brought under the same denomination, as being of essentially the same kind, as unities, which, on account of their differences, are not at all adapted to being classed with each other. This applies, also, to analyses of the blood and urine, even in patients suffering from the same disease, so soon as they are made by different chemists; and much more to observations or cases of a given disease, to histories of cures, &c., because in these, least of all, can we assume their agreement in all the essential points, especially in those with which we are still unacquainted. In all these cases the possibility exists that our classifications or comparisons are only partly and approximately correct, or perhaps even totally wrong.

But this likewise shows that by far the greater number of general assertions and theories in our department of science *may*, at least, be wrong; that even, at best, they can only be regarded as more or less probable, and far more frequently as half true or premature, whether it be that we assert anything positively by them, or whether we deny anything generally, and perhaps unhesitatingly declare it to be impossible. For so soon as we make any assertions of this kind concerning things, processes, &c., which we have, at best, become but partially acquainted with, and the internal connection and laws of which have remained more or less obscure, our general assertions in regard to them are always premature, and more likely to be erroneous, in one way or another, than perfectly correct.

It may, indeed, already be advanced that many of them *must* be wrong—namely, whenever anything is asserted which never can be demonstrated from experience, because the things in regard to which those assertions are made are beyond the reach of all observation and of all certain proofs. Nevertheless, such assertions are daily made, not only in common life, but even by men of science; for instance, if it were said that a given individual had never, during his whole life, uttered anything but the truth, had never been afraid, or never dreamt, &c., this would be to assert more than can ever be proved, if it were only because even the person concerned cannot, in after-life, remember everything with perfect accuracy which happened to him when he was a child or a boy. It is much the same if we say that everything occurs in the living body according to laws altogether peculiar and different from those of so-called inanimate nature, or, as we frequently express it, that entirely different forces are in operation in the one case and in the other. For we have not yet even become perfectly familiar with the phenomena and processes in inanimate nature—*e. g.*, in the atmosphere, the soil, &c.—much less with all their conditions and laws. And even if our knowledge of them should become greatly increased in future, it may yet be foreseen that it will never be sufficient to enable us to say that we know and understand them all, so that, even under the most favorable circumstances, we shall never be even in a position to demonstrate the existence of such forces, &c. The same would be the case if we should declare anything in nature, whether animate or inanimate, of which we can only acquire a knowledge from experience, to be absolutely impossible, merely because we had never observed it; or if, for the same reason, we were to assert that it could never take place in a given manner, even under entirely different and novel circumstances, for we can never be assured that conditions and circumstances may not come into action somewhere or somehow which may bring about a result never observed before. And just because no proposition merely founded upon experience, no empirical law, in itself, can be considered as an actual law of nature or causation, we easily fall into error in all general assertions of the above kind.

It is likewise evident that this will apply to everything going on in living bodies, whether healthy or diseased, which is an object of our observation or research in general, and finally of our assertions, so soon as it does not simply relate to space, weight, number, &c. We do not yet know nearly enough of the peculiar conditions and internal mechanism of all those processes, to be able to venture upon such a generalization with even approximate certainty.

Although such a venture always presupposes an illogical over-estimation of our knowledge, it is precisely those the horizon of whose knowledge is the most confined who are wont to express themselves most decidedly. Aristotle says: "Qui respiciunt ad pauca, de facili pronunciant." We daily, for instance, hear physicians affirm that a certain state of things always obtains, and that certain symptoms are always present in persons suffering from a given disease; that a given disease absolutely indicates a particular kind of treatment; that certain remedies have always rendered the most positive services in it, and other similar general assertions. But alas! our knowledge of all the points thus pronounced upon is, in reality, much too slight to enable us to make any assertions of this kind. With most physiologists, it is an axiom that nothing can pass, except in a state of solution, from the intestinal canal into the blood; and yet they have made scarcely any experiments on the subject up to the present day, but rather conclude that it is so from certain analogies and probable reasons, which for the present, and as such, do not prove anything with certainty. Here, as in the case of any other phenomenon or process in the living body, the conditions and conformity to fixed laws are not yet sufficiently established to enable us to say that any other state of things but the one assumed by us is absolutely impossible. We are apt to regard simple observations, isolated facts, or so-called empirical laws, as scientific explanations and real laws; and the mere knowledge of the fact that certain things perhaps always occur at the same time or successively is now considered as an understanding of their causal connection and peculiar conditions.

Another circumstance intimately connected with the above is, that we easily fall into error whenever we seek, for instance, to form a conclusion from the state of one function, or of one patient, as to that of other processes or states of disease, which perhaps resemble these in one way or another, or even as to their state under circumstances almost entirely unknown to us. But, in reality, the individual observer only too readily arrives at the belief that what has been observed and experienced by him must always take place precisely in the same manner, and hold good for all times and circumstances. But since all that which the physiologist or the physician knows concerning the occurrences and processes which he is seeking to investigate, for the present consists rather in a knowledge of their occurring together or in succession, and manifesting themselves in a certain manner, than of an insight into their proper conditions and laws, the individual observer can infer nothing more from all his observations and investigations than that, under given circumstances, he has seen certain things always occurring simultaneously or in succession, and running a certain course. He is liable to error, therefore, whenever he extends his experience to things (such, for instance, as organisms, functions, cases of disease, effects of medicine) which have neither been observed and investigated directly by himself nor others. Everybody, for instance, is aware how erroneous it would be for an individual to conclude, from the mere circumstance of his having observed ulcerations of the intestines in all the cases of typhus fever which he met with, that they are present in all cases of that disease, at all periods, and in all countries; or that, because a hundred years ago a certain per-centage of the inhabitants of a given town, country, or hospital, died yearly, an equal number must likewise die in the present day. And it would be no less erroneous to assert that the state of lepers and plague-stricken persons could not have been such as the ancients have described, merely because we see it to be quite different now; or to conclude that, because cases of intermittent fever, for example, are generally speedily and safely cured in Germany by bark, the same would be the case in the Campagna of Rome, or in New Orleans, &c. But everyone may be taught by his own experience how prone we are to make such general, and on that very account hazardous, if not erroneous, assertions; how easily the insignificant "I" is converted into the abstract "one" and "it;" for instance, instead of saying, "I have observed a certain thing, I believe it to be true, I take this or that view of it," we are wont to say, "It is a matter of experience, it has always been observed, it is the object, the indication, &c."

In the present state of our knowledge we run the same risk, even in the most simple comparisons and analogies, of shooting beyond the mark, and, under the most favorable circumstances, can add but little to our knowledge by such means—at least nothing certain. Nothing prevents us, it is true, from comparing various things with each other; neither could we, as has been shown already, free ourselves from this necessity even if we wished. The reasons which, for the present, render it difficult, if not impossible, for us to ascertain anything positive by such means, have likewise been explained. From the circumstance of certain things or processes (for example, some function, disease, effect of medicines) being similar, perhaps even identical, in one or more points, in certain properties, &c., we conclude that they must likewise agree in other points and properties, and this, too, precisely in the essential ones, which, at the utmost, we have only discovered in some of them, not in all. The proof of this, however, we are not able to give, because our knowledge, for the present, rarely or never enables us to form a certain judgment whether those properties or points which we have observed in the things compared together by us stand in such an essential connection with their other properties, &c., that the existence of the latter can be inferred from that of the former. In short, as matters happen to stand, our analogies and comparisons rather deserve the name of uncertain allegories, pictures, or metaphors; and often enough we do not fare much better than Captain Fluellen, in Shakespeare's Henry V, who insisted upon comparing his Harry with Alexander of Macedonia. And if we are not even able to compare together with some degree of accuracy various functions, diseases, effects of medicines, &c., in regard to their external appearances or symptoms, we are much less able to draw any safe conclusion from their agreement in certain points, as to the identity of their causal connection, conditions, and laws of action.

All this, however, suffices to explain the uncertainty, if not utter incorrectness, of all our comparisons. If, for instance, we unhesitatingly compare the processes and laws of respiration with those of combustion, or the functions of the nerves, and their laws, with those of electricity; or if, from the similarity alone of certain anatomical lesions in several diseases, we conclude their essential identity, it is just the same thing as if any one were to conclude that gentian, from the fact of its tasting bitter, like cinchona, must likewise contain certain alkaloids, or cure intermittent fever with equal certainty. We meet with comparisons of such calibre and tenor at every step, especially in medicine; for instance, from certain processes in wounds and fractures which are more or less analogous to inflammation, implying a favorable result in regard to their cure, some have inferred the usefulness or curative tendency of inflammation in general. Because astringent substances containing tannin had an astringent taste, and may really be of use in external hemorrhages, suppurations, &c., by coagulating albumen, they were likewise thought to be of use in cases of internal hemorrhages, hæmoptysis, and of purulent expectoration, as well as in diarrhœa and diaphoresis. And because the iodide of potassium dissolves certain combinations of quicksilver or lead with albumen in the test-tube, it is likewise said to dissolve similar combinations of these metals with the albuminous principles of the body, and so to effect perfect cures in cachexia induced by either of them (Dorvault).

These examples already prove how an erroneous employment of comparisons and analogies, especially in our department of science, almost necessarily follows so soon as we endeavour to explain a series of phenomena, processes, or effects, or to build up connected theories and systems. For in this way we are compelled to compare all these series of things according to certain analogous points, and to infer their essential similarity, or even identity (in regard to their connection), from their conformity in the few points which we can appreciate, whilst we are by no means in possession of any certain judgment whether and to what extent the points or circumstances in which they agree are in reality the essential ones or not.

This shows the intimate concatenation in which these errors and defects in comparison and judgment generally stand in regard to our classification and its peculiar imperfections and faults—for instance, in the department of pathology and materia medica. And as such a defective classification is itself the inevitable consequence of our imperfect discernment in regard to the things to be classified, so their defective, or wrong classification necessarily reacts in a troublesome manner upon our whole discernment, our judgment of the objects which have been grouped together, and thereby likewise upon their comparison, as well as upon all our assertions about them. It has been adverted to already upon a former occasion, how things are classed together under one and the same head, the exact agreement of which in the more essential points is frequently very doubtful and at all events rarely proved, because all of them are more or less problematic to us in their intrinsic conformity, conditions and effects. And yet the mere fact that certain diseases, remedies, &c., have been brought in one way or other under the same head of classification, naturally gives rise to the idea, that they will likewise agree in all their more essential points. We are therefore further tempted, at every instant, to make different general assertions at different times about them, which, for the above reasons, however, are for the most part highly arbitrary and uncertain, if not decidedly erroneous. A single glance at any attempt at classification in our department will prove this.

Such errors are no less essentially promoted by our technical language, and terminology, by its abundance of mere figures and metaphors, and of terms either ambiguous or nearly meaningless and decidedly false. It is said often, indeed, "in verbis simus leves;" yet the history of medicine, in particular, shows how dangerous and injurious the daily employment of words without a meaning, at least without a clear and definite one, may become. It is likewise sufficiently well known what frequent opportunities for a thousand errors are afforded by such terms, for instance, as *irritation, excitation, antagonism, metastasis, tone, reaction,* &c., and how, from our ontological names of diseases, the real state of things may easily be overlooked, and the clear and correct understanding of the states thus denominated disturbed. Similar errors are wont to arise from the double meaning, the ambiguity of so many of our terms (for example, *consumption, nervous fever, plethora, stagnation* or *stasis, lymphatic,* or to *strengthen,* to *astringe,* to *dissolve,* &c.), because we are so easily led to conclude, that what is applicable in one case—*i. e.,* taking a term in a peculiar meaning—would likewise be applicable in another, in which the same term is applied in an entirely different sense.

All this shows that the most important element in all our errors of the above kind (in our comparisons, &c.) is, that we compare certain processes, influences, and other things, with each other, of which the more essential points and parts are more or less unknown to us. And the common cause of the comparisons or classifications proving uncertain, if not decidedly false is, in the end, that we look upon certain properties and circumstances, which appertain equally to both, as essential or determining ones without their being so, or at least without their being proved to be such. But since our endeavours are directed to the explanation of all those processes, effects, &c., and to giving ourselves and others a certain account of them, we lay stress upon some single ones out of the whole aggregate of simultaneous or successive phenomena and circumstances as being the essential and determining ones. And because the latter have rarely or never become accurately known to us, because in our judgment about them we necessarily proceed in a more or less abitrary and *a priori* manner, we easily fall into error, and perhaps look upon subordinate, secondary circumstances as determining or essential ones, and still more frequently regard individual fragments or parts as the whole. Comparisons and general assertions of the above kind always, therefore, involve the erroneous notion, that we have already become acquainted with the real conditions of the process or event in question, and with the laws of action of the influences which affect it, and that we are thus capable of forming a judgment about it, whilst in reality we frequently do not even know all the modifying and co-operating circumstances, much less their real influence and mode of action. All we know rather consists merely in this, that certain circumstances, influences, and effects, certain events and phenomena, always occur simultaneously or successively.

Upon the same circumstance depends the well-known kind of error in our judgment or conclusion, which has long been expressed by *post* (*cum*) *hoc propter hoc*. For here the defect consists in our not being able to assign any other cause for our conclusion and assertion that certain circumstances are the real conditions of certain events or effects, than that those circumstances had preceded the event or effect in question—had been observed before, or at the moment of its occurrence. But there can hardly be any attempt made, at present, to explain a single process in the field of life, whether in health or disease, *i. e.*, to show its causes, without this error occurring more or less. For every one who undertakes this, asserts, in the end, that a given agent or element, (for example of a function, disease, cure, &c.,) was the really determining and essential one, without for the present being able to give better reasons for his assertion than the fact, that those agents or circumstances had *always* been observed in connection with the event or process in question. Perhaps too, in the most favorable case, certain other grounds of probability might speak for such an influence of the circumstances in question. The physiologist, for instance, falls into the above-mentioned error if he summarily ascribes to the bile an essential part in digestion, or if he assumes the higher oxidation of certain parts of the blood during respiration to be the essential condition of the latter; if he declares the atmospheric air to be the only indispensable agent of all respiration, or the heart that of all circulation, or looks upon the formation of infusoria, fungi, and such like, as the cause of all processes of putrefaction and fermentation, and even of many diseases, (especially such as are contagious and miasmatic). But that pathology and therapeutics, in particular, well nigh in all their attempts at explanation, fall into an error of this kind, is evident at the first glance, whether such an attempt may belong to the department of ætiology and pathogeny, or of the treatment of disease. Nearly every day brings forth explanations, such for example, as that cretinism, scrofulosis, and other endemic diseases are caused by the water which is drunk and the lime it contains, or by the deficiency of light in a valley, or by its marshy soil, by certain winds, perhaps even by some article of food. In the same manner, some select from the whole aggregate of circumstances and influences which have, in many cases, preceded the occurrence of pulmonary consumption, scrofulosis, and the like, the single fact, that the parents and relations likewise suffered from the same disease, and the affection is now attributed to an inheritance of pulmonary consumption, scrofulosis, &c., and, under certain circumstances, even to infection by it. And if any one should attribute the nostalgia of the Swiss to the denser and heavier air of the plains, or scurvy to the use of salt food, it is much the same as if one national economist were to attribute the commerce and industry of Great Britain to its abundance of coals, another to its insular position, and a third to the circumstance of its having a rich aristocracy. And those who at one time regard some disturbance of the functions of the nervous system, at another some "crasis" of the blood, or certain other anatomical changes and conditions, not only as what they are, namely as single elements or parts of the whole disease, but as the essential and properly determining points of it, fall, essentially, into the same error. If even the father of pathological anatomy, Morgagni, has given his work the title "de sedibus et causis morborum," others, who see, for example, in the whole of a disease only certain appearances or processes in various organs, or even in single tissues, who judge of its course and danger well nigh only according to the local lesions attending it, and declare that one person died of the ulcerations he had in his intestines or lungs, another of disease of the heart or softening of the brain, err in a similar manner. That we do this, however, nearly in every conclusion regarding the cure of our patients by medicines and other remedial measures has already been complained of by Zimmermann. We conclude, for instance, that the breaking out of an illness has been prevented by prophylactic measures, or that a disease has been cured by certain remedies, simply because these have been employed. But whenever we have no other more valid reasons for this conclusion, than the fact of their employment and of the consequent recovery of a certain number of patients, it is a mere conclusion by *post* (*cum*) *hoc propter hoc,* unless it had previously been shown that such patients, under other circumstances, and left to themselves, are not wont to recover with equal rapidity and frequency. This also explains why the faith in so many remedies is giving way more and more, and why so many of them are now thought totally ineffective, to which the best cures were formerly attributed; not so much because our scepticism, in itself, has become greater, but rather because our manner of observing and forming conclusions has become more circumspect, and because it has been proved that most patients recover just as well without any of those remedies. Essentially the same kind of error is likewise at the bottom of the well-known doctrines of a so-called antagonism of certain diseases, of crases of the blood, dyscrasias, &c. Leaving out of the question that the ontological allegorical conception of diseases as something self acting and having an independent existence is perhaps carried further than is compatible with our present notions and knowledge, the assertion that the mere presence, or previous existence, of a certain disease, or disposition to a disease, &c., has prevented the development of certain other diseases—*e. g.*, that a cancer-cachexia or intermittent fever has prevented the development of tubercles and pulmonary consumption—must be regarded as a premature and even false conclusion. For at all events a number of other circumstances and influences have constantly and in all cases been in operation which likewise might have had a share in determining the result in question, the influence of which, however, has hitherto remained just as little known. In the end, therefore, nothing more certain is known, than that people who are attacked by cancer and die of it, do not likewise fall ill and die of pulmonary consumption, and *vice versâ*. And in so far this manner of forming a conclusion does not essentially differ from the homœopathic doctrine of antagonism, according to which a disease is said to be cured by a more or less similar one having been artificially established by some remedy or other. The substance of this doctrine, however, was sufficiently well known to the sorcerers and witches of the middle ages. If, for instance, an antagonist, an enemy, was to be tortured, poisoned, or entirely done away with, they formed an image of him in wax, clay, &c., and treated this with poison, the dagger, or fire. Yea in the old Germanic law the case was provided against that in this manner a witch might swallow a free man with skin, hair, and all, and disgorge him again.

From all the foregoing it might be inferred, that every conclusion which we seek to draw in our department in regard to the causal connection of any process or event, and especially every assertion in regard to the conditions and actual development of a function, a disease, or its cure, will suffer more or less from the common error of this *post* (*cum*) *hoc propter hoc*. Proofs for all these conclusions, such as science must demand, we are for the present incapable of furnishing, and in so far they must be considered as more or less hasty and uncertain, if not as decidedly erroneous. Therefore we may always doubt and attack them again, and even, as our history and the experience of every day teach us, may assert the exact contrary, and, to a certain extent or in a certain sense, be justified in so doing.

It is not necessary to explain further, in this place, by what means alone a conclusion of the above kind, which is perhaps erroneous, or at least very doubtful, may be made to gain in credibility and truth. This would evidently be the case only if we should succeed, upon entirely different grounds, by further and special observation, in furnishing the proof, that those circumstances and influences, which have been assumed as determining or effective, really possess the capability of producing the effects attributed to them. And so long as this has not been determined, every conclusion in regard to their causal connection will be a doubtful one, even in the most fortunate case, because it is only based upon the simple empirical fact of the simultaneous or successive occurrence of certain circumstances and effects. We may already, for instance, conclude or assert with certainty, that if a person, after partaking of food with which a poison, such as arsenic, copper, &c., has been mixed, is taken ill in a certain manner, the poison and not the food must be considered as the cause of his illness. And if we once know that the property of rendering arsenic ineffective belongs to a certain combination of iron only, and that no one who is poisoned by it will die, provided that preparation of iron be employed in the proper manner and early enough, we may safely attribute the cure of the person poisoned by arsenic to that preparation of iron, even if, at the same time, entirely different remedies or circumstances were acting upon him. In the same manner, for instance, we justly regard cinchona (quinine) as a remedy for intermittent fever, quicksilver for syphilis, iodine for bronchocele, opium for pain, &c., because special experiences have shown, that such patients, left to themselves, or treated with other remedies, do not recover by far so frequently or speedily. In short, it is only by such special proof of the mode of action and influence of the individual agents or circumstances in the given case, whether it be furnished by experience or scientific research, that our fallacious manner of concluding *cum* or *post hoc* can be converted into the alone correct one of *propter hoc*. So long as this has not been done, everyone is in a manner free to consider sometimes one thing and sometimes another as the cause of the event in question, for instance of a disease or cure—and this, as may be foreseen, will frequently lead to error. The theorist will find the greatest room for this error in the ætiological chapter of our pathology, as well as in medicine itself. Whilst, for instance, the pathologist at one time selects for himself the functions of the nerves, at another the blood and its changes, or local anatomical changes, as the starting point and essence of all diseases, the therapeutist may, if he chooses, with equal right ascribe the cure of thousands of his patients to innocent drugs, to homœopathic or even sympathetic remedies. Thus we are in danger of confounding cause and effect together if we assert that grief may give rise to disease of the stomach and even to cancer of that organ; that the habit of sleeping after dinner, the *siesta*, causes obesity, congestion of the brain, and apoplexy; or that cancer of the uterus is produced by the cessation of the menses in the climateric years.

And since, as has been explained on a former occasion, it is not always quite clear to us, what it is exactly that we wish to prove, and that must be proved, and still less, in what manner alone it might be proved, a further source of an entire series of errors in our conclusions and assertions, as well as in our attempts at explanation in general, exists, which we are wont to comprise under the name of tautologies, arguments in a circle, and *petitiones principii*. Whilst, in the one case, we give a mere indirect description of the occurrence to be explained, and which we believe ourselves to have explained thereby, (for instance a disease, or the effect of some remedy,) whereas we are only describing it with other words, in the other case, we accept something as a proof of the course and connection of what is going on in a function, a disease, or a cure, which would itself first require an explanation.

However gross such an error may appear, and however easy it may seem to avoid it, the latter, at least, is not the case, and in our department of science everything, from the imperfection of our knowledge to the defects and want of precision of our terminology, coincides to favour such errors.

An old well-known example of this is furnished by the doctrine that "opium acts narcotically and hypnotically, because it possesses a narcotic and hypnotic power" (Molière), and everybody must be aware that many attempts at explanation—*e. g.*, in regard to a disease, or the effects of a remedy—are, in the end, no better. Many a one, for instance, believes that he has given a sufficiently satisfactory explanation of the origin of intermittent fever and miasmatic diseases by assuming that, in certain localities, "marsh miasmata" or "malaria" are evolved, and that miasmatic diseases are now generated because the people have been poisoned by a malaria. Thus we likewise very rarely have a better reason for assuming the so-called strengthening, tonic, or solvent effects of certain remedies upon our patients, than the circumstance that those remedies have the name of tonics, or solvents, and are considered as such. Errors of this kind, however, are very likely to occur so soon as we see ourselves compelled, not to deduce the causal connection and conditions of an event (for instance of a function, a disease, or a cure), from the determined laws of action of all the circumstances and influences acting upon it, but rather to infer them casually and superficially from other more or less uncertain conclusions, perhaps even from purely hypothetical notions on the subject. There is, however, no other more correct mode of proceeding at the disposal of one who wishes to explain anything in medical science, or who attempts to give his categorical judgment about the influence of any agent, whether deleterious or beneficial, upon the living body, or about any process in the latter. For he at once sees himself compelled to draw together his contingent of reasons from all quarters, without always examining into their trustworthiness in such a manner as would be necessary; and now he is easily induced to take the most doubtful conclusions as proofs of others, and in this manner to explain one unknown quantity by another, as, for instance, in the above case of the effect of opium. If we once assume as correct, what has been asserted at the end of the sentence (for instance, that opium has a narcotic effect) we are now easily led to infer or explain the anterior part of the sentence from it (for instance, that opium produces sleep), and now again conclude that the end of the sentence itself must be correct, because that which had been explained and inferred from it, or which had been expressed in the anterior part of the sentence, was an undoubted fact.

As, in our inability to furnish positive demonstrations, we are so fond of flattering ourselves with the notion, or rather of cheating ourselves into the belief, that we had already discovered a causal connection for various things, we now infer from it what ought, first of all, to have been proved before we could be justified in accepting it as the cause or proof of what is asserted in the anterior part of the sentence. Thus, for instance, we conclude that a so-called antiphlogistic, resolvent remedy must certainly act antiphlogistically or resolvently because it is an antiphlogistic, resolvent remedy. Intermittent fever and other miasmatic diseases are assumed to be generated by marsh-miasmata, &c., because in marshy districts, the noxious influence of which is undoubted, miasmata and malaria are evolved, which have such an injurious effect upon men. In patients affected with typhus fever or dysenteric disease, or suffering from pneumonia, &c., the entire disease, its course, malignity, and result, are assumed to depend essentially upon the local lesions—in the former of the intestinal canal, in the latter of the lungs—for these are regarded as the seat, the starting-point, and the properly determining or essential part of the whole disease.

Exactly the same error, in the end, underlies every attempt to deduce or to explain the mode of action of our remedies in a disease from the causes or the nature of the latter, in short, from the general state of things and from the intrinsic connection of the disease itself, which are likewise unknown to us. It is inferred, for instance, that venesection, nitre, quicksilver, &c., are beneficial in pneumonia, or steel in chlorosis, from their causing some change in the blood and its composition, and bringing it back to its normal state; an abnormal composition of the blood, in one way or other, being assumed as the determining and essential cause of the diseases in question. Iodine, salts, &c., are said to act upon the "lymphatic system" in scrofulous patients, because it is the seat and essence of scrofulous disease. But it is even more erroneous and rash to form a conclusion from the real or imagined success of a remedy, or mode of treatment in a disease, as to the intrinsic nature of the latter itself, as for instance the old doctrine of Hippocrates—"Naturam morborum ostendit curatio," recommends. In this manner, perhaps, A. infers the catarrhal nature of hooping cough from the services which he has derived, or at least thinks he has derived from hydrochlorate of ammonia, emetics, and such like; B. its spasmodic nature, or a neurosis of the pneumogastric nerves, from the beneficial effects of valerian or musk. And his hypothesis, that these or other diseases are no more than metastases of scabies, another perhaps endeavours to uphold by the precipitate conclusion that certain remedies (for instance tartar-emetic ointment, vapour-baths, &c.) are only of use in those diseases by re-establishing a kind of eruption upon the skin resembling scabies. But what opinion should we have of the logic of a physicist, who, after evolving certain gases, or mere watery vapour, or heating his furnace, not only believed that he had exercised a positive influence upon the weather or other processes in the atmosphere, but also attempted to explain the causal connection and laws of these from the only half-understood properties and modes of action of those his gases, &c.

We are the more likely to be tempted into such an error in our conclusions if a certain explanation of a process, (for instance of a disease, or a cure), or our conjecture in regard to a certain causal connection in it, happens to be correct, or at least sufficiently probable for certain cases. For that which is applicable to these, we now with so much the more confidence extend to other cases, for instance to certain other series of diseased conditions, cures, &c., which appear to us to agree with the former in the main points; and so soon as the latter does not prove to be the case, we assume that our previous conclusion is a false one. Thus, after the sudden disappearance of scabies, certain affections, such as palpitation of the heart, dyspnœa, neuralgia, &c., may have arisen, which we might, if we pleased, interpret as so-called "metastases of scabies." But if we once take such cases as proved metastases of scabies, we are easily led so far as to extend the connection which, in the former, was perhaps assumed with some probability, to entirely different diseases, likewise occurring in patients who had previously been suffering from scabies and had been cured of it, for example, even to cancer, medullary sarcoma, *tumor albus*, &c. What might possibly be applicable in the one case is likewise believed to be so here, where this kind of causal connection is infinitely more doubtful, *i. e.*, where it has not even been proved, whether scabies and its cure might have had any part whatever in the origin of those diseases; and thus a complete scabies-group might be constructed, as, for instance, the well-known one of Autenrieth.

Connected with this is the fact, that in our judgment and conclusions in a given case we often readily start from certain general doctrines or abstract truisms, or even from commonplace notions, and take them as proofs for those conclusions of ours, without having previously determined whether they are exactly applicable to the case at issue. Our error, therefore, consists in our regarding something as absolutely and always true or valid, whilst it, perhaps, merely applies to certain cases and to a certain ensemble of circumstances, which, in the case in question, are not all present, and where, therefore, our conclusion from it must become a false one. And how frequently conclusions of this kind are drawn in practice, is sufficiently well known. Thus it is said that cold is an agent hostile to life, and that it must, therefore, act injuriously upon various persons. And he who is once persuaded of the absolutely injurious effects of quicksilver in syphilitic patients, will easily attribute all casualties which may happen to such patients to this remedy; while he who, on the contrary, looks upon it as the only remedy for syphilis, will likewise believe it to be indicated, even where it may be injurious. In both cases, however, the conclusion is not more correct than if any one were to infer from the fact, that only a given per centage out of a thousand births arrive at a certain age, that a certain A. or B. would likewise attain this age, and neither die sooner nor later.

A further circumstance by which this kind of error is essentially promoted, is to be found in the obscure and especially in the ontological mode of contemplation in medical science, as well as in the ambiguity of our technical language and its Latin phraseology. For it is precisely behind learned expressions of this kind that defects, if not decided errors of our understanding and of our whole train of reasoning, are hidden. The interpretation of a given case is now perhaps expressed by a general abstract term or sentence, which is by no means an explanation, but merely a kind of indirect description of the case in question, therefore by words and expressions, which, in the end, only state in another form the fact of this event and the existence of the things to be explained. We readily, however, accept them as a kind of interpretation, because we are misled by the tenor of the words themselves. If, for example, diseases, such as dysentery, cholera, typhus, make their appearance as epidemics, the physician naturally wishes to explain their occurrence, though he is, indeed, for the most part unable to do so. But instead of expressing his error, his reasoning in a circle, in clear words, and of saying, for instance, that those diseases appear epidemically because it happens to be their character and nature to do so, he perhaps says that they prevail epidemically at present, because the epidemic *genius morborum*, the epidemic constitution of this year, or of this district, happen to be of this kind. In a similar manner the fact, that children of scrofulous, or syphilitic parents are likewise frequently scrofulous and syphilitic, is sought to be explained by saying that a hereditary disposition to those diseases was innate in them. And if various influences or remedies affect certain persons in a peculiar manner, this is held to be dependent upon their constitution, temperament, &c. In the same manner it is said, that certain functions, certain excretory processes, are connected with each other, because they stand in sympathetic relation to each other; that cantharides act irritatingly upon the urinary organs, and that quicksilver acts upon the salivary glands, because they stand in a specific relation to those organs. Again it is asserted that consumptive patients do not suffer from cancer, and that pulmonary consumption and tubercular disease in general are very rare, or entirely absent, in miasmatic districts, because an antagonism exists between those diseases.

Finally, daily experience teaches how we are most easily led into the gravest of these errors in our reasoning and attempts at explanation, if we follow any decided tendency therein, or if we seek to demonstrate the correctness of a previously-conceived notion, and this especially during a contest with opponents—perhaps pressed by their cogent objections. Into this difficulty all of us, in the end, are thrown easily enough, even the most calm and clear thinkers, because we all believe so many things without having real and valid reasons for doing so, frequently even without knowing, or at least without remembering, how we acquired this belief, these conceptions and notions. In the beginning we perhaps yielded to the natural desire of looking upon those circumstances and points, under which we just happened to have observed the event or effect in question, (for instance, a disease or a cure,) as being also the causes of these. We have perhaps arranged and explained everything according to this our point of view, and have leisurely formed further combinations, built up further conclusions upon previous hypotheses, without clearly enough recognizing their prematureness or one-sidedness. It is only, perhaps, during a contest with an opponent or in the presence of an opposite opinion that we find ourselves induced to look round for real reasons and more valid proofs for our assertions and previous attempts at explanation. But as we have brought forward the latter without having previously examined into the real state of things more accurately, and still more without having become acquainted with it in a sufficiently comprehensive and correct manner, we, in the end, commit one logical error after the other in our reasoning.

Proofs enough of this are furnished by every controversy and polemic treatise, and nowhere to such an extent as in medicine—with the exception, perhaps, of politics and the daily press.

CONCLUSION.

As it is the province of scientific history to trace up the growth and expansion of thought, and to track, step by step, the course through which our knowledge of natural subjects has attained to its present development, so it is the task of logic to decide upon the final establishment of truth, through the accurate adjustment and balancing of the particulars which history furnishes.

Now in bringing our labours in this particular department to a close, a few questions such as these suggest themselves. To what point have the increased resources and superior enlightenment of the present day brought us; to what further acquisitions may our hopes and endeavours reasonably tend; and by what means may we most speedily and safely press forward upon the path of progress?

It has been explained already how the art of healing, passing from the simple empiricism of its first attempts to alleviate human suffering, became mixed up with the superstitious practices and mystical ceremonies of the existing age and nation. And the stamp of this its origin and infancy has, to some extent, remained impressed upon it even to the present day, in an adherence to traditionary maxims, a proneness to indulge in rash hypotheses, and a deficiency in an accurate knowledge of the subjects with which, as a science and an art, it is concerned. How often, even in the present day, do we see medicine in the dark both as to the object it would achieve and as to the means by which it would achieve it. As an art, it too seldom attains to a clear insight into the processes required for the accomplishment of its proposed objects, and failing this, we see it attempting to carry them out through a series of arbitrary and self-imposed rules. It now acts upon the results of simple experience, relying solely upon the indication *a juvantibus et nocentibus*, without any clear discernment of the processes and conditions upon which the fulfilment of its task of prevention or of cure depends, and with as little real understanding of the means and appliances whereby this desired end is to be attained.

By this we do not mean to imply that medicine has attained, so far, to nothing more definite than a collection of single facts or cases, histories of diseases and cures, which time and observation have gathered together. The human mind has, in this respect, proved itself both willing and able to reflect upon that which it has observed, to arrange its complicated materials, and to discriminate which among them is best fitted to assist it in its proposed efforts. From the very beginning the observer has sought for an explanation of the phenomena and processes in the living body, its ailments, and their cure; and in this manner one system has followed upon the other. Each of them, probably, as it arose, gave rise to expectations of which advanced experience, in its turn, demonstrated the fallacy, and one generation after another questioned, doubted, or abolished what the preceding one had looked upon as established certainties, perhaps without being able to replace them by anything more stable and reliable. For nearly all medical interpretations or theories were of such a nature that a single new case, an individual novel fact, was perhaps sufficient to shake the conclusions we had built upon them, or even to prove them utterly false. And nearly every hitherto praised remedy and medical treatment is in equal danger of being abandoned on the appearance of a new one; while only simply empirical facts—whether in science or in art—have enjoyed a real stability.

Bacon looked upon the source and origin, as well as the results and progress of a science, as the sign or *criterium* of its trustworthiness and healthy state; and attached weight to the circumstance of the most able specialists and writers of a period being agreed upon its main doctrines and views, or not. Kant asserts the same thing when he says—"We may easily judge by the results of our labours whether we are pursuing the cultivation of knowledge by the sure and enlightened path of a science. If, after many preparations and provisions, it begins to flag as soon as an object is attained, or, in order to attain it, has frequently to go back and set out upon a fresh course; also, if it finds a difficulty in making its different plans and combinations work harmoniously together towards the attainment of the end they are jointly concerned with, we may feel certain that our study has not yet entered into the sure path of a science, but is so far merely seeking about for it."

If we judge of our medical acquisitions by this standard, the result may not prove particularly flattering in regard to medicine as a science. But it is of little use to close our eyes, and to pay compliments to ourselves and to each other upon a progress and upon acquisitions whose nature and extent, after all, remain undefined. For we have not yet advanced from empiricism to science, using this latter word in its full meaning, and the rules or principles according to which the physician as well as any other artist acts, are not safely to be trusted so long as he is not able to deduce them from an adequate understanding of the objects with which his attention is concerned; so long as the practical observation of every individual case, and every additional circumstance connected with it, demand his special attention, calculation, and consideration.

And here the physician is almost necessarily wont to content himself with a mode of observing and judging which other arts and sciences have long ago abolished, on account of the instability and uncertainty attached to them. Few among us, even now, see clearly, and fewer still will openly confess, how little all our bed-side experiences are in reality able to prove and establish, from the abundant and inevitable sources of error connected with our observation of disease and cure, and from the uncertainty to which we are liable in seeking to trace out these in their conditions and true causal connection; an uncertainty which is increased by our being yet far from possessing a just knowledge of their common laws, as shown in the unchanging conformity or order of their actions, influences and processes. In every individual case, therefore, we are left to a doubtful calculation of probabilities in regard to the total effect or final result, (*e. g.* of a disease or cure,) as a whole, in our endeavour to ascend to their possible, and, in the precise case at issue, perhaps more probable conditions. Even diseases which physicians have been observing daily for centuries—from the simple furuncle, catarrh, or spasm, to inflammation of the lungs, typhus, or epilepsy—offer us too many problems to allow us to boast of having possessed ourselves of the essential processes and intrinsic concatenation of circumstances by which they are attended. We have only become better acquainted with a few of their individual phenomena and effects, and therefore, in spite of ourselves, we are compelled to allow our judgment in the case of an individual patient to be directed by conjectures more or less uncertain concerning the conditions and nature of his disease, and by comparisons more or less adventurous with other patients, similar states, &c. And after all we must endeavour to remove symptoms and phenomena of disease whose peculiar nature, cause, and importance we have not yet been able to recognize with certainty. Again, the physician must form a conclusion as to the probable benefit of his measures and remedies by taking the particular circumstances of each individual case into due consideration, and this because the treatment and measures he adopts are seldom based upon a knowledge of the mode in which they act upon the disease in question, but are generally simply empirical, and have probably been discovered by mere chance. Aristotle said "ubi desinit physicus, ibi incipit medicus," and so, in essentials, it has remained to the present day.

All this brings us to the question why our knowledge has, after all, remained so stationary? And to this our previous remarks on medical logic will furnish the best answer. Its chief cause—in addition to the many inevitable difficulties of our department of science—may be traced to an opinion which, especially since the time of Bacon, has gained a certain footing among us, viz., that in general the experimental knowledge of all that takes place during the natural course of events, as they are connected with patients and their cure, was the main point by means of which we might, singly and unaided, arrive at an understanding of the nature and conditions of the disease. This is expressed, for instance, by Baglivi's "ars medica tota in observationibus," and so it came to pass that we learnt to be contented with the observation and collation of simply empirical facts, and, to a great extent, have remained so even to the present day. But in the general investigation of nature, our aim and our task must consist in the discernment of the connection and conformity of its various processes, as well in health as in disease; and the want of fully understanding this has led to an undue neglect of the only ways and means by which the end in question, had it but been more clearly placed before us, might have been attained. Knowledge, it is true, can only arise from experience, but we must not, under circumstances so complicated and difficult as those with which we are concerned, rely, for understanding them, upon experiences alone. Some, so-called empirics, little able to judge how the matter really stands, are accustomed to condemn all desire for deeper and more penetrating research as being vain, theoretical, and valueless. In this they pit experience against scientific knowledge, or practice against theory, observation against penetrating inquiry, in a manner which, while it betrays the confusion of their own ideas, must also work badly for the very carrying out of those practical requirements to which they attach themselves so exclusively. Empiricism has always advanced its claims with a presumption exactly proportioned to its inability to establish them. Instead of modestly doubting the trustworthiness of its insight and the actual value of the conclusions it leads to, it is satisfied with the little it has, or rather believes itself to have, and perhaps does not even recognize the facts by which the one as well as the other are daily rendered questionable. It holds fast by authority and prescription, for want of considering that such were established at a time when science was but in its infancy, and that, if we justly accord faith to the age and experience of an individual, so ought we, on kindred grounds, to yield it to the science of our days, in right of the more mature and experienced age it has attained to. Others, on the contrary, incline to believe that the bold and perhaps ingenious achievements of an individual may at once attain to results which the most circumspect investigation of centuries has been aiming at, and it is well known that false prophets, pretending to be able to fulfil such requirements, have never been wanting.

To this we may add that every science, in proportion to its inability to plant its inferences upon a solid experimental basis, becomes more dependent upon all other sciences, just as a weak power becomes dependent upon its mightier neighbours. All our so-called auxiliary sciences—anatomy and physiology, as well as chemistry and physics—have thus successively acted upon the ill-joined materials of medical science; so also has it been influenced by the changing systems of philosophy, just as its development happened at the time to be connected with the various discoveries these have made, and the various points they have established. Never has there been a want of heads to apply these detached hints to the purposes of medical science, or a lack of hands to work up these scattered crumbs falling from the master's table; only it has proved that the elevation so gained has, at the same time, afforded a standing-point from which to view the desert that surrounds our improved edifice. And, instead of setting about the diligent cultivation of this waste, we have too often become bewildered and blinded by the sirocco of crude hypotheses and arbitrary *a priori* attempts at explanation which was unceasingly wafted across it. We found ourselves able, perhaps, to cast various things into the new form, but their kernel, their intrinsic value, remained the same. Yet we have seen many physicians attach weight to the opinions of men of general scientific enlightenment, who have at the same time, held no position in regard to their medical attainments. We have allowed the auxiliary sciences to play the dictator over us in succession, instead of endeavouring ourselves to solve our own problems after the methods they have suggested. We have expected to arrive by their aid at knowledge which can never be attained except through our own exertions; and with this are connected an over-estimation of novel acquisitions, and that fallacious semblance of positivism or exactness which has crept into medicine in the train of microscopico-anatomical and chemical methods of investigation. Instead of inciting the physician to scientifically circumspect and sober investigation in his own department, they have rather tended to lull him into a fresh kind of delusion, and to entice him into hazardous hypotheses and theories without ground and foundation. He who perhaps understands pretty well how to explain the appearances in a *post-mortem* dissection, and to auscultate and percuss the chest of a patient, has often thereby become an authority in medicine—the celebrity of the day; and so it has been with many microscopists and chemists, who, perhaps, have furnished us with a number of hypotheses and one-sided arbitrary explanations, but rarely with facts and proofs. And if the wood at one time prevented us from seeing the single trees, we are now prevented by the number of single trees from seeing the wood.

It would, on the other hand, be not only unjust, but even quite absurd, to doubt that important progress has been made in the field of general knowledge in our science. The scientific horizon of the student of our day is an infinitely wider one than that which the most able physician enjoyed even a century ago; and views, notions, and theories which flourished a few *decennia* ago, are now seen in their true light of impossibilities. And if doubts *must* remain as to the certainty of our bed-side practice, it is no less certain that every patient, attended by an able physician, receives from him the maximum of assistance which his case makes obtainable. It is easy, in all cases, for the sceptic to confound mere uncertainty with impotence, and to class difficulties and impediments with impossibilities; and Bacon justly calls it a calumniation of the human mind, as well as of nature, to declare that to be impossible which happens to surpass our present powers, and to lie beyond the limits of our actual mental horizon. Here, too, the words of Humboldt are sufficiently encouraging to us, in which he says, "it is a certain test that much may yet be expected of a science when the facts and discoveries connected with it still stand unconnected, and nearly without relation to each other—yea, even if they appear to be contradictory." For it is well known that no science furnishes more opportunities of making discoveries, and of reconciling apparent contradictions of all kinds, than does that of medicine. We must still regret that physicians and investigators of nature, from their imperfect acquaintance with causal conditions, are apt to deceive themselves as to the extent and certainty of their knowledge and power, so that, by striving for things which, under the given circumstances, are impossible, they overlook and leave undone that which is sufficiently capable of accomplishment. For no cause can more impede the fitting choice of the ways and means which can alone lead to our desired end, than the want of a clear and discerning knowledge of causes and the effects attached to them. And on this deficiency the chief stress has been laid during the whole course of our argument. We have no longer, as in former days, to contend with the spirit of superstition, still less to complain of the want of aids and resources. Our chief impediment lies in ourselves; in the want of discerning what are our nearest and most important tasks; in the employment of unproductive, if not of wrong, methods of investigation; and in the imperfect and scanty employment of such as are good and fertile in improvement.

It may, perhaps, appear presuming, and forgetful of the principles already laid down, if I venture to add some intimations as to how the inquiries connected with medical science may proceed, so as to most surely tend to the attainment of the object with which we are so deeply, both as regards theory and practice, concerned. Yet I feel that I cannot more fitly take leave of my reader than by pointing out a few of the ways and means by which the physician may arrive at a clearer knowledge of his objects, and be able to carry them into more efficient practical working.

If we look upon the course which other branches of human knowledge have pursued in their development, we shall recognize an analogy pervading them all. A. Comte has observed that all knowledge, more especially such as regards nature, runs through three stages: that of fiction and poesy, of simple faith, and dogmatic opinion (Comte calls it the theological state); which is succeeded by that of arbitrary hypotheses, *a priori* abstraction, combination, or speculation (which he describes as the metaphysical state); while, last of all, arrives the stage of sober but positive science, based upon a clear understanding of what it has to deal with. The first is the necessary starting-point, the last the requisite aim and only legitimate conclusion, just as the individual who, during childhood, takes his belief upon trust, resting its grounds on the authority of others, becomes in youth a speculative metaphysician, and concludes, at a more mature age, with a clear insight and rational discernment which, at the same time, permit him to doubt of much that was formerly believed in implicitly.

During each of these epochs of our knowledge and judgment, the human mind must proceed in whatever pursuits and researches it enters into just as the state of its existing knowledge permits it. First of all, we seek to comprehend and discern that which a higher degree of knowledge would teach us it was in vain to aspire to—I mean the understanding of the intrinsic nature or essence, the ultimate cause, as well as the true purpose, of all the things round about us. But the nearest, because the most convenient, substitute for this is found in referring these unknown causes to some arbitrary arrangement, effected during the first stage by the intervention of a supernatural agency, which man can only imagine as personified. For everything he sees he has, as G. Forster says,[1] "but one cause, and this he calls God. For him a God thunders in the clouds, and leads him over the sea; to him there exists a God in the green oak and in the fragrant flower." At a later period, during the speculative metaphysical stage, this is replaced by similar but less distinctly personified abstractions and *a priori* conceptions—*i. e.*, so-called "forces," which are imagined as acting self-efficient entities which dwell in matter, and can of themselves call forth all the phenomena and processes which we observe to occur in it. Now it matters little, in regard to the main point, whether they are called force of gravity, or vital force, whether nervous or electric fluid, or principle of heat or light. In the last stage of science and positive investigation of nature, man learns to give up the inquiry into, and desire of understanding, the peculiar essence and ultimate causes of the things which surround him. All he wants is to determine, by sober positive investigation, the way and manner, the order and conformity to fixed laws, according to which certain phenomena and processes in nature really take place. Law, and a firm and intrinsically necessary order, step, in all things, into the place which has, on one side, been left vacant for chance to fill, and, on the other, been filled up by allegorical and arbitrarily erected aggregates of causes.

Upon this last stage we have long since seen natural philosophy and chemistry fairly enter; and as the investigation of the physiologist, and even of the pathologist, have already passed its threshold, we may expect to see the whole body of medical science follow in their train.

[1] Opera Omnia, vol. vi, p. 176; Leipzig, 1843.

First of all, as regards observation, medicine seems at last to have aimed at a sober view of its objects; to understand, if not yet thoroughly, the nature of the tasks and problems set before it; and to have become aware of the extent of the capabilities it can bring to bear upon them. But we have yet to emulate and copy more closely the methods by which our sister sciences have attained to such solid and brilliant acquisitions. And in all cases we must begin with that which lies the nearest and is the most easy—*i. e.*, with the expansion and determination of our simple experience of natural objects, or with the comprehensive yet exact confirmation of everything which, in the field of life, with the outward influences that bear upon it —in health as well as in disease—is already accessible to our observation. We must not be satisfied with a merely superficial observation of phenomena, but must seek after their elucidation with all possible accuracy as regards all their bearings, making use of all mechanical aids to this end,—such as weights and measures, retorts, and other similar expedients. Just as the phlogiston-theory was overthrown by the sole help of the scales, and by this the path was formed towards a better understanding of chemical processes, so may we also expect from stricter methods of observation to gain for the future a more certain knowledge. At present, with regard to the living human body, not even the smallest part of that is accurately determined which might be ascertained by mere simple observation—least so of all in the case of the sick, with regard to whom a more accurate system of investigation has only taken root in this century. Just as far are we from even an empirical acquaintance with the various diseases of the different zones and nations of the world, and remain even more in the dark with regard to the analogous conditions in plants and animals, especially in those of the lower orders. We are, therefore, in a great measure deficient in that comparative knowledge which the anatomist, the botanist, and the zoologist have at their disposal. And yet we shall hardly arrive at any specific knowledge of the diseases of our temperate zone without having become acquainted with those which prevail in the others, any more than we should be in a position to form a clear idea of mankind in general, before we had become acquainted with the human race as it exists under climates and aspects the most varying. It is with us, here, as in meteorology, where we have such need of reliable observations with regard to the state of the atmosphere and climate in the tropical and polar zones, that we cannot yet attain to such an insight into its processes as would justify us in speaking of meteorology as a whole.

But as our chief aim—*i. e.*, knowledge—can never be attained by the mere observation of the natural course of things in itself; as we do not, for instance, learn to understand a disease solely by the determination of the phenomena (symptoms) in the patient in question, but seek to trace out and bring to light the circumstances and changes by which these phenomena themselves may be caused, here, as in all cases, our chief task must be to determine their causal connection and conformity to fixed laws. We have endeavoured, on a former occasion, to explain the manner in which we should proceed for this purpose. Before all things, we must begin with the most simple and, comparatively speaking, most accessible relations and circumstances. Instead, for instance, of adhering, in our so-called ætiology, to miasmata, marshy atmospheres, or even to epidemic constitution, disposition to disease, and such like terms and aggregates, all the properties and laws of action of the individual agents, or influences, ought first to be investigated separately. Diseases themselves must not be looked upon as anomalous isolated processes and conditions, but rather as a series of simple modifications, or peculiar forms of various processes in the living body, differing only in the gradations or transitions of their physiological conditions or modes of action. The physician can never hope to attain to an accurate understanding of the diseases he has to deal with until he has learnt to connect together, as a whole, what has, in the course of time, become disjoined in physiology and pathology. We should likewise previously endeavour to ascertain the conformity and intrinsic connection of the individual processes or elements of a disease, instead of seeking in the first place to explain the whole, and to pronounce upon its proper ultimate causes. It is by this kind of resignation and wise self-restriction that other natural sciences have been able to attain that which we, up to this day, so painfully miss, and have yet to look for. We have, perhaps, laid these words of Descartes too little to our hearts;

"Nous devons préférer la connaissance de quelque peu de vérité, à la vanité de paraître n'ignorer rien." Everybody must know that the conquest of medical science is not to be achieved by a *coup-de-main,* nor even by a number of these in succession, let them be ever so well aimed and skilfully combined, but that it must be attained to by a regular and methodically consequential research. The "anticipatio mentis et naturæ" is quite a different thing from the circumspect and slowly, but surely, progressing "interpretatio naturæ" and "rectè veritas temporis filia dicitur, non auctoritatis." (Bacon.)

But for this it is obvious that we need a concurrence in the various branches of our investigation, as well as that, from as many sides and under as varying aspects as possible, they should meet and co-operate to one given end. In this track physicists and astronomers have long ago preceded us, and only by numerous observations, bearing upon one end, yet taken from many points of the globe, and carried on through a long series of years, have they been able to attain to a better understanding of such phenomena, for instance, as those connected with the magnetism of the earth and the relations of weather and climate, as well as such as relate to the movements of the celestial bodies. We should learn more and more, in regard to medical problems, to effect the same distribution and combination of labour. And as men of science, in separate countries, have already formed themselves into societies for the advancement of their general aims, they may the more easily, to this end, join hands over the whole civilized globe.

It does not rest with us to prove, here, that the whole practice of the physician, with the success and certainty attached to it, must depend upon the advance of true scientific knowledge, for Bacon, in the superscription of his second book, has taken the *interpretatio naturæ* to be synonymous with *regnum hominis.*

Far from there existing any discrepancy between the knowledge of a thing and the power of doing it, between understanding and performing, it is obvious that the two things are inseparably linked together. Such a difference, it is true, may exist between the capabilities and talents of the individual physician and investigator, because one and the same mind is not often found capable of following up comprehensive scientific principles, and of at the same time so applying his results as to make the best practical use of them. It is left (generally speaking) to some men to discover and furnish scientific rules, ideas, and truths, while others have best understood how to apply and render them useful. Our art depends, in a much higher degree than any other, upon those sciences which are concerned with the investigation of the living body, and with that of general nature and its processes. And in medicine we can only speak of fixed principles, and of a consciousness of security in our practice, in proportion as those sciences themselves have attained to established truths. Every artist is compelled to aim at a certain result (with us, for instance, the cure of a patient), and seeks by one or other proceeding or interference on his part to produce an effect, the success of which entirely depends upon the coinciding of certain conditions and circumstances. Every physician, therefore, as well as every other artist, should be in possession of an amount of knowledge at least sufficient to make him capable of judging by what proceedings and means he may best be able to accomplish the result he has in view—for instance, the fulfilment of an "indication" at the bedside, or the cure of a patient. He should be able to assure himself as to the circumstances and properties from which the effects of the remedies and mode of treatment he has adopted have arisen; he must know what has influenced and modified them, or perhaps directly opposed them, and, in the latter case, must strive to ascertain whether such opposition arises in the body he is acting upon, or is worked by the agent he is employing. And it is evident that the principles and rules of his art will only be useful to him in so far as they are based upon an understanding of and accordance with, what has here been stated. But his mere art, as such, will never put him in possession of this knowledge, &c. It is science alone which so instructs him as to the efficiency of his art as to enable him to judge of the means and measures by which he may most safely attain to the ends it is concerned with. Then only would it be the province of his art to furnish him, by the aid of such knowledge, with appropriate and fixed rules for the most complete possible performance of his task.

We know, however, that in medicine we have not yet, either in knowledge or in skill, attained to this point. In no art can knowledge be converted into tolerably certain precepts or rules for the practitioner so long as that knowledge itself is so fragmentary and incomplete as ours. For our rules of art are not (and this of necessity) derived from an understanding of the processes or conditions on which we are called upon to act, or from a knowledge of the remedies by means of which we expect to act upon them, but are drawn solely from our experience at the bed-side, taken in connection with an empirical knowledge of a more or less uncertain character. We have not yet even ascertained so much by simple observation as to know whether certain diseases may not perhaps be cured by the course of nature, undisturbed by any interference of art. And yet this step would appear to be indispensable to the formation of any judgment with regard to the adoption of such interference in the majority of cases. We can only attribute a value to our empirical rules in those ordinary and average cases, from the observation of which they have been derived as the final result of a certain number of individual experiences. Even in the most favorable case, therefore, they can only be looked upon as a general guidance, the observance of which, in the individual case at issue, must again require a special consideration of all the concomitant circumstances, and an appropriate modification of our proceedings in accordance with these. Thus it is less important to know that such and such results are, in general, to be obtained by certain measures or remedies, than to be able to judge whether and how far these will prove available in a given case, and what modification our treatment will have to undergo on account of the peculiar circumstances of the latter. But the possibility of arriving at this judgment—the most important element in our practical proceeding—is the result, as is clearly evident, of our previously acquired insight and knowledge.

Now, if the present state of our knowledge scarcely enables us to calculate or predict *anything* with certainty, least of all shall we be able to do so with regard to the results of our art. Not as if the processes and occurrences in our patients were deficient in a certain conformity of their own; nor as if effects could here take place without their adequate causes, or could fail to take place in spite of the action of their adequate causes, but because a number of causes and circumstances are here at all times in operation, the influence or laws of which have remained for the most part, if not entirely, unknown to us. In all cases, therefore, we can only attain to a kind of supposition of probabilities, and not to a demonstration of certainties; and even the probability of the former will, in a great measure, depend upon the discernment and circumspection of the individual. To this must be added that we physicians never are, nor shall be, masters of all the circumstances and influences under which, or against which, we have to act: we can do as little in this respect as the navigator or the statesman. If, then, there is any established certainty in medicine, we must not place it in the result of our curative attempts, but in the due understanding of what the preservation of health, or the completion of the cure, renders necessary. Our security consists, therefore, in the recognition of the indications at the bed-side, but not in their fulfilment. This makes it apparent that medicine might be infinitely more powerless and inefficient, as an art, than it is in reality, even if, as a science, it already held a high and secure position.

That the patient should expect his physician to fulfil all that which the interest of his health requires lies in the nature of the case, even if in this, as so frequently happens, he is asking for what is impossible. Unprofessional people are not aware of the limits which are placed alike to our knowledge of our art and its application. It is more to be wondered at that physicians should themselves further this error, by thinking proper to surround themselves with a halo of power which they do not possess, renouncing that to which their art truly entitles them, and may, in future, do so in a yet higher degree, as shall be presently shown. Nobody thinks of blaming or deriding the natural philosopher or meteorologist because he has not the power of averting tempests and storms, or of converting bad into fine weather; nor does he ever propose to deceive himself or others by the assumption of such an attribute of Divinity. Yet in our department of science, deceptions, at least self-deceptions, of this kind are not rare, although we have also to deal with processes and events which, equally with those of the natural philosopher or meteorologist, take place and proceed according to their fixed, unchangeable laws.

And whatever the physician is capable of changing, modifying, and effecting in his peculiar province, he can only accomplish through his knowledge of these laws, and through his respect for their operations.

We are far from wishing to quarrel with physicians who see the highest destination of medicine in its practical activity, as exerted for the health and bodily welfare of their fellow-creatures. Not without good reason may they be proud of this their vocation, for a nobler or more important one does not exist. But if they really are as desirous of affording relief as they aver themselves to be, and if they feel the whole responsibility which they have taken upon themselves with their calling, it will be their endeavour to adopt those measures and ways of succour which are most surely conducive to their noble end in the relief of suffering humanity, and to render their duty a more and more attainable thing. What our ancestors were wont to look upon as the only, or at least the most efficacious, means towards such an end can no longer be considered such by us. What a Paracelsus, or even a Sydenham, was warranted in believing, the physician of the present day neither can nor dare believe, any more than that the earth is a disc, or that there is no such thing as the circulation of the blood. If we look over the long series of our remedies and modes of treatment, we see them, with few exceptions, one after the other, condemned as inefficient, and sinking into oblivion; and it is only our ignorance, and consequent vague, if not erroneous, appreciation of their actual efficiency and services which have always remained the same. It is the most experienced physicians, best able to form a judgment in the matter, who have always been the first to perceive that when disease, more especially if its character be virulent, is once generated, medical skill can afford little help of a *positive* nature, even by the use of those measures upon which we have been accustomed to place the greatest reliance. J. Primerosius has said in his Preface,[1] "Rectè scripsit Hippocrates, medicinam omnium artium esse præstantissimam . . . . verum addit: propter ignorantiam eorum qui eam exercent, et eorum qui temerè de illis judicant, reliquis artibus inferiorem videri." And essentially the same thought is expressed in the

[1] 'De vulgi erroribus in Medicinâ,' lib. iv; Amsterdam, 1644.

complaint of Reveillé Parise:[1] "La médecine est la plus noble des professions, et le plus triste des métiers."

Fortunately, however, medicine is not so utterly destitute of expedients as our fears would lead us to imagine, only we must not look for them in our medicine-chests, but rather in Nature herself, and in her laws. We must learn how to satisfy the exigencies of the sound as well as of the diseased human body, and we must unlearn, little palatable as such a process may be, much that we have heretofore rested on as certain. We must renounce faith that has held dominion over us for centuries; and if the physician would remain, or, to speak more justly, would *become,* what his loftiest aims point to—the friend and counsellor of his suffering fellow-creatures—he will find himself compelled to adopt other ways and means than he has hitherto been contented with. "Be truthful if you would be believed and trusted" is here an applicable maxim. Man, even now, in all civilised countries, enjoys a better state of health than he has done in any former period; for many deadly plagues and epidemic diseases have, in a great measure, disappeared. But we have not become more free from them through learning to cure them better than our ancestors were able to do, but simply because the general habits of mankind have become, through an advanced civilization, more favorable to the maintenance of health. From this the physician may derive a valuable hint; and, forsaking the attempt to accomplish impossible ends by impossible means, he will the more endeavour to achieve that which lies evidently within the limits of possibility, and will learn, first of all, to look upon the prevention of disease as his primary and most sacred duty. When disease has already obtained ground, he will accustom himself to lay less stress upon his remedies than upon supplying his patient with all the necessaries of life. The more fully he becomes aware *how much,* even under his best endeavours, lies altogether beyond the compass of his powers, so much more earnestly will he strive, by means of such expedients as are attainable, to preserve and promote health, alike in the individual and in the state.

In short, it is probable that the therapeutics of the old school will have to give place more and more to prophylaxis, a change

[1] 'Gazette Médicale de Paris,' No. 29, 1851.

which has already been effected to some extent in practical North America (Massachusetts). By this, not only will the natural alliance between medicine and hygiene be re-established, but a more intimate bond than hitherto will be formed between these and all other natural sciences. And this will the more certainly take place the more physicians become clearly conscious that they must learn to look upon man only as a part of entire nature, in regard to his existence and requirements, his health, sickness, and recovery. A truly scientific and practical education, combined with the positive activity such an education implies, will place the physician in a far higher and more secure position in the public credit than he gained from the *nimbus* former times drew around him, or than he won from the privileges with which the state and its laws are wont to endow him.

To many it will perhaps appear melancholy to reflect that the utmost our science can accomplish is, to render itself superfluous through the success of its endeavours to preserve mankind in health. However, it is the greatest glory of the physician to relinquish such aims as are merely personal and selfish, in favour of the general benefit of mankind: but, after all, the danger of seeing medicine and its services rendered superfluous does not threaten us very closely.

But this will the sooner take place the more physicians become penetrated by the spirit of true scientific research in all things wherewith their practice connects them. And to further this enlightened investigation as far as in us lay, was one of the highest and most significant tasks we placed before us in our "Medical Logic."

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