# CHAPTER X.

OF PLURALITY OF CAUSES; AND OF THE INTERMIXTURE OF EFFECTS.

Sec. 1. In the preceding exposition of the four methods of observation and experiment, by which we contrive to distinguish among a mass of coexistent phenomena the particular effect due to a given cause, or the particular cause which gave birth to a given effect; it has been necessary to suppose, in the first instance, for the sake of simplification, that this analytical operation is encumbered by no other difficulties than what are essentially inherent in its nature; and to represent to ourselves, therefore, every effect, on the one hand as connected exclusively with a single cause, and on the other hand as incapable of being mixed and confounded with any other coexistent effect. We have regarded *a b c d e*, the aggregate of the phenomena existing at any moment, as consisting of dissimilar facts, *a*, *b*, *c*, *d*, and *e*, for each of which one, and only one, cause needs be sought; the difficulty being only that of singling out this one cause from the multitude of antecedent circumstances, A, B, C, D, and E. The cause indeed may not be simple; it may consist of an assemblage of conditions; but we have supposed that there was only one possible assemblage of conditions, from which the given effect could result.

If such were the fact, it would be comparatively an easy task to investigate the laws of nature. But the supposition does not hold, in either of its parts. In the first place, it is not true that the same phenomenon is always produced by the same cause: the effect *a* may sometimes arise from A, sometimes from B. And, secondly, the effects of different causes are often not dissimilar, but homogeneous, and marked out by no assignable boundaries from one another: A and B may produce not *a* and *b*, but different portions of an effect *a*. The obscurity and difficulty of the investigation of the laws of phenomena is singularly increased by the necessity of adverting to these two circumstances; Intermixture of Effects, and Plurality of Causes. To the latter, being the simpler of the two considerations, we shall first direct our attention.

It is not true, then, that one effect must be connected with only one cause, or assemblage of conditions; that each phenomenon can be produced only in one way. There are often several independent modes in which the same phenomenon could have originated. One fact may be the consequent in several invariable sequences; it may follow, with equal uniformity, any one of several antecedents, or collections of antecedents. Many causes may produce motion: many causes may produce some kinds of sensation: many causes may produce death. A given effect may really be produced by a certain cause, and yet be perfectly capable of being produced without it.

Sec. 2. One of the principal consequences of this fact of Plurality of Causes is, to render the first of the inductive methods, that of Agreement, uncertain. To illustrate that method, we supposed two instances, A B C followed by *a b c*, and A D E followed by *a d e*. From these instances it might apparently be concluded that A is an invariable antecedent of *a*, and even that it is the unconditional invariable antecedent, or cause, if we could be sure that there is no other antecedent common to the two cases. That this difficulty may not stand in the way, let us suppose the two cases positively ascertained to have no antecedent in common except A. The moment, however, that we let in the possibility of a plurality of causes, the conclusion fails. For it involves a tacit supposition, that *a* must have been produced in both instances by the same cause. If there can possibly have been two causes, those two may, for example, be C and E: the one may have been the cause of *a* in the former of the instances, the other in the latter, A having no influence in either case.

Suppose, for example, that two great artists, or great philosophers, that two extremely selfish, or extremely generous characters, were compared together as to the circumstances of their education and history, and the two cases were found to agree only in one circumstance: would it follow that this one circumstance was the cause of the quality which characterized both those individuals? Not at all; for the causes which may produce any type of character are innumerable; and the two persons might equally have agreed in their character, though there had been no manner of resemblance in their previous history.

This, therefore, is a characteristic imperfection of the Method of Agreement; from which imperfection the Method of Difference is free. For if we have two instances, A B C and B C, of which B C gives *b c*, and A being added converts it into *a b c*, it is certain that in this instance at least, A was either the cause of *a*, or an indispensable portion of its cause, even though the cause which produces it in other instances may be altogether different. Plurality of Causes, therefore, not only does not diminish the reliance due to the Method of Difference, but does not even render a greater number of observations or experiments necessary: two instances, the one positive and the other negative, are still sufficient for the most complete and rigorous induction. Not so, however, with the Method of Agreement. The conclusions which that yields, when the number of instances compared is small, are of no real value, except as, in the character of suggestions, they may lead either to experiments bringing them to the test of the Method of Difference, or to reasonings which may explain and verify them deductively.

It is only when the instances, being indefinitely multiplied and varied, continue to suggest the same result, that this result acquires any high degree of independent value. If there are but two instances, A B C and A D E, though these instances have no antecedent in common except A, yet as the effect may possibly have been produced in the two cases by different causes, the result is at most only a slight probability in favour of A; there may be causation, but it is almost equally probable that there was only a coincidence. But the oftener we repeat the observation, varying the circumstances, the more we advance towards a solution of this doubt. For if we try A F G, A H K, &c., all unlike one another except in containing the circumstance A, and if we find the effect *a* entering into the result in all these cases, we must suppose one of two things, either that it is caused by A, or that it has as many different causes as there are instances. With each addition, therefore, to the number of instances, the presumption is strengthened in favour of A. The inquirer, of course, will not neglect, if an opportunity present itself, to exclude A from some one of these combinations, from A H K for instance, and by trying H K separately, appeal to the Method of Difference in aid of the Method of Agreement. By the Method of Difference alone can it be ascertained that A is the cause of *a*; but that it is either the cause, or another effect of the same cause, may be placed beyond any reasonable doubt by the Method of Agreement, provided the instances are very numerous, as well as sufficiently various.

After how great a multiplication, then, of varied instances, all agreeing in no other antecedent except A, is the supposition of a plurality of causes sufficiently rebutted, and the conclusion that *a* is connected with A divested of the characteristic imperfection, and reduced to a virtual certainty? This is a question which we cannot be exempted from answering: but the consideration of it belongs to what is called the Theory of Probability, which will form the subject of a chapter hereafter. It is seen, however, at once, that the conclusion does amount to a practical certainty after a sufficient number of instances, and that the method, therefore, is not radically vitiated by the characteristic imperfection. The result of these considerations is only, in the first place, to point out a new source of inferiority in the Method of Agreement as compared with other modes of investigation, and new reasons for never resting contented with the results obtained by it, without attempting to confirm them either by the Method of Difference, or by connecting them deductively with some law or laws already ascertained by that superior method. And, in the second place, we learn from this the true theory of the value of mere *number* of instances in inductive inquiry. The Plurality of Causes is the only reason why mere number is of any importance. The tendency of unscientific inquirers is to rely too much on number, without analysing the instances; without looking closely enough into their nature, to ascertain what circumstances are or are not eliminated by means of them. Most people hold their conclusions with a degree of assurance proportioned to the mere *mass* of the experience on which they appear to rest; not considering that by the addition of instances to instances, all of the same kind, that is, differing from one another only in points already recognised as immaterial, nothing whatever is added to the evidence of the conclusion. A single instance eliminating some antecedent which existed in all the other cases, is of more value than the greatest multitude of instances which are reckoned by their number alone. It is necessary, no doubt, to assure ourselves, by repetition of the observation or experiment, that no error has been committed concerning the individual facts observed; and until we have assured ourselves of this, instead of varying the circumstances, we cannot too scrupulously repeat the same experiment or observation without any change. But when once this assurance has been obtained, the multiplication of instances which do not exclude any more

circumstances is entirely useless, provided there have been already enough to exclude the supposition of Plurality of Causes.

It is of importance to remark, that the peculiar modification of the Method of Agreement, which, as partaking in some degree of the nature of the Method of Difference, I have called the Joint Method of Agreement and Difference, is not affected by the characteristic imperfection now pointed out. For, in the joint method, it is supposed not only that the instances in which *a* is, agree only in containing A, but also that the instances in which *a* is not, agree only in not containing A. Now, if this be so, A must be not only the cause of *a*, but the only possible cause: for if there were another, as for example B, then in the instances in which *a* is not, B must have been absent as well as A, and it would not be true that these instances agree *only* in not containing A. This, therefore, constitutes an immense advantage of the joint method over the simple Method of Agreement. It may seem, indeed, that the advantage does not belong so much to the joint method, as to one of its two premises, (if they may be so called,) the negative premise. The Method of Agreement, when applied to negative instances, or those in which a phenomenon does *not* take place, is certainly free from the characteristic imperfection which affects it in the affirmative case. The negative premise, it might therefore be supposed, could be worked as a simple case of the Method of Agreement, without requiring an affirmative premise to be joined with it. But though this is true in principle, it is generally altogether impossible to work the Method of Agreement by negative instances without positive ones: it is so much more difficult to exhaust the field of negation than that of affirmation. For instance, let the question be, what is the cause of the transparency of bodies; with what prospect of success could we set ourselves to inquire directly in what the multifarious substances which are *not* transparent, agree? But we might hope much sooner to seize some point of resemblance among the comparatively few and definite species of objects which *are* transparent; and this being attained, we should quite naturally be put upon examining whether the *absence* of this one circumstance be not precisely the point in which all opaque substances will be found to resemble.

The Joint Method of Agreement and Difference, therefore, or, as I have otherwise called it, the Indirect Method of Difference (because, like the Method of Difference properly so called, it proceeds by ascertaining how and in what the cases where the phenomenon is present, differ from those in which it is absent) is, after the Direct Method of Difference, the most powerful of the remaining instruments of inductive investigation; and in the sciences which depend on pure observation, with little or no aid from experiment, this method, so well exemplified in the speculation on the cause of dew, is the primary resource, so far as direct appeals to experience are concerned.

Sec. 3. We have thus far treated Plurality of Causes only as a possible supposition, which, until removed, renders our inductions uncertain; and have only considered by what means, where the plurality does not really exist, we may be enabled to disprove it. But we must also consider it as a case actually occurring in nature, and which, as often as it does occur, our methods of induction ought to be capable of ascertaining and establishing. For this, however, there is required no peculiar method. When an effect is really producible by two or more causes, the process for detecting them is in no way different from that by which we discover single causes. They may (first) be discovered as separate sequences, by separate sets of instances. One set of observations or experiments shows that the sun is a cause of heat, another that friction is a source of it, another that percussion, another that electricity, another that chemical action is such a source. Or (secondly) the plurality may come to light in the course of collating a number of instances, when we attempt to find some circumstance in which they all agree, and fail in doing so. We find it impossible to trace, in all the cases in which the effect is met with, any common circumstance. We find that we can eliminate *all* the antecedents; that no one of them is present in all the instances, no one of them indispensable to the effect. On closer scrutiny, however, it appears that though no one is always present, one or other of several always is. If, on further analysis, we can detect in these any common element, we may be able to ascend from them to some one cause which is the really operative circumstance in them all. Thus it is now thought that in the production of heat by friction, percussion, chemical action, &c., the ultimate source is one and the same. But if (as continually happens) we cannot take this ulterior step, the different antecedents must be set down provisionally as distinct causes, each sufficient of itself to produce the effect.

We here close our remarks on the Plurality of Causes, and proceed to the still more peculiar and more complex case of the Intermixture of Effects, and the interference of causes with one another: a case constituting the principal part of the complication and difficulty of the study of nature; and with which the four only possible methods of directly inductive investigation by observation and experiment, are for the most part, as will appear presently, quite unequal to cope. The instrument of Deduction alone is adequate to unravel the complexities proceeding from this source; and the four methods have little more in their power than to supply premises for, and a verification of, our deductions.

Sec. 4. A concurrence of two or more causes, not separately producing each its own effect, but interfering with or modifying the effects of one another, takes place, as has already been explained, in two different ways. In the one, which is exemplified by the joint operation of different forces in mechanics, the separate effects of all the causes continue to be produced, but are compounded with one another, and disappear in one total. In the other, illustrated by the case of chemical action, the separate effects cease entirely, and are succeeded by phenomena altogether different, and governed by different laws.

Of these cases the former is by far the more frequent, and this case it is which, for the most part, eludes the grasp of our experimental methods. The other and exceptional case is essentially amenable to them. When the laws of the original agents cease entirely, and a phenomenon makes its appearance, which, with reference to those laws, is quite heterogeneous; when, for example, two gaseous substances, hydrogen and oxygen, on being brought together, throw off their peculiar properties, and produce the substance called water; in such cases the new fact may be subjected to experimental inquiry, like any other phenomenon; and the elements which are said to compose it may be considered as the mere agents of its production; the conditions on which it depends, the facts which make up its cause.

The *effects* of the new phenomenon, the *properties* of water, for instance, are as easily found by experiment as the effects of any other cause. But to discover the *cause* of it, that is, the particular conjunction of agents from which it results, is often difficult enough. In the first place, the origin and actual production of the phenomenon are most frequently inaccessible to our observation. If we could not have learned the composition of water until we found instances in which it was actually produced from oxygen and hydrogen, we should have been forced to wait until the casual thought struck some one of passing an electric spark through a mixture of the two gases, or inserting a lighted taper into it, merely to try what would happen. Besides, many substances, though they can be analysed, cannot by any known artificial means be recompounded. Further, even if we could have ascertained, by the Method of Agreement, that oxygen and hydrogen were both present when water is produced, no experimentation on oxygen and hydrogen separately, no knowledge of their laws, could have enabled us deductively to infer that they would produce water. We require a specific experiment on the two combined.

Under these difficulties, we should generally have been indebted for our knowledge of the causes of this class of effects, not to any inquiry directed specifically towards that end, but either to accident, or to the gradual progress of experimentation on the different combinations of which the producing agents are susceptible; if it were not for a peculiarity belonging to effects of this description, that they often, under some particular combination of circumstances, reproduce their causes. If water results from the juxtaposition of hydrogen and oxygen whenever this can be made sufficiently close and intimate, so, on the other hand, if water itself be placed in certain situations, hydrogen and oxygen are reproduced from it: an abrupt termination is put to the new laws, and the agents reappear separately with their own properties as at first. What is called chemical analysis is the process of searching for the causes of a phenomenon among its effects, or rather among the effects produced by the action of some other causes upon it.

Lavoisier, by heating mercury to a high temperature in a close vessel containing air, found that the mercury increased in weight, and became what was then called red precipitate, while the air, on being examined after the experiment, proved to have lost weight, and to have become incapable of supporting life or combustion. When red precipitate was exposed to a still greater heat, it became mercury again, and gave off a gas which

did support life and flame. Thus the agents which by their combination produced red precipitate, namely the mercury and the gas, reappear as effects resulting from that precipitate when acted upon by heat. So, if we decompose water by means of iron filings, we produce two effects, rust and hydrogen: now rust is already known by experiments upon the component substances, to be an effect of the union of iron and oxygen: the iron we ourselves supplied, but the oxygen must have been produced from the water. The result therefore is that water has disappeared, and hydrogen and oxygen have appeared in its stead: or in other words, the original laws of these gaseous agents, which had been suspended by the superinduction of the new laws called the properties of water, have again started into existence, and the causes of water are found among its effects.

Where two phenomena, between the laws or properties of which considered in themselves no connexion can be traced, are thus reciprocally cause and effect, each capable in its turn of being produced from the other, and each, when it produces the other, ceasing itself to exist (as water is produced from oxygen and hydrogen, and oxygen and hydrogen are reproduced from water); this causation of the two phenomena by one another, each being generated by the other's destruction, is properly transformation. The idea of chemical composition is an idea of transformation, but of a transformation which is incomplete; since we consider the oxygen and hydrogen to be present in the water *as* oxygen and hydrogen, and capable of being discovered in it if our senses were sufficiently keen: a supposition (for it is no more) grounded solely on the fact, that the weight of the water is the sum of the separate weights of the two ingredients. If there had not been this exception to the entire disappearance, in the compound, of the laws of the separate ingredients; if the combined agents had not, in this one particular of weight, preserved their own laws, and produced a joint result equal to the sum of their separate results; we should never, probably, have had the notion now implied by the words chemical composition: and, in the facts of water produced from hydrogen and oxygen, and hydrogen and oxygen produced from water, as the transformation would have been complete, we should have seen only a transformation.

The very promising generalization now commonly known as the Conservation or Persistence of Force, bears a close resemblance to what the conception of chemical composition would become, if divested of the one circumstance which now distinguishes it from simple transformation. It has long been known that heat is capable of producing electricity, and electricity heat; that mechanical motion in numerous cases produces and is produced by them both; and so of all other physical forces. It has of late become the general belief of scientific inquirers that mechanical force, electricity, magnetism, heat, light, and chemical action (to which has subsequently been added vital action) are not so much causes of one another as convertible into one another; and they are now generally spoken of as forms of one and the same force, varying only in its manifestations. This doctrine may be admitted, without by any means implying that Force is a real entity, a Thing in itself, distinct from all its phenomenal manifestations to our organs. Supposing the doctrine true, the several kinds of phenomena which it identifies in respect of their origin would nevertheless remain different facts; facts which would be causes of one another--reciprocally causes and effects, which is the first element in the form of causation properly called transformation. What the doctrine contains more than this, is, that in each of these cases of reciprocal causation, the causes are reproduced without alteration in quantity. This is what takes place in the transformations of matter: when water has been converted into hydrogen and oxygen, these can be reconverted into precisely the same quantity of water from which they were produced. To establish a corresponding law in regard to Force, it has to be proved that heat is capable of being converted into electricity, electricity into chemical action, chemical action into mechanical force, and mechanical force back again into the exact quantity of heat which was originally expended; and so through all the interchanges. Were this proved, it would establish what constitutes transformation, as distinguished from the simple fact of reciprocal causation. The fact in issue is simply the quantitative equivalence of all these natural agencies; whereby a given quantity of any one is convertible into, and interchangeable with, a given, and always the same, quantity of any other: this, no less, but also no more. It cannot yet be said that the law has been fully proved of any case, except that of interchange between heat and mechanical motion. It does seem to be ascertained, not only that these two are convertible into each other, but that after any number of conversions the original quantities reappear without addition or diminution, like the original quantities of hydrogen and oxygen after passing through the condition of water. If the same thing comes to be proved true of all the other

forces, in relation to these two and to one another, the law of Conservation will be established; and it will be a legitimate mode of expressing the fact, to speak of Force, as we already speak of Matter, as indestructible. But Force will not the less remain, to the philosopher, a mere abstraction of the mind. All that will have been proved is, that in the phenomena of Nature, nothing actually ceases without generating a calculable, and always the same, quantity of some other natural phenomenon, which again, when it ceases, will in its turn either generate a calculable, and always the same, quantity of some third phenomenon, or reproduce the original quantity of the first.

In these cases, where the heteropathic effect (as we called it in a former chapter)[44] is but a transformation of its cause, or in other words, where the effect and its cause are reciprocally such, and mutually convertible into each other; the problem of finding the cause resolves itself into the far easier one of finding an effect, which is the kind of inquiry that admits of being prosecuted by direct experiment. But there are other cases of heteropathic effects to which this mode of investigation is not applicable. Take, for instance, the heteropathic laws of mind; that portion of the phenomena of our mental nature which are analogous to chemical rather than to dynamical phenomena; as when a complex passion is formed by the coalition of several elementary impulses, or a complex emotion by several simple pleasures or pains, of which it is the result without being the aggregate, or in any respect homogeneous with them. The product, in these cases, is generated by its various factors; but the factors cannot be reproduced from the product; just as a youth can grow into an old man, but an old man cannot grow into a youth. We cannot ascertain from what simple feelings any of our complex states of mind are generated, as we ascertain the ingredients of a chemical compound, by making it, in its turn, generate them. We can only, therefore, discover these laws by the slow process of studying the simple feelings themselves, and ascertaining synthetically, by experimenting on the various combinations of which they are susceptible, what they, by their mutual action upon one another, are capable of generating.

Sec. 5. It might have been supposed that the other, and apparently simpler variety of the mutual interference of causes, where each cause continues to produce its own proper effect according to the same laws to which it conforms in its separate state, would have presented fewer difficulties to the inductive inquirer than that of which we have just finished the consideration. It presents, however, so far as direct induction apart from deduction is concerned, infinitely greater difficulties. When a concurrence of causes gives rise to a new effect, bearing no relation to the separate effects of those causes, the resulting phenomenon stands forth undisguised, inviting attention to its peculiarity, and presenting no obstacle to our recognising its presence or absence among any number of surrounding phenomena. It admits therefore of being easily brought under the canons of Induction, provided instances can be obtained such as those canons require: and the non-occurrence of such instances, or the want of means to produce them artificially, is the real and only difficulty in such investigations; a difficulty not logical, but in some sort physical. It is otherwise with cases of what, in a preceding chapter, has been denominated the Composition of Causes. There, the effects of the separate causes do not terminate and give place to others, thereby ceasing to form any part of the phenomenon to be investigated; on the contrary, they still take place, but are intermingled with, and disguised by, the homogeneous and closely allied effects of other causes. They are no longer *a*, *b*, *c*, *d*, *e*, existing side by side, and continuing to be separately discernible; they are + *a*, - *a*, 1/2 *b*, - *b*, 2 *b*, &c., some of which cancel one another, while many others do not appear distinguishably, but merge in one sum: forming altogether a result, between which and the causes whereby it was produced there is often an insurmountable difficulty in tracing by observation any fixed relation whatever.

The general idea of the Composition of Causes has been seen to be, that though two or more laws interfere with one another, and apparently frustrate or modify one another's operation, yet in reality all are fulfilled, the collective effect being the exact sum of the effects of the causes taken separately. A familiar instance is that of a body kept in equilibrium by two equal and contrary forces. One of the forces if acting alone would carry the body in a given time a certain distance to the west, the other if acting alone would carry it exactly as far towards the east; and the result is the same as if it had been first carried to the west as far as the one force would carry it, and then back towards the east as far as the other would carry it, that is, precisely the same distance; being ultimately left where it was found at first.

All laws of causation are liable to be in this manner counteracted, and seemingly frustrated, by coming into conflict with other laws, the separate result of which is opposite to theirs, or more or less inconsistent with it. And hence, with almost every law, many instances in which it really is entirely fulfilled, do not, at first sight, appear to be cases of its operation at all. It is so in the example just adduced: a force, in mechanics, means neither more nor less than a cause of motion, yet the sum of the effects of two causes of motion may be rest. Again, a body solicited by two forces in directions making an angle with one another, moves in the diagonal; and it seems a paradox to say that motion in the diagonal is the sum of two motions in two other lines. Motion, however, is but change of place, and at every instant the body is in the exact place it would have been in if the forces had acted during alternate instants instead of acting in the same instant; (saving that if we suppose two forces to act successively which are in truth simultaneous, we must of course allow them double the time.) It is evident, therefore, that each force has had, during each instant, all the effect which belonged to it; and that the modifying influence which one of two concurrent causes is said to exercise with respect to the other, may be considered as exerted not over the action of the cause itself, but over the effect after it is completed. For all purposes of predicting, calculating, or explaining their joint result, causes which compound their effects may be treated as if they produced simultaneously each of them its own effect, and all these effects coexisted visibly.

Since the laws of causes are as really fulfilled when the causes are said to be counteracted by opposing causes, as when they are left to their own undisturbed action, we must be cautious not to express the laws in such terms as would render the assertion of their being fulfilled in those cases a contradiction. If, for instance, it were stated as a law of nature that a body to which a force is applied moves in the direction of the force, with a velocity proportioned to the force directly, and to its own mass inversely; when in point of fact some bodies to which a force is applied do not move at all, and those which do move (at least in the region of our earth) are, from the very first, retarded by the action of gravity and other resisting forces, and at last stopped altogether; it is clear that the general proposition, though it would be true under a certain hypothesis, would not express the facts as they actually occur. To accommodate the expression of the law to the real phenomena, we must say, not that the object moves, but that it *tends* to move, in the direction and with the velocity specified. We might, indeed, guard our expression in a different mode, by saying that the body moves in that manner unless prevented, or except in so far as prevented, by some counteracting cause. But the body does not only move in that manner unless counteracted; it *tends* to move in that manner even when counteracted; it still exerts, in the original direction, the same energy of movement as if its first impulse had been undisturbed, and produces, by that energy, an exactly equivalent quantity of effect. This is true even when the force leaves the body as it found it, in a state of absolute rest; as when we attempt to raise a body of three tons weight with a force equal to one ton. For if, while we are applying this force, wind or water or any other agent supplies an additional force just exceeding two tons, the body will be raised; thus proving that the force we applied exerted its full effect, by neutralizing an equivalent portion of the weight which it was insufficient altogether to overcome. And if while we are exerting this force of one ton upon the object in a direction contrary to that of gravity, it be put into a scale and weighed, it will be found to have lost a ton of its weight, or in other words, to press downwards with a force only equal to the difference of the two forces.

These facts are correctly indicated by the expression *tendency*. All laws of causation, in consequence of their liability to be counteracted, require to be stated in words affirmative of tendencies only, and not of actual results. In those sciences of causation which have an accurate nomenclature, there are special words which signify a tendency to the particular effect with which the science is conversant; thus *pressure*, in mechanics, is synonymous with tendency to motion, and forces are not reasoned on as causing actual motion, but as exerting pressure. A similar improvement in terminology would be very salutary in many other branches of science.

The habit of neglecting this necessary element in the precise expression of the laws of nature, has given birth to the popular prejudice that all general truths have exceptions; and much unmerited distrust has thence accrued to the conclusions of science, when they have been submitted to the judgment of minds insufficiently disciplined and cultivated. The rough generalizations suggested by common observation usually have exceptions; but principles of science, or in other words, laws of causation, have not. "What is thought to be an

exception to a principle," (to quote words used on a different occasion,) "is always some other and distinct principle cutting into the former; some other force which impinges[45] against the first force, and deflects it from its direction. There are not a law and an exception to that law, the law acting in ninety-nine cases, and the exception in one. There are two laws, each possibly acting in the whole hundred cases, and bringing about a common effect by their conjunct operation. If the force which, being the less conspicuous of the two, is called the *disturbing* force, prevails sufficiently over the other force in some one case, to constitute that case what is commonly called an exception, the same disturbing force probably acts as a modifying cause in many other cases which no one will call exceptions.

"Thus if it were stated to be a law of nature that all heavy bodies fall to the ground, it would probably be said that the resistance of the atmosphere, which prevents a balloon from falling, constitutes the balloon an exception to that pretended law of nature. But the real law is, that all heavy bodies *tend* to fall; and to this there is no exception, not even the sun and moon; for even they, as every astronomer knows, tend towards the earth, with a force exactly equal to that with which the earth tends towards them. The resistance of the atmosphere might, in the particular case of the balloon, from a misapprehension of what the law of gravitation is, be said to *prevail over* the law; but its disturbing effect is quite as real in every other case, since though it does not prevent, it retards the fall of all bodies whatever. The rule, and the so-called exception, do not divide the cases between them; each of them is a comprehensive rule extending to all cases. To call one of these concurrent principles an exception to the other, is superficial, and contrary to the correct principles of nomenclature and arrangement. An effect of precisely the same kind, and arising from the same cause, ought not to be placed in two different categories, merely as there does or does not exist another cause preponderating over it."[46]

Sec. 6. We have now to consider according to what method these complex effects, compounded of the effects of many causes, are to be studied; how we are enabled to trace each effect to the concurrence of causes in which it originated, and ascertain the conditions of its recurrence--the circumstances in which it may be expected again to occur. The conditions of a phenomenon which arises from a composition of causes, may be investigated either deductively or experimentally.

The case, it is evident, is naturally susceptible of the deductive mode of investigation. The law of an effect of this description is a result of the laws of the separate causes on the combination of which it depends, and is therefore in itself capable of being deduced from these laws. This is called the method *a priori*. The other, or *a posteriori* method, professes to proceed according to the canons of experimental inquiry. Considering the whole assemblage of concurrent causes which produced the phenomenon, as one single cause, it attempts to ascertain the cause in the ordinary manner, by a comparison of instances. This second method subdivides itself into two different varieties. If it merely collates instances of the effect, it is a method of pure observation. If it operates upon the causes, and tries different combinations of them, in hopes of ultimately hitting the precise combination which will produce the given total effect, it is a method of experiment.

In order more completely to clear up the nature of each of these three methods, and determine which of them deserves the preference, it will be expedient (conformably to a favourite maxim of Lord Chancellor Eldon, to which, though it has often incurred philosophical ridicule, a deeper philosophy will not refuse its sanction) to "clothe them in circumstances." We shall select for this purpose a case which as yet furnishes no very brilliant example of the success of any of the three methods, but which is all the more suited to illustrate the difficulties inherent in them. Let the subject of inquiry be, the conditions of health and disease in the human body; or (for greater simplicity) the conditions of recovery from a given disease; and in order to narrow the question still more, let it be limited, in the first instance, to this one inquiry: Is, or is not some particular medicament (mercury, for instance) a remedy for the given disease.

Now, the deductive method would set out from known properties of mercury, and known laws of the human body, and by reasoning from these, would attempt to discover whether mercury will act upon the body when in the morbid condition supposed, in such a manner as to restore health. The experimental method would

simply administer mercury in as many cases as possible, noting the age, sex, temperament, and other peculiarities of bodily constitution, the particular form or variety of the disease, the particular stage of its progress, &c., remarking in which of these cases it produced a salutary effect, and with what circumstances it was on those occasions combined. The method of simple observation would compare instances of recovery, to find whether they agreed in having been preceded by the administration of mercury; or would compare instances of recovery with instances of failure, to find cases which, agreeing in all other respects, differed only in the fact that mercury had been administered, or that it had not.

Sec. 7. That the last of these three modes of investigation is applicable to the case, no one has ever seriously contended. No conclusions of value on a subject of such intricacy, ever were obtained in that way. The utmost that could result would be a vague general impression for or against the efficacy of mercury, of no avail for guidance unless confirmed by one of the other two methods. Not that the results, which this method strives to obtain, would not be of the utmost possible value if they could be obtained. If all the cases of recovery which presented themselves, in an examination extending to a great number of instances, were cases in which mercury had been administered, we might generalize with confidence from this experience, and should have obtained a conclusion of real value. But no such basis for generalization can we, in a case of this description, hope to obtain. The reason is that which we have spoken of as constituting the characteristic imperfection of the Method of Agreement; Plurality of Causes. Supposing even that mercury does tend to cure the disease, so many other causes, both natural and artificial, also tend to cure it, that there are sure to be abundant instances of recovery in which mercury has not been administered: unless, indeed, the practice be to administer it in all cases; on which supposition it will equally be found in the cases of failure.

When an effect results from the union of many causes, the share which each has in the determination of the effect cannot in general be great: and the effect is not likely, even in its presence or absence, still less in its variations, to follow, even approximately, any one of the causes. Recovery from a disease is an event to which, in every case, many influences must concur. Mercury may be one such influence; but from the very fact that there are many other such, it will necessarily happen that although mercury is administered, the patient, for want of other concurring influences, will often not recover, and that he often will recover when it is not administered, the other favourable influences being sufficiently powerful without it. Neither, therefore, will the instances of recovery agree in the administration of mercury, nor will the instances of failure agree in its non-administration. It is much if, by multiplied and accurate returns from hospitals and the like, we can collect that there are rather more recoveries and rather fewer failures when mercury is administered than when it is not; a result of very secondary value even as a guide to practice, and almost worthless as a contribution to the theory of the subject.

Sec. 8. The inapplicability of the method of simple observation to ascertain the conditions of effects dependent on many concurring causes, being thus recognised; we shall next inquire whether any greater benefit can be expected from the other branch of the *a posteriori* method, that which proceeds by directly trying different combinations of causes, either artificially produced or found in nature, and taking notice what is their effect: as, for example, by actually trying the effect of mercury, in as many different circumstances as possible. This method differs from the one which we have just examined, in turning our attention directly to the causes or agents, instead of turning it to the effect, recovery from the disease. And since, as a general rule, the effects of causes are far more accessible to our study than the causes of effects, it is natural to think that this method has a much better chance of proving successful than the former.

The method now under consideration is called the Empirical Method; and in order to estimate it fairly, we must suppose it to be completely, not incompletely, empirical. We must exclude from it everything which partakes of the nature not of an experimental but of a deductive operation. If for instance we try experiments with mercury upon a person in health, in order to ascertain the general laws of its action upon the human body, and then reason from these laws to determine how it will act upon persons affected with a particular disease, this may be a really effectual method, but this is deduction. The experimental method does not derive the law of a complex case from the simpler laws which conspire to produce it, but makes its experiments

directly upon the complex case. We must make entire abstraction of all knowledge of the simpler tendencies, the *modi operandi* of mercury in detail. Our experimentation must aim at obtaining a direct answer to the specific question, Does or does not mercury tend to cure the particular disease?

Let us see, therefore, how far the case admits of the observance of those rules of experimentation, which it is found necessary to observe in other cases. When we devise an experiment to ascertain the effect of a given agent, there are certain precautions which we never, if we can help it, omit. In the first place, we introduce the agent into the midst of a set of circumstances which we have exactly ascertained. It needs hardly be remarked how far this condition is from being realized in any case connected with the phenomena of life; how far we are from knowing what are all the circumstances which pre-exist in any instance in which mercury is administered to a living being. This difficulty, however, though insuperable in most cases, may not be so in all; there are sometimes concurrences of many causes, in which we yet know accurately what the causes are. Moreover, the difficulty may be attenuated by sufficient multiplication of experiments, in circumstances rendering it improbable that any of the unknown causes should exist in them all. But when we have got clear of this obstacle, we encounter another still more serious. In other cases, when we intend to try an experiment, we do not reckon it enough that there be no circumstance in the case the presence of which is unknown to us. We require also that none of the circumstances which we do know, shall have effects susceptible of being confounded with those of the agent whose properties we wish to study. We take the utmost pains to exclude all causes capable of composition with the given cause; or if forced to let in any such causes, we take care to make them such that we can compute and allow for their influence, so that the effect of the given cause may, after the subduction of those other effects, be apparent as a residual phenomenon.

These precautions are inapplicable to such cases as we are now considering. The mercury of our experiment being tried with an unknown multitude (or even let it be a known multitude) of other influencing circumstances, the mere fact of their being influencing circumstances implies that they disguise the effect of the mercury, and preclude us from knowing whether it has any effect or not. Unless we already knew what and how much is owing to every other circumstance, (that is, unless we suppose the very problem solved which we are considering the means of solving,) we cannot tell that those other circumstances may not have produced the whole of the effect, independently or even in spite of the mercury. The Method of Difference, in the ordinary mode of its use, namely by comparing the state of things following the experiment with the state which preceded it, is thus, in the case of intermixture of effects, entirely unavailing; because other causes than that whose effect we are seeking to determine, have been operating during the transition. As for the other mode of employing the Method of Difference, namely by comparing, not the same case at two different periods, but different cases, this in the present instance is quite chimerical. In phenomena so complicated it is questionable if two cases, similar in all respects but one, ever occurred; and were they to occur, we could not possibly know that they were so exactly similar.

Anything like a scientific use of the method of experiment, in these complicated cases, is therefore out of the question. We can in the most favourable cases only discover, by a succession of trials, that a certain cause is *very often* followed by a certain effect. For, in one of these conjunct effects, the portion which is determined by any one of the influencing agents, is generally, as we before remarked, but small; and it must be a more potent cause than most, if even the tendency which it really exerts is not thwarted by other tendencies in nearly as many cases as it is fulfilled.

If so little can be done by the experimental method to determine the conditions of an effect of many combined causes, in the case of medical science; still less is this method applicable to a class of phenomena more complicated than even those of physiology, the phenomena of politics and history. There, Plurality of Causes exists in almost boundless excess, and effects are, for the most part, inextricably interwoven with one another. To add to the embarrassment, most of the inquiries in political science relate to the production of effects of a most comprehensive description, such as the public wealth, public security, public morality, and the like: results liable to be affected directly or indirectly either in *plus* or in *minus* by nearly every fact which exists, or event which occurs, in human society. The vulgar notion, that the safe methods on political subjects are those

of Baconian induction--that the true guide is not general reasoning, but specific experience--will one day be quoted as among the most unequivocal marks of a low state of the speculative faculties in any age in which it is accredited. Nothing can be more ludicrous than the sort of parodies on experimental reasoning which one is accustomed to meet with, not in popular discussion only, but in grave treatises, when the affairs of nations are the theme. "How," it is asked, "can an institution be bad, when the country has prospered under it?" "How can such or such causes have contributed to the prosperity of one country, when another has prospered without them?" Whoever makes use of an argument of this kind, not intending to deceive, should be sent back to learn the elements of some one of the more easy physical sciences. Such reasoners ignore the fact of Plurality of Causes in the very case which affords the most signal example of it. So little could be concluded, in such a case, from any possible collation of individual instances, that even the impossibility, in social phenomena, of making artificial experiments, a circumstance otherwise so prejudicial to directly inductive inquiry, hardly affords, in this case, additional reason of regret. For even if we could try experiments upon a nation or upon the human race, with as little scruple as M. Magendie tried them on dogs and rabbits, we should never succeed in making two instances identical in every respect except the presence or absence of some one definite circumstance. The nearest approach to an experiment in the philosophical sense, which takes place in politics, is the introduction of a new operative element into national affairs by some special and assignable measure of government, such as the enactment or repeal of a particular law. But where there are so many influences at work, it requires some time for the influence of any new cause upon national phenomena to become apparent; and as the causes operating in so extensive a sphere are not only infinitely numerous, but in a state of perpetual alteration, it is always certain that before the effect of the new cause becomes conspicuous enough to be a subject of induction, so many of the other influencing circumstances will have changed as to vitiate the experiment.

Two, therefore, of the three possible methods for the study of phenomena resulting from the composition of many causes, being, from the very nature of the case, inefficient and illusory, there remains only the third,--that which considers the causes separately, and infers the effect from the balance of the different tendencies which produce it: in short, the deductive, or *a priori* method. The more particular consideration of this intellectual process requires a chapter to itself.