much and such varied scope for observation, we have made most scanty progress in ascertaining any laws of causation. We know not with certainty, in the case of most of the phenomena that we find conjoined, which is the condition of the other; which is cause, and which effect, or whether either of them is so, or they are not rather conjunct effects of causes yet to be discovered, complex results of laws hitherto unknown.

Although some of the foregoing observations may be, in technical strictness of arrangement, premature in this place, it seemed that a few general remarks on the difference between sciences of mere observation and sciences of experimentation, and the extreme disadvantage under which directly inductive inquiry is necessarily carried on in the former, were the best
[278] preparation for discussing the methods of direct induction; a preparation rendering superfluous much that must otherwise have been introduced, with some inconvenience, into the heart of that discussion. To the consideration of these methods we now proceed.

Chapter VIII.

Of The Four Methods Of Experimental Inquiry.

§ 1. The simplest and most obvious modes of singling out from among the circumstances which precede or follow a phenomenon, those with which it is really connected by an invariable law, are two in number. One is, by comparing together different instances in which the phenomenon occurs. The other is, by comparing instances in which the phenomenon does occur, with instances in other respects similar in which it does not. These two methods may be respectively denominated, the Method of Agreement, and the Method of Difference.

In illustrating these methods, it will be necessary to bear in mind the twofold character of inquiries into the laws of phenomena; which may be either inquiries into the cause of a given effect, or into the effects or properties of a given cause. We shall consider the methods in their application to either order of investigation, and shall draw our examples equally from both.

We shall denote antecedents by the large letters of the alphabet, and the consequents corresponding to them by the small. Let A, then, be an agent or cause, and let the object of our inquiry be to ascertain what are the effects of this cause. If we can either find, or produce, the agent A in such varieties of circumstances that the different cases have no circumstance in common except A; then whatever effect we find to be produced in all our trials, is indicated as the effect of A. Suppose, for example, that A is tried along with B and C, and that the effect is *a b c*; and suppose that A is next tried with D and E, but without B and C, and that the effect is *a d e*. Then we may reason thus: *b* and *c* are not effects of A, for they were not produced by it in the second experiment; nor are *d* and *e*, for they were not produced in the first. Whatever is really the effect of A must have been produced in both instances; now this condition is fulfilled by no circumstance except *a*. The phenomenon *a* can not have been the effect of B or C, since it was produced where they were not; nor of D or E, since it was produced where they were not. Therefore it is the effect of A.

For example, let the antecedent A be the contact of an alkaline substance and an oil. This combination being tried under several varieties of circumstances, resembling each other in nothing else, the results agree in the production of a greasy and detersive or saponaceous substance: it is therefore concluded that the combination of an oil and an alkali causes the production of a soap. It is thus we inquire, by the Method of Agreement, into the effect of a given cause.

In a similar manner we may inquire into the cause of a given effect. Let *a* be the effect. Here, as shown in the last chapter, we have only the resource of observation without experiment: we can not take a phenomenon of which we know not the origin,
[279] and try to find its mode of production by producing it: if we succeeded in such a random trial it could only be by accident. But if we can observe a in two different combinations, *a b c* and *a d e*; and if we know, or can discover, that the antecedent circumstances in these cases respectively were A B C and A D E, we may conclude by a reasoning similar to that in the preceding example, that A is the antecedent connected with the consequent *a* by a law of causation. B and C, we may say, can not be causes of *a*, since on its second occurrence they were not present; nor are D and E, for they were not present on its first occurrence. A, alone of the five circumstances, was found among the antecedents of *a* in both instances.

For example, let the effect *a* be crystallization. We compare instances in which bodies are known to assume crystalline structure, but which have no other point of agreement; and we find them to have one, and as far as we can observe, only one, antecedent in common: the deposition of a solid matter from a liquid state, either a state of fusion or of solution. We conclude, therefore, that the solidification of a substance from a liquid state is an invariable antecedent of its crystallization.

In this example we may go further, and say, it is not only the invariable antecedent but the cause; or at least the proximate event which completes the cause. For in this case we are able, after detecting the antecedent A, to produce it artificially, and by finding that *a* follows it, verify the result of our induction. The importance of thus reversing the proof was strikingly manifested when, by keeping a phial of water charged with siliceous particles undisturbed for years, a chemist (I believe Dr. Wollaston) succeeded in obtaining crystals of quartz; and in the equally interesting experiment in which Sir James Hall produced artificial marble by the cooling of its materials from fusion under immense pressure: two admirable examples of the light which may be thrown upon the most secret processes of Nature by well-contrived interrogation of her.

But if we can not artificially produce the phenomenon A, the conclusion that it is the cause of *a* remains subject to very considerable doubt. Though an invariable, it may not be the unconditional antecedent of *a*, but may precede it as day precedes night or night day. This uncertainty arises from the impossibility of assuring ourselves that A is the *only* immediate antecedent common to both the instances. If we could be certain of having ascertained all the invariable antecedents, we might be sure that the unconditional invariable antecedent, or cause, must be found somewhere among them. Unfortunately it is hardly ever possible to ascertain all the antecedents, unless the phenomenon is one which we can produce artificially. Even then, the difficulty is merely lightened, not removed: men knew how to raise water in pumps long before they adverted to what was really the operating circumstance in the means they employed, namely, the pressure of the atmosphere on the open surface of the water. It is, however, much easier to analyze completely a set of arrangements made by ourselves, than the whole complex mass of the agencies which nature happens to be exerting at the moment of the production of a given phenomenon. We may overlook some of the material circumstances in an experiment with an electrical machine; but we shall, at the worst, be better acquainted with them than with those of a thunder-storm.

The mode of discovering and proving laws of nature, which we have now examined, proceeds on the following axiom: Whatever circumstances can be excluded, without prejudice to the phenomenon, or can be absent notwithstanding its presence, is not connected with it in the way of causation. The casual [280]
circumstances being thus eliminated, if only one remains, that

one is the cause which we are in search of: if more than one, they either are, or contain among them, the cause; and so, *mutatis mutandis*, of the effect. As this method proceeds by comparing different instances to ascertain in what they agree, I have termed it the Method of Agreement; and we may adopt as its regulating principal the following canon:

First Canon.

If two or more instances of the phenomenon under investigation have only one circumstance in common, the circumstance in which alone all the instances agree, is the cause (or effect) of the given phenomenon.

Quitting for the present the Method of Agreement, to which we shall almost immediately return, we proceed to a still more potent instrument of the investigation of nature, the Method of Difference.

§ 2. In the Method of Agreement, we endeavored to obtain instances which agreed in the given circumstance but differed in every other: in the present method we require, on the contrary, two instances resembling one another in every other respect, but differing in the presence or absence of the phenomenon we wish to study. If our object be to discover the effects of an agent A, we must procure A in some set of ascertained circumstances, as A B C, and having noted the effects produced, compare them with the effect of the remaining circumstances B C, when A is absent. If the effect of A B C is *a b c*, and the effect of B C *b c*, it is evident that the effect of A is *a*. So again, if we begin at the other end, and desire to investigate the cause of an effect *a*, we must select an instance, as *a b c*, in which the effect occurs, and in which the antecedents were A B C, and we must look out for another instance in which the remaining circumstances, *b c*, occur without *a*. If the antecedents, in that instance, are B C, we know that the cause of *a* must be A: either A alone, or A in conjunction with some of the other circumstances present.

It is scarcely necessary to give examples of a logical process to which we owe almost all the inductive conclusions we draw in daily life. When a man is shot through the heart, it is by this method we know that it was the gunshot which killed him: for he was in the fullness of life immediately before, all circumstances being the same, except the wound.

The axioms implied in this method are evidently the following. Whatever antecedent can not be excluded without preventing the phenomenon, is the cause, or a condition, of that phenomenon: whatever consequent can be excluded, with no other difference in the antecedents than the absence of a particular one, is the effect of that one. Instead of comparing different instances of a phenomenon, to discover in what they agree, this method compares an instance of its occurrence with an instance of its non-occurrence, to discover in what they differ. The canon which is the regulating principle of the Method of Difference may be expressed as follows:

Second Canon.

If an instance in which the phenomenon under investigation occurs, and an instance in which it does not occur, have every circumstance in common save one, that one occurring only in the former; the circumstance in which alone the two instances differ, is the effect, or the cause, or an indispensable part of the cause, of the phenomenon. [281]

§ 3. The two methods which we have now stated have many features of resemblance, but there are also many distinctions between them. Both are methods of *elimination*. This term (employed in the theory of equations to denote the process by which one after another of the elements of a question is excluded, and the solution made to depend on the relation between the remaining elements only) is well suited to express the operation, analogous to this, which has been understood since the time of Bacon to be the foundation of experimental inquiry: namely, the successive exclusion of the various circumstances which are found to accompany a phenomenon in a given instance, in order to ascertain what are those among them which can be absent consistently with the existence of the phenomenon. The Method of Agreement stands on the ground that whatever can be eliminated, is not connected with the phenomenon by any law. The Method of Difference has for its foundation, that whatever can not be eliminated, is connected with the phenomenon by a law.

Of these methods, that of Difference is more particularly a method of artificial experiment; while that of Agreement is more especially the resource employed where experimentation is impossible. A few reflections will prove the fact, and point out the reason of it.

It is inherent in the peculiar character of the Method of Difference, that the nature of the combinations which it requires is much more strictly defined than in the Method of Agreement. The two instances which are to be compared with one another must be exactly similar, in all circumstances except the one which we are attempting to investigate: they must be in the relation of A B C and B C, or of *a b c* and *b c*. It is true that this similarity of circumstances needs not extend to such as are already known to be immaterial to the result. And in the case of most phenomena we learn at once, from the commonest experience, that most of the co-existent phenomena of the universe may be either present or absent without affecting the given phenomenon; or, if present, are present indifferently when the phenomenon does not happen and when it does. Still, even limiting the identity which is required between the two instances, A B C and B C, to such circumstances as are not already known to be indifferent, it is very seldom that nature affords two instances, of which we can be assured that they stand in this precise relation to one another. In the spontaneous operations of nature there is generally such complication and such obscurity, they are mostly either on so overwhelmingly large or on so inaccessibly minute a scale, we are so ignorant of a great part of the facts which really take place, and even those of which we are not ignorant are so multitudinous, and therefore so seldom exactly alike in any two cases, that a spontaneous experiment, of the kind required by the Method of Difference, is commonly not to be found. When, on the contrary, we obtain a phenomenon by an artificial experiment, a pair of instances such as the method requires is obtained almost as a matter of course, provided the process does not last a long time. A certain state of surrounding circumstances existed before we commenced the experiment; this is B C. We then introduce A; say, for instance, by merely bringing an object from another part of the room, before there has been time for any change in the other elements. It is, in short (as M. Comté observes), the very nature of an experiment, to introduce into the pre-existing state of circumstances a change perfectly definite. We choose a previous state of things with which we are well acquainted, so that no unforeseen alteration in that state is likely to pass unobserved; and into this we introduce, as rapidly as possible, the phenomenon which we wish to study; so that in general [282] we are entitled to feel complete assurance that the pre-existing state, and the state which we have produced, differ in nothing except the presence or absence of that phenomenon. If a bird is taken from a cage, and instantly plunged into carbonic acid gas, the experimentalist may be fully assured (at all events after one or two repetitions) that no circumstance capable of causing suffocation had supervened in the interim, except the change from immersion in the atmosphere to immersion in carbonic acid gas. There is one doubt, indeed, which may remain in some cases of this description; the effect may have been produced not by the change, but by the means employed to produce the change. The possibility, however, of this last supposition generally admits of being conclusively tested by other experiments. It thus appears that in the study of the various kinds of phenomena which we can, by our voluntary agency, modify or control, we can in general satisfy the requisitions of the Method of Difference; but that by the spontaneous operations of nature those requisitions are seldom fulfilled.

The reverse of this is the case with the Method of Agreement. We do not here require instances of so special and determinate a kind. Any instances whatever, in which nature presents us with a phenomenon, may be examined for the purposes of this method; and if all such instances agree in any thing, a conclusion of considerable value is already attained. We can seldom, indeed, be sure that the one point of agreement is the only one; but this ignorance does not, as in the Method of Difference, vitiate the conclusion; the certainty of the result, as far as it goes, is not affected. We have ascertained one invariable antecedent or consequent, however many other invariable antecedents or consequents may still remain unascertained. If A B C, A D E, A F G, are all equally followed by a, then a is an invariable consequent of A. If *a b c*, *a d e*, *a f g*, all number A among their antecedents, then A is connected as an antecedent, by some invariable law, with *a*. But to determine whether this invariable antecedent is a cause, or this invariable consequent an effect, we must be able, in addition, to produce the one by means of the other; or, at least, to obtain that which alone constitutes our assurance of having produced any thing, namely, an instance in which the effect, *a*, has come into existence, with no other change in the pre-existing circumstances than the addition of A. And this, if we can do it, is an application of the Method of Difference, not of the Method of Agreement.

It thus appears to be by the Method of Difference alone that we can ever, in the way of direct experience, arrive with certainty at causes. The Method of Agreement leads only to laws of phenomena (as some writers call them, but improperly, since laws of causation are also laws of phenomena): that is, to uniformities, which either are not laws of causation, or in which the question of causation must for the present remain undecided. The Method of Agreement is chiefly to be resorted to, as a means of suggesting applications of the Method of Difference (as in the last example the comparison of A B C, A D E, A F G, suggested that A was the antecedent on which to try the experiment whether it could produce *a*); or as an inferior resource, in case the Method of Difference is impracticable; which, as we before showed, generally arises from the impossibility of artificially producing the phenomena. And hence it is that the Method of Agreement, though applicable in principle to either case, is more emphatically the method of investigation on those subjects where artificial experimentation is impossible; because on those it is, generally, our only resource of a directly inductive nature; while, [283] in the phenomena which we can produce at pleasure, the Method of Difference generally affords a more efficacious process, which will ascertain causes as well as mere laws.

§ 4. There are, however, many cases in which, though our power of producing the phenomenon is complete, the Method of Difference either can not be made available at all, or not without a previous employment of the Method of Agreement. This occurs when the agency by which we can produce the phenomenon is not that of one single antecedent, but a combination of antecedents, which we have no power of separating from each other, and exhibiting apart. For instance, suppose the subject of inquiry to be the cause of the double refraction of light. We can produce this phenomenon at pleasure, by employing any one of the many substances which are known to refract light in that peculiar manner. But if, taking one of those substances, as Iceland spar, for example, we wish to determine on which of the properties of Iceland spar this remarkable phenomenon depends, we can make no use, for that purpose, of the Method of Difference; for we can not find another substance precisely resembling Iceland spar except in some one property. The only mode, therefore, of prosecuting this inquiry is that afforded by the Method of Agreement; by which, in fact, through a comparison of all the known substances which have the property of doubly refracting light, it was ascertained that they agree in the circumstance of being crystalline substances; and though the converse does not hold, though all crystalline substances have not the property of double refraction, it was concluded, with reason, that there is a real connection between these two properties; that either crystalline structure, or the cause which gives rise to that structure, is one of the conditions of double refraction.

Out of this employment of the Method of Agreement arises a peculiar modification of that method, which is sometimes of great avail in the investigation of nature. In cases similar to the above, in which it is not possible to obtain the precise pair of instances which our second canon requires—instances agreeing in every antecedent except A, or in every consequent except *a*, we may yet be able, by a double employment of the Method of Agreement, to discover in what the instances which contain A or *a* differ from those which do not.

If we compare various instances in which *a* occurs, and find that they all have in common the circumstance A, and (as far as can be observed) no other circumstance, the Method of Agreement, so far, bears testimony to a connection between A and *a*. In order to convert this evidence of connection into proof of causation by the direct Method of Difference, we ought to be able, in some one of these instances, as for example, A B C, to leave out A, and observe whether by doing so, *a* is prevented. Now supposing (what is often the case) that we are not able to try this decisive experiment; yet, provided we can by any means discover what would be its result if we could try it, the advantage will be the same. Suppose, then, that as we previously examined a variety of instances in which *a* occurred, and found them to agree in containing A, so we now observe a variety of instances in which *a* does not occur, and find them agree in not containing A; which establishes, by the Method of Agreement, the same connection between the absence of A and the absence of *a*, which was before established between their presence. As, then, it had been shown that whenever A is present *a* is present, so, it being now shown that when A is taken away a is removed along with [284] it, we have by the one proposition A B C, *a b c*, by the other B C, *b c*, the positive and negative instances which the Method of Difference requires.

This method may be called the Indirect Method of Difference, or the Joint Method of Agreement and Difference; and consists in a double employment of the Method of Agreement, each proof being independent of the other, and corroborating it. But it is not equivalent to a proof by the direct Method of Difference. For the requisitions of the Method of Difference are not satisfied, unless we can be quite sure either that the instances affirmative of *a* agree in no antecedent whatever but A, or that the instances negative of *a* agree in nothing but the negation of A. Now, if it were possible, which it never is, to have this assurance, we should not need the joint method; for either of the two sets of instances separately would then be sufficient to prove causation. This indirect method, therefore, can only be regarded as a great extension and improvement of the Method of Agreement, but not as participating in the more cogent nature of the Method of Difference. The following may be stated as its canon:

THIRD CANON.

If two or more instances in which the phenomenon occurs have only one circumstance in common, while two or more instances in which it does not occur have nothing in common save the absence of that circumstance, the circumstance in which alone the two sets of instances differ, is the effect, or the cause, or an indispensable part of the cause, of the phenomenon.

We shall presently see that the Joint Method of Agreement and Difference constitutes, in another respect not yet adverted to, an improvement upon the common Method of Agreement, namely, in being unaffected by a characteristic imperfection of that method, the nature of which still remains to be pointed out. But as we can not enter into this exposition without introducing a new element of complexity into this long and intricate discussion, I shall postpone it to a subsequent chapter, and shall at once proceed to a statement of two other methods, which will complete the enumeration of the means which mankind possess for exploring the laws of nature by specific observation and experience.

§ 5. The first of these has been aptly denominated the Method of Residues. Its principle is very simple. Subducting from any given phenomenon all the portions which, by virtue of preceding inductions, can be assigned to known causes, the remainder will be the effect of the antecedents which had been overlooked, or of which the effect was as yet an unknown quantity.

Suppose, as before, that we have the antecedents A B C, followed by the consequents *a b c*, and that by previous inductions (founded, we will suppose, on the Method of Difference) we have ascertained the causes of some of these effects, or the effects of some of these causes; and are thence apprised that the effect of A is *a*, and that the effect of B is *b*. Subtracting the sum of these effects from the total phenomenon, there remains *c*, which now, without any fresh experiments, we may know to be the effect of C. This Method of Residues is in truth a peculiar modification of the Method of Difference. If the instance A B C, *a b c*, could have been compared with a single instance A B, *a b*, we should have proved C to be the cause of *c*, by the common process of the Method of Difference. In the present case, however, instead of a
[285] single instance A B, we have had to study separately the causes A and B, and to infer from the effects which they produce separately what effect they must produce in the case A B C, where they act together. Of the two instances, therefore, which the Method of Difference requires—the one positive, the other negative—the negative one, or that in which the given phenomenon is absent, is not the direct result of observation and experiment, but has been arrived at by deduction. As one of the forms of the Method of Difference, the Method of Residues partakes of its rigorous certainty, provided the previous inductions, those which gave

the effects of A and B, were obtained by the same infallible method, and provided we are certain that C is the *only* antecedent to which the residual phenomenon *c* can be referred; the only agent of which we had not already calculated and subducted the effect. But as we can never be quite certain of this, the evidence derived from the Method of Residues is not complete unless we can obtain C artificially, and try it separately, or unless its agency, when once suggested, can be accounted for, and proved deductively from known laws.

Even with these reservations, the Method of Residues is one of the most important among our instruments of discovery. Of all the methods of investigating laws of nature, this is the most fertile in unexpected results: often informing us of sequences in which neither the cause nor the effect were sufficiently conspicuous to attract of themselves the attention of observers. The agent C may be an obscure circumstance, not likely to have been perceived unless sought for, nor likely to have been sought for until attention had been awakened by the insufficiency of the obvious causes to account for the whole of the effect. And *c* may be so disguised by its intermixture with *a* and *b*, that it would scarcely have presented itself spontaneously as a subject of separate study. Of these uses of the method, we shall presently cite some remarkable examples. The canon of the Method of Residues is as follows:

FOURTH CANON.

Subduct from any phenomenon such part as is known by previous inductions to be the effect of certain antecedents, and the residue of the phenomenon is the effect of the remaining antecedents.

§ 6. There remains a class of laws which it is impracticable to ascertain by any of the three methods which I have attempted to characterize: namely, the laws of those Permanent Causes, or indestructible natural agents, which it is impossible either to exclude or to isolate; which we can neither hinder from being

present, nor contrive that they shall be present alone. It would appear at first sight that we could by no means separate the effects of these agents from the effects of those other phenomena with which they can not be prevented from co-existing. In respect, indeed, to most of the permanent causes, no such difficulty exists; since, though we can not eliminate them as co-existing facts, we can eliminate them as influencing agents, by simply trying our experiment in a local situation beyond the limits of their influence. The pendulum, for example, has its oscillations disturbed by the vicinity of a mountain: we remove the pendulum to a sufficient distance from the mountain, and the disturbance ceases: from these data we can determine by the Method of Difference, the amount of effect due to the mountain; and beyond a certain distance every thing goes on precisely as it would do if the mountain exercised no influence whatever, which, accordingly, we, with sufficient reason, conclude to be
[286] the fact.

The difficulty, therefore, in applying the methods already treated of to determine the effects of Permanent Causes, is confined to the cases in which it is impossible for us to get out of the local limits of their influence. The pendulum can be removed from the influence of the mountain, but it can not be removed from the influence of the earth: we can not take away the earth from the pendulum, nor the pendulum from the earth, to ascertain whether it would continue to vibrate if the action which the earth exerts upon it were withdrawn. On what evidence, then, do we ascribe its vibrations to the earth's influence? Not on any sanctioned by the Method of Difference; for one of the two instances, the negative instance, is wanting. Nor by the Method of Agreement; for though all pendulums agree in this, that during their oscillations the earth is always present, why may we not as well ascribe the phenomenon to the sun, which is equally a co-existent fact in all the experiments? It is evident that to establish even so simple a fact of causation as this, there was required some method over and above those which we have yet examined.

As another example, let us take the phenomenon Heat. Independently of all hypothesis as to the real nature of the agency so called, this fact is certain, that we are unable to exhaust any body of the whole of its heat. It is equally certain that no one ever perceived heat not emanating from a body. Being unable, then, to separate Body and Heat, we can not effect such a variation of circumstances as the foregoing three methods require; we can not ascertain, by those methods, what portion of the phenomena exhibited by any body is due to the heat contained in it. If we could observe a body with its heat, and the same body entirely divested of heat, the Method of Difference would show the effect due to the heat, apart from that due to the body. If we could observe heat under circumstances agreeing in nothing but heat, and therefore not characterized also by the presence of a body, we could ascertain the effects of heat, from an instance of heat with a body and an instance of heat without a body, by the Method of Agreement; or we could determine by the Method of Difference what effect was due to the body, when the remainder which was due to the heat would be given by the Method of Residues. But we can do none of these things; and without them the application of any of the three methods to the solution of this problem would be illusory. It would be idle, for instance, to attempt to ascertain the effect of heat by subtracting from the phenomena exhibited by a body all that is due to its other properties; for as we have never been able to observe any bodies without a portion of heat in them, effects due to that heat might form a part of the very results which we were affecting to subtract, in order that the effect of heat might be shown by the residue.

If, therefore, there were no other methods of experimental investigation than these three, we should be unable to determine the effects due to heat as a cause. But we have still a resource.

Though we can not exclude an antecedent altogether, we may be able to produce, or nature may produce for us some modification in it. By a modification is here meant, a change in it not amounting to its total removal. If some modification in the antecedent A is always followed by a change in the consequent *a*, the other consequents *b* and *c* remaining the same; or *vicè versa*, if every change in *a* is found to have been preceded by some modification in A, none being observable in any of the other antecedents, we may safely conclude that *a* is, wholly or in part, an effect traceable to A, or at least in some way connected with it
[287] through causation. For example, in the case of heat, though we can not expel it altogether from any body, we can modify it in quantity, we can increase or diminish it; and doing so, we find by the various methods of experimentation or observation already treated of, that such increase or diminution of heat is followed by expansion or contraction of the body. In this manner we arrive at the conclusion, otherwise unattainable by us, that one of the effects of heat is to enlarge the dimensions of bodies; or, what is the same thing in other words, to widen the distances between their particles.

A change in a thing, not amounting to its total removal, that is, a change which leaves it still the same thing it was, must be a change either in its quantity, or in some of its variable relations to other things, of which variable relations the principal is its position in space. In the previous example, the modification which was produced in the antecedent was an alteration in its quantity. Let us now suppose the question to be, what influence the moon exerts on the surface of the earth. We can not try an experiment in the absence of the moon, so as to observe what terrestrial phenomena her annihilation would put an end to; but when we find that all the variations in the *position* of the moon are followed by corresponding variations in the time and place of high water, the place being always either the part of the earth which is nearest to, or that which is most remote from, the moon, we have ample evidence that the moon is, wholly or partially, the cause which determines the tides. It very commonly happens, as it does in this instance, that the variations of an effect are correspondent, or analogous, to those of its cause; as the moon moves farther toward the east, the high-water point does the same: but this is not an indispensable condition, as may be seen in the same example, for along with that high-water point there is at the same instant another high-water point diametrically opposite to it, and which, therefore, of necessity, moves toward the west, as the moon, followed by the nearer of the tide waves, advances toward the east: and yet both these motions are equally effects of the moon's motion.

That the oscillations of the pendulum are caused by the earth, is proved by similar evidence. Those oscillations take place between equidistant points on the two sides of a line, which, being perpendicular to the earth, varies with every variation in the earth's position, either in space or relatively to the object. Speaking accurately, we only know by the method now characterized, that all terrestrial bodies tend to the earth, and not to some unknown fixed point lying in the same direction. In every twenty-four hours, by the earth's rotation, the line drawn from the body at right angles to the earth coincides successively with all the radii of a circle, and in the course of six months the place of that circle varies by nearly two hundred millions of miles; yet in all these changes of the earth's position, the line in which bodies tend to fall continues to be directed toward it: which proves that terrestrial gravity is directed to the earth, and not, as was once fancied by some, to a fixed point of space.

The method by which these results were obtained may be termed the Method of Concomitant Variations; it is regulated by the following canon:

FIFTH CANON.

Whatever phenomenon varies in any manner whenever another phenomenon varies in some particular manner, is either

a cause or an effect of that phenomenon, or is connected with it
[288] *through some fact of causation.*

The last clause is subjoined, because it by no means follows when two phenomena accompany each other in their variations, that the one is cause and the other effect. The same thing may, and indeed must happen, supposing them to be two different effects of a common cause: and by this method alone it would never be possible to ascertain which of the suppositions is the true one. The only way to solve the doubt would be that which we have so often adverted to, viz., by endeavoring to ascertain whether we can produce the one set of variations by means of the other. In the case of heat, for example, by increasing the temperature of a body we increase its bulk, but by increasing its bulk we do not increase its temperature; on the contrary (as in the rarefaction of air under the receiver of an air-pump), we generally diminish it: therefore heat is not an effect, but a cause, of increase of bulk. If we can not ourselves produce the variations, we must endeavor, though it is an attempt which is seldom successful, to find them produced by nature in some case in which the pre-*existing circumstances are perfectly known to us.

It is scarcely necessary to say, that in order to ascertain the uniform concomitance of variations in the effect with variations in the cause, the same precautions must be used as in any other case of the determination of an invariable sequence. We must endeavor to retain all the other antecedents unchanged, while that particular one is subjected to the requisite series of variations; or, in other words, that we may be warranted in inferring causation from concomitance of variations, the concomitance itself must be proved by the Method of Difference.

It might at first appear that the Method of Concomitant Variations assumes a new axiom, or law of causation in general, namely, that every modification of the cause is followed by a change in the effect. And it does usually happen that when a phenomenon A causes a phenomenon *a*, any variation in the quantity or in the various relations of A, is uniformly followed by a variation in the quantity or relations of *a*. To take a familiar instance, that of gravitation. The sun causes a certain tendency to motion in the earth; here we have cause and effect; but that tendency is *toward* the sun, and therefore varies in direction as the sun varies in the relation of position; and, moreover, the tendency varies in intensity, in a certain numerical correspondence to the sun's distance from the earth, that is, according to another relation of the sun. Thus we see that there is not only an invariable connection between the sun and the earth's gravitation, but that two of the relations of the sun, its position with respect to the earth and its distance from the earth, are invariably connected as antecedents with the quantity and direction of the earth's gravitation. The cause of the earth's gravitating at all, is simply the sun; but the cause of its gravitating with a given intensity and in a given direction, is the existence of the sun in a given direction and at a given distance. It is not strange that a modified cause, which is in truth a different cause, should produce a different effect.

Although it is for the most part true that a modification of the cause is followed by a modification of the effect, the Method of Concomitant Variations does not, however, presuppose this as an axiom. It only requires the converse proposition: that any thing on whose modifications, modifications of an effect are invariably consequent, must be the cause (or connected with the cause) of that effect; a proposition, the truth of which is evident; for if the thing itself had no influence on the effect, neither could the modifications of the thing have any influence. If the stars have no power over the fortunes of mankind, it is implied in the very [289] terms that the conjunctions or oppositions of different stars can have no such power.

Although the most striking applications of the Method of Concomitant Variations take place in the cases in which the

Method of Difference, strictly so called, is impossible, its use is not confined to those cases; it may often usefully follow after the Method of Difference, to give additional precision to a solution which that has found. When by the Method of Difference it has first been ascertained that a certain object produces a certain effect, the Method of Concomitant Variations may be usefully called in, to determine according to what law the quantity or the different relations of the effect follow those of the cause.

§ 7. The case in which this method admits of the most extensive employment, is that in which the variations of the cause are variations of quantity. Of such variations we may in general affirm with safety, that they will be attended not only with variations, but with similar variations, of the effect: the proposition that more of the cause is followed by more of the effect, being a corollary from the principle of the Composition of Causes, which, as we have seen, is the general rule of causation; cases of the opposite description, in which causes change their properties on being conjoined with one another, being, on the contrary, special and exceptional. Suppose, then, that when A changes in quantity, *a* also changes in quantity, and in such a manner that we can trace the numerical relation which the changes of the one bear to such changes of the other as take place within our limits of observation. We may then, with certain precautions, safely conclude that the same numerical relation will hold beyond those limits. If, for instance, we find that when A is double, *a* is double; that when A is treble or quadruple, *a* is treble or quadruple; we may conclude that if A were a half or a third, *a* would be a half or a third, and finally, that if A were annihilated, *a* would be annihilated; and that *a* is wholly the effect of A, or wholly the effect of the same cause with A. And so with any other numerical relation according to which A and *a* would vanish simultaneously; as, for instance, if *a* were proportional to the square of A. If, on the other hand, *a* is not wholly the effect of A, but yet varies when A varies, it is probably a mathematical function not of A alone, but of A and something else: its changes, for example, may be such as would occur if part of it remained constant, or varied on some other principle, and the remainder varied in some numerical relations to the variations of A. In that case, when A diminishes, *a* will be seen to approach not toward zero, but toward some other limit; and when the series of variations is such as to indicate what that limit is, if constant, or the law of its variation, if variable, the limit will exactly measure how much of *a* is the effect of some other and independent cause, and the remainder will be the effect of A (or of the cause of A).

These conclusions, however, must not be drawn without certain precautions. In the first place, the possibility of drawing them at all, manifestly supposes that we are acquainted not only with the variations, but with the absolute quantities both of A and *a*. If we do not know the total quantities, we can not, of course, determine the real numerical relation according to which those quantities vary. It is, therefore, an error to conclude, as some have concluded, that because increase of heat expands bodies, that is, increases the distance between their particles, therefore the distance is wholly the effect of heat, and that if we could entirely exhaust the body of its heat, the particles would be [290] in complete contact. This is no more than a guess, and of the most hazardous sort, not a legitimate induction: for since we neither know how much heat there is in any body, nor what is the real distance between any two of its particles, we can not judge whether the contraction of the distance does or does not follow the diminution of the quantity of heat according to such a numerical relation that the two quantities would vanish simultaneously.

In contrast with this, let us consider a case in which the absolute quantities are known; the case contemplated in the first law of motion: viz., that all bodies in motion continue to move in a straight line with uniform velocity until acted upon by some new force. This assertion is in open opposition to first appearances; all terrestrial objects, when in motion, gradually abate their velocity, and at last stop; which accordingly the ancients, with their *inductio per enumerationem simplicem,* imagined to be the law. Every moving body, however, encounters various obstacles, as friction, the resistance of the atmosphere, etc., which we know by daily experience to be causes capable of destroying motion. It was suggested that the whole of the retardation might be owing to these causes. How was this inquired into? If the obstacles could have been entirely removed, the case would have been amenable to the Method of Difference. They could not be removed, they could only be diminished, and the case, therefore, admitted only of the Method of Concomitant Variations. This accordingly being employed, it was found that every diminution of the obstacles diminished the retardation of the motion: and inasmuch as in this case (unlike the case of heat) the total quantities both of the antecedent and of the consequent were known, it was practicable to estimate, with an approach to accuracy, both the amount of the retardation and the amount of the retarding causes, or resistances, and to judge how near they both were to being exhausted; and it appeared that the effect dwindled as rapidly, and at each step was as far on the road toward annihilation, as the cause was. The simple oscillation of a weight suspended from a fixed point, and moved a little out of the perpendicular, which in ordinary circumstances lasts but a few minutes, was prolonged in Borda's experiments to more than thirty hours, by diminishing as much as possible the friction at the point of suspension, and by making the body oscillate in a space exhausted as nearly as possible of its air. There could therefore be no hesitation in assigning the whole of the retardation of motion to the influence of the obstacles; and since, after subducting this retardation from the total phenomenon, the remainder was a uniform velocity, the result was the proposition known as the first law of motion.

There is also another characteristic uncertainty affecting the

inference that the law of variation which the quantities observe within our limits of observation, will hold beyond those limits. There is, of course, in the first instance, the possibility that beyond the limits, and in circumstances therefore of which we have no direct experience, some counteracting cause might develop itself; either a new agent or a new property of the agents concerned, which lies dormant in the circumstances we are able to observe. This is an element of uncertainty which enters largely into all our predictions of effects; but it is not peculiarly applicable to the Method of Concomitant Variations. The uncertainty, however, of which I am about to speak, is characteristic of that method; especially in the cases in which the extreme limits of our observation are very narrow, in comparison with the possible variations in the quantities of the phenomena. Any one who [291]
has the slightest acquaintance with mathematics, is aware that very different laws of variation may produce numerical results which differ but slightly from one another within narrow limits; and it is often only when the absolute amounts of variation are considerable, that the difference between the results given by one law and by another becomes appreciable. When, therefore, such variations in the quantity of the antecedents as we have the means of observing are small in comparison with the total quantities, there is much danger lest we should mistake the numerical law, and be led to miscalculate the variations which would take place beyond the limits; a miscalculation which would vitiate any conclusion respecting the dependence of the effect upon the cause, that could be founded on those variations. Examples are not wanting of such mistakes. "The formulæ," says Sir John Herschel,[136] "which have been empirically deduced for the elasticity of steam (till very recently), and those for the resistance of fluids, and other similar subjects," when relied on beyond the limits of the observations from which they were

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[136] *Discourse on the Study of Natural Philosophy*, p. 179.

deduced, "have almost invariably failed to support the theoretical structures which have been erected on them."

In this uncertainty, the conclusion we may draw from the concomitant variations of *a* and A, to the existence of an invariable and exclusive connection between them, or to the permanency of the same numerical relation between their variations when the quantities are much greater or smaller than those which we have had the means of observing, can not be considered to rest on a complete induction. All that in such a case can be regarded as proved on the subject of causation is, that there is some connection between the two phenomena; that A, or something which can influence A, must be *one* of the causes which collectively determine *a*. We may, however, feel assured that the relation which we have observed to exist between the variations of A and *a*, will hold true in all cases which fall between the same extreme limits; that is, wherever the utmost increase or diminution in which the result has been found by observation to coincide with the law, is not exceeded.

The four methods which it has now been attempted to describe, are the only possible modes of experimental inquiry—of direct induction *a posteriori*, as distinguished from deduction: at least, I know not, nor am able to imagine any others. And even of these, the Method of Residues, as we have seen, is not independent of deduction; though, as it also requires specific experience, it may, without impropriety, be included among methods of direct observation and experiment.

These, then, with such assistance as can be obtained from Deduction, compose the available resources of the human mind for ascertaining the laws of the succession of phenomena. Before proceeding to point out certain circumstances by which the employment of these methods is subjected to an immense increase of complication and of difficulty, it is expedient to illustrate the use of the methods, by suitable examples drawn from actual physical investigations. These, accordingly, will form the subject of the succeeding chapter.

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Chapter IX.

Miscellaneous Examples Of The Four Methods.

§ 1. I shall select, as a first example, an interesting speculation of one of the most eminent of theoretical chemists, Baron Liebig. The object in view is to ascertain the immediate cause of the death produced by metallic poisons.

Arsenious acid, and the salts of lead, bismuth, copper, and mercury, if introduced into the animal organism, except in the smallest doses, destroy life. These facts have long been known, as insulated truths of the lowest order of generalization; but it was reserved for Liebig, by an apt employment of the first two of our methods of experimental inquiry, to connect these truths together by a higher induction, pointing out what property, common to all these deleterious substances, is the really operating cause of their fatal effect.

When solutions of these substances are placed in sufficiently close contact with many animal products, albumen, milk, muscular fibre, and animal membranes, the acid or salt leaves the water in which it was dissolved, and enters into combination with the animal substance, which substance, after being thus acted upon, is found to have lost its tendency to spontaneous decomposition, or putrefaction.

Observation also shows, in cases where death has been produced by these poisons, that the parts of the body with which the poisonous substances have been brought into contact, do not afterward putrefy.

And, finally, when the poison has been supplied in too small a quantity to destroy life, eschars are produced, that is, certain superficial portions of the tissues are destroyed, which are afterward thrown off by the reparative process taking place in the healthy parts.

These three sets of instances admit of being treated according to the Method of Agreement. In all of them the metallic compounds are brought into contact with the substances which compose the human or animal body; and the instances do not seem to agree in any other circumstance. The remaining antecedents are as different, and even opposite, as they could possibly be made; for in some the animal substances exposed to the action of the poisons are in a state of life, in others only in a state of organization, in others not even in that. And what is the result which follows in all the cases? The conversion of the animal substance (by combination with the poison) into a chemical compound, held together by so powerful a force as to resist the subsequent action of the ordinary causes of decomposition. Now, organic life (the necessary condition of sensitive life) consisting in a continual state of decomposition and recomposition of the different organs and tissues, whatever incapacitates them for this decomposition destroys life. And thus the proximate cause of the death produced by this description of poisons is ascertained, as far as the Method of Agreement can ascertain it.

Let us now bring our conclusion to the test of the Method of Difference. Setting out from the cases already mentioned, in which the antecedent is the presence of substances forming with
[293] the tissues a compound incapable of putrefaction, (and *a fortiori* incapable of the chemical actions which constitute life), and the consequent is death, either of the whole organism, or of some

portion of it; let us compare with these cases other cases, as much resembling them as possible, but in which that effect is not produced. And, first, “many insoluble basic salts of arsenious acid are known not to be poisonous. The substance called alkargen, discovered by Bunsen, which contains a very large quantity of arsenic, and approaches very closely in composition to the organic arsenious compounds found in the body, has not the slightest injurious action upon the organism.” Now when these substances are brought into contact with the tissues in any way, they do not combine with them; they do not arrest their progress to decomposition. As far, therefore, as these instances go, it appears that when the effect is absent, it is by reason of the absence of that antecedent which we had already good ground for considering as the proximate cause.

But the rigorous conditions of the Method of Difference are not yet satisfied; for we can not be sure that these unpoisonous bodies agree with the poisonous substances in every property, except the particular one of entering into a difficultly decomposable compound with the animal tissues. To render the method strictly applicable, we need an instance, not of a different substance, but of one of the very same substances, in circumstances which would prevent it from forming, with the tissues, the sort of compound in question; and then, if death does not follow, our case is made out. Now such instances are afforded by the antidotes to these poisons. For example, in case of poisoning by arsenious acid, if hydrated peroxide of iron is administered, the destructive agency is instantly checked. Now this peroxide is known to combine with the acid, and form a compound, which, being insoluble, can not act at all on animal tissues. So, again, sugar is a well-known antidote to poisoning by salts of copper; and sugar reduces those salts either into metallic copper, or into the red sub-oxide, neither of which enters into combination with animal matter. The disease called painter's colic, so common in manufactories of white-lead, is unknown where the workmen are accustomed to take, as a preservative, sulphuric acid lemonade (a solution of sugar rendered acid by sulphuric acid). Now diluted sulphuric acid has the property of decomposing all compounds of lead with organic matter, or of preventing them from being formed.

There is another class of instances, of the nature required by the Method of Difference, which seem at first sight to conflict with the theory. Soluble salts of silver, such for instance as the nitrate, have the same stiffening antiseptic effect on decomposing animal substances as corrosive sublimate and the most deadly metallic poisons; and when applied to the external parts of the body, the nitrate is a powerful caustic, depriving those parts of all active vitality, and causing them to be thrown off by the neighboring living structures, in the form of an eschar. The nitrate and the other salts of silver ought, then, it would seem, if the theory be correct, to be poisonous; yet they may be administered internally with perfect impunity. From this apparent exception arises the strongest confirmation which the theory has yet received. Nitrate of silver, in spite of its chemical properties, does not poison when introduced into the stomach; but in the stomach, as in all animal liquids, there is common salt; and in the stomach there is also free muriatic acid. These substances operate as natural antidotes, combining with the nitrate, and if its quantity is not too great, immediately converting it into chloride of silver, a substance very
[294] slightly soluble, and therefore incapable of combining with the tissues, although to the extent of its solubility it has a medicinal influence, though an entirely different class of organic actions.

The preceding instances have afforded an induction of a high order of conclusiveness, illustrative of the two simplest of our four methods; though not rising to the maximum of certainty which the Method of Difference, in its most perfect exemplification, is capable of affording. For (let us not forget) the positive instance and the negative one which the rigor of that method requires, ought to differ only in the presence or absence of one single circumstance. Now, in the preceding argument, they differ in the presence or absence not of a single *circumstance*, but of a single *substance*: and as every substance has innumerable properties, there is no knowing what number of real differences are involved in what is nominally and apparently only one difference. It is conceivable that the antidote, the peroxide of iron for example, may counteract the poison through some other of its properties than that of forming an insoluble compound with it; and if so, the theory would fall to the ground, so far as it is supported by that instance. This source of uncertainty, which is a serious hinderance to all extensive generalizations in chemistry, is, however, reduced in the present case to almost the lowest degree possible, when we find that not only one substance, but many substances, possess the capacity of acting as antidotes to metallic poisons, and that all these agree in the property of forming insoluble compounds with the poisons, while they can not be ascertained to agree in any other property whatsoever. We have thus, in favor of the theory, all the evidence which can be obtained by what we termed the Indirect Method of Difference, or the Joint Method of Agreement and Difference; the evidence of which, though it never can amount to that of the Method of Difference properly so called, may approach indefinitely near to it.

§ 2. Let the object be[137] to ascertain the law of what is termed *induced* electricity; to find under what conditions any electrified body, whether positively or negatively electrified, gives rise to a contrary electric state in some other body adjacent to it.

The most familiar exemplification of the phenomenon to be investigated is the following. Around the prime conductors of an electrical machine the atmosphere to some distance, or any conducting surface suspended in that atmosphere, is found to be

[137] For this speculation, as for many other of my scientific illustrations, I am indebted to Professor Bain, whose subsequent treatise on Logic abounds with apt illustrations of all the inductive methods.

in an electric condition opposite to that of the prime conductor itself. Near and around the positive prime conductor there is negative electricity, and near and around the negative prime conductor there is positive electricity. When pith balls are brought near to either of the conductors, they become electrified with the opposite electricity to it; either receiving a share from the already electrified atmosphere by conduction, or acted upon by the direct inductive influence of the conductor itself: they are then attracted by the conductor to which they are in opposition; or, if withdrawn in their electrified state, they will be attracted by any other oppositely charged body. In like manner the hand, if brought near enough to the conductor, receives or gives an electric discharge; now we have no evidence that a charged conductor can be suddenly discharged unless by the approach
[295] of a body oppositely electrified. In the case, therefore, of the electric machine, it appears that the accumulation of electricity in an insulated conductor is always accompanied by the excitement of the contrary electricity in the surrounding atmosphere, and in every conductor placed near the former conductor. It does not seem possible, in this case, to produce one electricity by itself.

Let us now examine all the other instances which we can obtain, resembling this instance in the given consequent, namely, the evolution of an opposite electricity in the neighborhood of an electrified body. As one remarkable instance we have the Leyden jar; and after the splendid experiments of Faraday in complete and final establishment of the substantial identity of magnetism and electricity, we may cite the magnet, both the natural and the electro-magnet, in neither of which it is possible to produce one kind of electricity by itself, or to charge one pole without charging an opposite pole with the contrary electricity at the same time. We can not have a magnet with one pole: if we break a natural loadstone into a thousand pieces, each piece will have its two oppositely electrified poles complete within itself. In the voltaic circuit, again, we can not have one current without its opposite. In the ordinary electric machine, the glass cylinder or plate, and the rubber, acquire opposite electricities.

From all these instances, treated by the Method of Agreement, a general law appears to result. The instances embrace all the known modes in which a body can become charged with electricity; and in all of them there is found, as a concomitant or consequent, the excitement of the opposite electric state in some other body or bodies. It seems to follow that the two facts are invariably connected, and that the excitement of electricity in any body has for one of its necessary conditions the possibility of a simultaneous excitement of the opposite electricity in some neighboring body.

As the two contrary electricities can only be produced together, so they can only cease together. This may be shown by an application of the Method of Difference to the example of the Leyden jar. It needs scarcely be here remarked that in the Leyden jar, electricity can be accumulated and retained in considerable quantity, by the contrivance of having two conducting surfaces of equal extent, and parallel to each other through the whole of that extent, with a non-conducting substance such as glass between them. When one side of the jar is charged positively, the other is charged negatively, and it was by virtue of this fact that the Leyden jar served just now as an instance in our employment of the Method of Agreement. Now it is impossible to discharge one of the coatings unless the other can be discharged at the same time. A conductor held to the positive side can not convey away any electricity unless an equal quantity be allowed to pass from the negative side: if one coating be perfectly insulated, the charge is safe. The dissipation of one must proceed *pari passu* with that of the other.

The law thus strongly indicated admits of corroboration by the Method of Concomitant Variations. The Leyden jar is capable of receiving a much higher charge than can ordinarily be given to the conductor of an electrical machine. Now in the case of

the Leyden jar, the metallic surface which receives the induced electricity is a conductor exactly similar to that which receives the primary charge, and is therefore as susceptible of receiving and retaining the one electricity, as the opposite surface of receiving and retaining the other; but in the machine, the neighboring body which is to be oppositely electrified is the surrounding atmosphere, or any body casually brought near to the conductor;
[296] and as these are generally much inferior in their capacity of becoming electrified, to the conductor itself, their limited power imposes a corresponding limit to the capacity of the conductor for being charged. As the capacity of the neighboring body for supporting the opposition increases, a higher charge becomes possible: and to this appears to be owing the great superiority of the Leyden jar.

A further and most decisive confirmation by the Method of Difference, is to be found in one of Faraday's experiments in the course of his researches on the subject of Induced Electricity.

Since common or machine electricity, and voltaic electricity, may be considered for the present purpose to be identical, Faraday wished to know whether, as the prime conductor develops opposite electricity upon a conductor in its vicinity, so a voltaic current running along a wire would induce an opposite current upon another wire laid parallel to it at a short distance. Now this case is similar to the cases previously examined, in every circumstance except the one to which we have ascribed the effect. We found in the former instances that whenever electricity of one kind was excited in one body, electricity of the opposite kind must be excited in a neighboring body. But in Faraday's experiment this indispensable opposition exists within the wire itself. From the nature of a voltaic charge, the two opposite currents necessary to the existence of each other are both accommodated in one wire; and there is no need of another wire placed beside it to contain one of them, in the same way as the Leyden jar must have a positive and a negative surface. The exciting cause can and does produce all the effect which its laws require, independently of any electric excitement of a neighboring body. Now the result of the experiment with the second wire was, that no opposite current was produced. There was an instantaneous effect at the closing and breaking of the voltaic circuit; electric inductions appeared when the two wires were moved to and from one another; but these are phenomena of a different class. There was no induced electricity in the sense in which this is predicated of the Leyden jar; there was no sustained current running up the one wire while an opposite current ran down the neighboring wire; and this alone would have been a true parallel case to the other.

It thus appears by the combined evidence of the Method of Agreement, the Method of Concomitant Variations, and the most rigorous form of the Method of Difference, that neither of the two kinds of electricity can be excited without an equal excitement of the other and opposite kind: that both are effects of the same cause; that the possibility of the one is a condition of the possibility of the other, and the quantity of the one an impassable limit to the quantity of the other. A scientific result of considerable interest in itself, and illustrating those three methods in a manner both characteristic and easily intelligible.[138]

§ 3. Our third example shall be extracted from Sir John Herschel's *Discourse course on the Study of Natural Philosophy*, [297]
a work replete with happily-selected exemplifications of

[138] This view of the necessary co-existence of opposite excitements involves a great extension of the original doctrine of two electricities. The early theorists assumed that, when amber was rubbed, the amber was made positive and the rubber negative to the same degree; but it never occurred to them to suppose that the existence of the amber charge was dependent on an opposite charge in the bodies with which the amber was contiguous, while the existence of the negative charge on the rubber was equally dependent on a contrary state of the surfaces that might accidentally be confronted with it; that, in fact, in a case of electrical excitement by friction, four charges were the minimum that could exist. But this double electrical action is essentially implied in the explanation now universally adopted in regard to the phenomena of the common electric machine.

inductive processes from almost every department of physical science, and in which alone, of all books which I have met with, the four methods of induction are distinctly recognized, though not so clearly characterized and defined, nor their correlation so fully shown, as has appeared to me desirable. The present example is described by Sir John Herschel as "one of the most beautiful specimens" which can be cited "of inductive experimental inquiry lying within a moderate compass;" the theory of dew, first promulgated by the late Dr. Wells, and now universally adopted by scientific authorities. The passages in inverted commas are extracted verbatim from the Discourse.[139]

"Suppose *dew* were the phenomenon proposed, whose cause we would know. In the first place" we must determine precisely what we mean by dew: what the fact really is whose cause we desire to investigate. "We must separate dew from rain, and the moisture of fogs, and limit the application of the term to what is really meant, which is the spontaneous appearance of moisture on substances exposed in the open air when no rain or *visible* wet is falling." This answers to a preliminary operation which will be characterized in the ensuing book, treating of operations subsidiary to induction.[140]

"Now, here we have analogous phenomena in the moisture which bedews a cold metal or stone when we breathe upon it; that which appears on a glass of water fresh from the well in hot weather; that which appears on the inside of windows when sudden rain or hail chills the external air; that which runs down our walls when, after a long frost, a warm, moist thaw comes on." Comparing these cases, we find that they all contain the phenomenon which was proposed as the subject of investigation. Now "all these instances agree in one point, the coldness of the object dewed, in comparison with the air in contact with it." But there still remains the most important case of all, that of nocturnal

[139] Pp. 110, 111.
[140] Infra, book iv., chap. ii., On Abstraction.

dew: does the same circumstance exist in this case? “Is it a fact that the object dewed *is* colder than the air? Certainly not, one would at first be inclined to say; for what is to *make* it so? But ... the experiment is easy: we have only to lay a thermometer in contact with the dewed substance, and hang one at a little distance above it, out of reach of its influence. The experiment has been therefore made, the question has been asked, and the answer has been invariably in the affirmative. Whenever an object contracts dew, it *is* colder than the air.”

Here, then, is a complete application of the Method of Agreement, establishing the fact of an invariable connection between the deposition of dew on a surface, and the coldness of that surface compared with the external air. But which of these is cause, and which effect? or are they both effects of something else? On this subject the Method of Agreement can afford us no light: we must call in a more potent method. “We must collect more facts, or, which comes to the same thing, vary the circumstances; since every instance in which the circumstances differ is a fresh fact: and especially, we must note the contrary or negative cases, *i.e.*, where no dew is produced:” a comparison between instances of dew and instances of no dew, being the condition necessary to bring the Method of Difference into play.

“Now, first, no dew is produced on the surface of polished metals, but it *is* very copiously on glass, both exposed with their [298] faces upward, and in some cases the under side of a horizontal plate of glass is also dewed.” Here is an instance in which the effect is produced, and another instance in which it is not produced; but we can not yet pronounce, as the canon of the Method of Difference requires, that the latter instance agrees with the former in all its circumstances except one; for the differences between glass and polished metals are manifold, and the only thing we can as yet be sure of is, that the cause of dew will be found among the circumstances by which the former substance is distinguished from the latter. But if we could be sure that glass, and the various other substances on which dew is deposited, have only one quality in common, and that polished metals and the other substances on which dew is not deposited, have also nothing in common but the one circumstance of not having the one quality which the others have; the requisitions of the Method of Difference would be completely satisfied, and we should recognize, in that quality of the substances, the cause of dew. This, accordingly, is the path of inquiry which is next to be pursued.

"In the cases of polished metal and polished glass, the contrast shows evidently that the *substance* has much to do with the phenomenon; therefore let the substance *alone* be diversified as much as possible, by exposing polished surfaces of various kinds. This done, a *scale of intensity* becomes obvious. Those polished substances are found to be most strongly dewed which conduct heat worst; while those which conduct heat well, resist dew most effectually." The complication increases; here is the Method of Concomitant Variations called to our assistance; and no other method was practicable on this occasion; for the quality of conducting heat could not be excluded, since all substances conduct heat in some degree. The conclusion obtained is, that *cæteris paribus* the deposition of dew is in some proportion to the power which the body possesses of resisting the passage of heat; and that this, therefore (or something connected with this), must be at least one of the causes which assist in producing the deposition of dew on the surface.

"But if we expose rough surfaces instead of polished, we sometimes find this law interfered with. Thus, roughened iron, especially if painted over or blackened, becomes dewed sooner than varnished paper; the kind of *surface*, therefore, has a great influence. Expose, then, the *same* material in very diversified states, as to surface" (that is, employ the Method of Difference to ascertain concomitance of variations), "and another scale of intensity becomes at once apparent; those *surfaces* which *part* *with their heat* most readily by radiation are found to contract dew most copiously." Here, therefore, are the requisites for a second employment of the Method of Concomitant Variations; which in this case also is the only method available, since all substances radiate heat in some degree or other. The conclusion obtained by this new application of the method is, that *cæteris paribus* the deposition of dew is also in some proportion to the power of radiating heat; and that the quality of doing this abundantly (or some cause on which that quality depends) is another of the causes which promote the deposition of dew on the substance.

"Again, the influence ascertained to exist of *substance* and *surface* leads us to consider that of *texture*: and here, again, we are presented on trial with remarkable differences, and with a third scale of intensity, pointing out substances of a close, firm texture, such as stones, metals, etc., as unfavorable, but those of a loose one, as cloth, velvet, wool, eider-down, cotton, etc., as eminently favorable to the contraction of dew." The [299] Method of Concomitant Variations is here, for the third time, had recourse to; and, as before, from necessity, since the texture of no substance is absolutely firm or absolutely loose. Looseness of texture, therefore, or something which is the cause of that quality, is another circumstance which promotes the deposition of dew; but this third course resolves itself into the first, viz., the quality of resisting the passage of heat: for substances of loose texture "are precisely those which are best adapted for clothing, or for impeding the free passage of heat from the skin into the air, so as to allow their outer surfaces to be very cold, while they remain warm within;" and this last is, therefore, an induction (from fresh instances) simply *corroborative* of a former induction.

It thus appears that the instances in which much dew is deposited, which are very various, agree in this, and, so far as we are able to observe, in this only, that they either radiate heat rapidly or conduct it slowly: qualities between which there is no other circumstance of agreement than that by virtue of either, the body tends to lose heat from the surface more rapidly than it can be restored from within. The instances, on the contrary, in which no dew, or but a small quantity of it, is formed, and which are also extremely various, agree (as far as we can observe) in nothing except in *not* having this same property. We seem, therefore, to have detected the characteristic difference between the substances on which dew is produced and those on which it is not produced. And thus have been realized the requisitions of what we have termed the Indirect Method of Difference, or the Joint Method of Agreement and Difference. The example afforded of this indirect method, and of the manner in which the data are prepared for it by the Methods of Agreement and of Concomitant Variations, is the most important of all the illustrations of induction afforded by this interesting speculation.

We might now consider the question, on what the deposition of dew depends, to be completely solved, if we could be quite sure that the substances on which dew is produced differ from those on which it is not, in *nothing* but in the property of losing heat from the surface faster than the loss can be repaired from within. And though we never can have that complete certainty, this is not of so much importance as might at first be supposed; for we have, at all events, ascertained that even if there be any other quality hitherto unobserved which is present in all the substances which contract dew, and absent in those which do not, this other property must be one which, in all that great number of substances, is present or absent exactly where the property of being a better radiator than conductor is present or absent; an extent of coincidence which affords a strong presumption of a community of cause, and a consequent invariable co-existence between the two properties; so that the property of being a better radiator than conductor, if not itself the cause, almost certainly always accompanies the cause, and for purposes of prediction, no error is likely to be committed by treating it as if it were really such.

Reverting now to an earlier stage of the inquiry, let us remember that we had ascertained that, in every instance where dew is formed, there is actual coldness of the surface below the temperature of the surrounding air; but we were not sure whether this coldness was the cause of dew, or its effect. This doubt we are now able to resolve. We have found that, in every such instance, the substance is one which, by its own properties or laws, would, if exposed in the night, become colder than the surrounding air. The coldness, therefore, being accounted [300]
for independently of the dew, while it is proved that there is a connection between the two, it must be the dew which depends on the coldness; or, in other words, the coldness is the cause of the dew.

This law of causation, already so amply established, admits, however, of efficient additional corroboration in no less than three ways. First, by deduction from the known laws of aqueous vapor when diffused through air or any other gas; and though we have not yet come to the Deductive Method, we will not omit what is necessary to render this speculation complete. It is known by direct experiment that only a limited quantity of water can remain suspended in the state of vapor at each degree of temperature, and that this maximum grows less and less as the temperature diminishes. From this it follows, deductively, that if there is already as much vapor suspended as the air will contain at its existing temperature, any lowering of that temperature will cause a portion of the vapor to be condensed, and become water. But again, we know deductively, from the laws of heat, that the contact of the air with a body colder than itself will necessarily lower the temperature of the stratum of air immediately applied to its surface; and will, therefore, cause it to part with a portion of its water, which accordingly will, by the ordinary laws of gravitation or cohesion, attach itself to the surface of the body, thereby constituting dew. This deductive proof, it will have been seen, has the advantage of at once proving causation as well as co-existence; and it has the additional advantage that it also accounts for the exceptions to the occurrence of the phenomenon, the cases in which, although the body is colder than the air, yet no dew is deposited; by showing that this will necessarily be the case when the air is so under-supplied with aqueous vapor, comparatively to its temperature, that even when somewhat cooled by the contact of the colder body it can still continue to hold in suspension all the vapor which was previously suspended in it: thus in a very dry summer there are no dews, in a very dry winter no hoar-frost. Here, therefore, is an additional condition of the production of dew, which the methods we previously made use of failed to detect, and which might have remained still undetected, if recourse had not been had to the plan of deducing the effect from the ascertained properties of the agents known to be present.

The second corroboration of the theory is by direct experiment, according to the canon of the Method of Difference. We can, by cooling the surface of any body, find in all cases some temperature (more or less inferior to that of the surrounding air, according to its hygrometric condition) at which dew will begin to be deposited. Here, too, therefore, the causation is directly proved. We can, it is true, accomplish this only on a small scale, but we have ample reason to conclude that the same operation, if conducted in nature's great laboratory, would equally produce the effect.

And, finally, even on that great scale we are able to verify the result. The case is one of those rare cases, as we have shown them to be, in which nature works the experiment for us in the same manner in which we ourselves perform it; introducing into the previous state of things a single and perfectly definite new circumstance, and manifesting the effect so rapidly that there is not time for any other material change in the pre-existing circumstances. “It is observed that dew is never copiously deposited in situations much screened from the open sky, and not at all in a cloudy night; but *if the clouds withdraw even for a few minutes, and leave a clear opening, a deposition of dew presently begins*, and goes on increasing... Dew formed in clear intervals [301] will often even evaporate again when the sky becomes thickly overcast." The proof, therefore, is complete, that the presence or absence of an uninterrupted communication with the sky causes the deposition or non-deposition of dew. Now, since a clear sky is nothing but the absence of clouds, and it is a known property of clouds, as of all other bodies between which and any given object nothing intervenes but an elastic fluid, that they tend to raise or keep up the superficial temperature of the object by radiating heat to it, we see at once that the disappearance of clouds will cause the surface to cool; so that nature, in this case, produces a change in the antecedent by definite and known means, and the consequent follows accordingly: a natural experiment which satisfies the requisitions of the Method of Difference.[141]

The accumulated proof of which the Theory of Dew has

[141] I must, however, remark, that this example, which seems to militate against the assertion we made of the comparative inapplicability of the Method of Difference to cases of pure observation, is really one of those exceptions which, according to a proverbial expression, prove the general rule. For in this case, in which Nature, in her experiment, seems to have imitated the type of the experiments made by man, she has only succeeded in producing the likeness of man's most imperfect experiments; namely, those in which, though he succeeds in producing the phenomenon, he does so by employing complex means, which he is unable perfectly to analyze, and can form, therefore, no sufficient judgment what portion of the effects may be due, not to the supposed cause, but to some unknown agency of the means by which that cause was produced. In the natural experiment which we are speaking of, the means used was the clearing off a canopy of clouds; and we certainly do not know sufficiently in what this process consists, or on what it depends, to be certain *a priori* that it might not operate upon the deposition of dew independently of any thermometric effect at the earth's surface. Even, therefore, in a case so favorable as this to Nature's experimental talents, her experiment is of little value except in corroboration of a conclusion already attained through other means.

been found susceptible, is a striking instance of the fullness of assurance which the inductive evidence of laws of causation may attain, in cases in which the invariable sequence is by no means obvious to a superficial view.

§ 4. The admirable physiological investigations of Dr. Brown-Séquard afford brilliant examples of the application of the Inductive Methods to a class of inquiries in which, for reasons which will presently be given, direct induction takes place under peculiar difficulties and disadvantages. As one of the most apt instances, I select his speculation (in the proceedings of the Royal Society for May 16, 1861) on the relations between muscular irritability, cadaveric rigidity, and putrefaction.

The law which Dr. Brown-Séquard's investigation tends to establish, is the following: "The greater the degree of muscular irritability at the time of death, the later the cadaveric rigidity sets in, and the longer it lasts, and the later also putrefaction appears, and the slower it progresses." One would say at first sight that the method here required must be that of Concomitant Variations. But this is a delusive appearance, arising from the circumstance that the conclusion to be tested is itself a fact of concomitant variations. For the establishment of that fact any of the Methods may be put in requisition, and it will be found that the fourth Method, though really employed, has only a subordinate place in this particular investigation.

The evidences by which Dr. Brown-Séquard establishes the law may be enumerated as follows:

1st. Paralyzed muscles have greater irritability than healthy muscles. Now, paralyzed muscles are later in assuming the cadaveric rigidity than healthy muscles, the rigidity lasts longer,
[302] and putrefaction sets in later, and proceeds more slowly.

Both these propositions had to be proved by experiment; and for the experiments which prove them, science is also indebted to Dr. Brown-Séquard. The former of the two—that paralyzed muscles have greater irritability than healthy muscles—he ascertained in various ways, but most decisively by “comparing the duration of irritability in a paralyzed muscle and in the corresponding healthy one of the opposite side, while they are both submitted to the same excitation.” He “often found, in experimenting in that way, that the paralyzed muscle remained irritable twice, three times, or even four times as long as the healthy one.” This is a case of induction by the Method of Difference. The two limbs, being those of the same animal, were presumed to differ in no circumstance material to the case except the paralysis, to the presence and absence of which, therefore, the difference in the muscular irritability was to be attributed. This assumption of complete resemblance in all material circumstances save one, evidently could not be safely made in any one pair of experiments, because the two legs of any given animal might be accidentally in very different pathological conditions; but if, besides taking pains to avoid any such difference, the experiment was repeated sufficiently often in different animals to exclude the supposition that any abnormal circumstance could be present in them all, the conditions of the Method of Difference were adequately secured.

In the same manner in which Dr. Brown-Séquard proved that paralyzed muscles have greater irritability, he also proved the correlative proposition respecting cadaveric rigidity and putrefaction. Having, by section of the roots of the sciatic nerve, and again of a lateral half of the spinal cord, produced paralysis in one hind leg of an animal while the other remained healthy, he found that not only did muscular irritability last much longer in the paralyzed limb, but rigidity set in later and ended later, and putrefaction began later and was less rapid than on the healthy side. This is a common case of the Method of Difference, requiring no comment. A further and very important corroboration was obtained by the same method. When the animal was killed, not shortly after the section of the nerve, but a month later, the effect was reversed; rigidity set in sooner, and lasted a shorter time, than in the healthy muscles. But after this lapse of time, the paralyzed muscles, having been kept by the paralysis in a state of rest, had lost a great part of their irritability, and instead of more, had become less irritable than those on the healthy side. This gives the A B C, *a b c*, and B C, b c, of the Method of Difference. One antecedent, increased irritability, being changed, and the other circumstances being the same, the consequence did not follow; and, moreover, when a new antecedent, contrary to the first, was supplied, it was followed by a contrary consequent. This instance is attended with the special advantage of proving that the retardation and prolongation of the rigidity do not depend directly on the paralysis, since that was the same in both the instances; but specifically on one effect of the paralysis, namely, the increased irritability; since they ceased when it ceased, and were reversed when it was reversed.

2d. Diminution of the temperature of muscles before death increases their irritability. But diminution of their temperature also retards cadaveric rigidity and putrefaction.

Both these truths were first made known by Dr. Brown-Séquard himself, through experiments which conclude according to the Method of Difference. There is nothing in the nature of
[303] the process requiring specific analysis.

3d. Muscular exercise, prolonged to exhaustion, diminishes the muscular irritability. This is a well-known truth, dependent on the most general laws of muscular action, and proved by experiments under the Method of Difference, constantly repeated. Now, it has been shown by observation that overdriven cattle, if killed before recovery from their fatigue, become rigid and putrefy in a surprisingly short time. A similar fact has been observed in the case of animals hunted to death; cocks killed during or shortly after a fight; and soldiers slain in the field of battle. These various cases agree in no circumstance, directly connected with the muscles, except that these have just been subjected to exhausting exercise. Under the canon, therefore, of the Method of Agreement, it may be inferred that there is a connection between the two facts. The Method of Agreement, indeed, as has been shown, is not competent to prove causation. The present case, however, is already known to be a case of causation, it being certain that the state of the body after death must somehow depend upon its state at the time of death. We are, therefore, warranted in concluding that the single circumstance in which all the instances agree, is the part of the antecedent which is the cause of that particular consequent.

4th. In proportion as the nutrition of muscles is in a good state, their irritability is high. This fact also rests on the general evidence of the laws of physiology, grounded on many familiar applications of the Method of Difference. Now, in the case of those who die from accident or violence, with their muscles in a good state of nutrition, the muscular irritability continues long after death, rigidity sets in late, and persists long without the putrefactive change. On the contrary, in cases of disease in which nutrition has been diminished for a long time before death, all these effects are reversed. These are the conditions of the Joint Method of Agreement and Difference. The cases of retarded and long continued rigidity here in question agree only in being preceded by a high state of nutrition of the muscles; the cases of rapid and brief rigidity agree only in being preceded by a low state of muscular nutrition; a connection is, therefore, inductively proved between the degree of the nutrition, and the slowness and prolongation of the rigidity.

5th. Convulsions, like exhausting exercise, but in a still greater degree, diminish the muscular irritability. Now, when death follows violent and prolonged convulsions, as in tetanus, hydrophobia, some cases of cholera, and certain poisons, rigidity sets in very rapidly, and after a very brief duration, gives place to putrefaction. This is another example of the Method of Agreement, of the same character with No. 3.

6th. The series of instances which we shall take last, is of a more complex character, and requires a more minute analysis.

It has long been observed that in some cases of death by lightning, cadaveric rigidity either does not take place at all, or is of such extremely brief duration as to escape notice, and that in these cases putrefaction is very rapid. In other cases, however, the usual cadaveric rigidity appears. There must be some difference in the cause, to account for this difference in the effect. Now, “death by lightning may be the result of, 1st, a syncope by fright, or in consequence of a direct or reflex influence of lightning on the par vagum; 2d, hemorrhage in or around the brain, or in the lungs, the pericardium, etc.; 3d, concussion, or some other alteration in the brain;” none of which phenomena have any known property capable of accounting for the suppression, or almost suppression, of the cadaveric rigidity.
[304] But the cause of death may also be that the lightning produces “a violent convulsion of every muscle in the body,” of which, if of sufficient intensity, the known effect would be that “muscular irritability ceases almost at once.” If Dr. Brown-Séquard's generalization is a true law, these will be the very cases in which rigidity is so much abridged as to escape notice; and the cases in which, on the contrary, rigidity takes place as usual, will be those in which the stroke of lightning operates in some of the other modes which have been enumerated. How, then, is this brought to the test? By experiments, not on lightning, which can not be commanded at pleasure, but on the same natural agency in a manageable form, that of artificial galvanism. Dr. Brown-Séquard galvanized the entire bodies of animals immediately after death. Galvanism can not operate in any of the modes in which the stroke of lightning may have operated, except the single one of producing muscular convulsions. If, therefore, after the bodies have been galvanized, the duration of rigidity is much shortened and putrefaction much accelerated, it is reasonable to ascribe the same effects when produced by lightning to the property which galvanism shares with lightning, and not to those

which it does not. Now this Dr. Brown-Séquard found to be the fact. The galvanic experiment was tried with charges of very various degrees of strength; and the more powerful the charge, the shorter was found to be the duration of rigidity, and the more speedy and rapid the putrefaction. In the experiment in which the charge was strongest, and the muscular irritability most promptly destroyed, the rigidity only lasted fifteen minutes. On the principle, therefore, of the Method of Concomitant Variations, it may be inferred that the duration of the rigidity depends on the degree of the irritability; and that if the charge had been as much stronger than Dr. Brown-Séquard's strongest, as a stroke of lightning must be stronger than any electric shock which we can produce artificially, the rigidity would have been shortened in a corresponding ratio, and might have disappeared altogether. This conclusion having been arrived at, the case of an electric shock, whether natural or artificial, becomes an instance, in addition to all those already ascertained, of correspondence between the irritability of the muscle and the duration of rigidity.

All these instances are summed up in the following statement: "That when the degree of muscular irritability at the time of death is considerable, either in consequence of a good state of nutrition, as in persons who die in full health from an accidental cause, or in consequence of rest, as in cases of paralysis, or on account of the influence of cold, cadaveric rigidity in all these cases sets in late and lasts long, and putrefaction appears late, and progresses slowly;" but "that when the degree of muscular irritability at the time of death is slight, either in consequence of a bad state of nutrition, or of exhaustion from overexertion, or from convulsions caused by disease or poison, cadaveric rigidity sets in and ceases soon, and putrefaction appears and progresses quickly." These facts present, in all their completeness, the conditions of the Joint Method of Agreement and Difference. Early and brief rigidity takes place in cases which agree only in the circumstance of a low state of muscular irritability. Rigidity begins late and lasts long in cases which agree only in the contrary circumstance, of a muscular irritability high and unusually prolonged. It follows that there is a connection through causation between the degree of muscular irritability after death, and the tardiness and prolongation of the cadaveric rigidity.

This investigation places in a strong light the value and efficacy [305] of the Joint Method. For, as we have already seen, the defect of that Method is, that like the Method of Agreement, of which it is only an improved form, it can not prove causation. But in the present case (as in one of the steps in the argument which led up to it) causation is already proved; since there could never be any doubt that the rigidity altogether, and the putrefaction which follows it, are caused by the fact of death: the observations and experiments on which this rests are too familiar to need analysis, and fall under the Method of Difference. It being, therefore, beyond doubt that the aggregate antecedent, the death, is the actual cause of the whole train of consequents, whatever of the circumstances attending the death can be shown to be followed in all its variations by variations in the effect under investigation, must be the particular feature of the fact of death on which that effect depends. The degree of muscular irritability at the time of death fulfills this condition. The only point that could be brought into question, would be whether the effect depended on the irritability itself, or on something which always accompanied the irritability: and this doubt is set at rest by establishing, as the instances do, that by whatever cause the high or low irritability is produced, the effect equally follows; and can not, therefore, depend upon the causes of irritability, nor upon the other effects of those causes, which are as various as the causes themselves, but upon the irritability, solely.

§ 5. The last two examples will have conveyed to any one by whom they have been duly followed, so clear a conception of the use and practical management of three of the four methods of experimental inquiry, as to supersede the necessity of any further exemplification of them. The remaining method, that of Residues, not having found a place in any of the preceding investigations, I shall quote from Sir John Herschel some examples of that method, with the remarks by which they are introduced.

"It is by this process, in fact, that science, in its present advanced state, is chiefly promoted. Most of the phenomena which Nature presents are very complicated; and when the effects of all known causes are estimated with exactness, and subducted, the residual facts are constantly appearing in the form of phenomena altogether new, and leading to the most important conclusions.

"For example: the return of the comet predicted by Professor Eucke a great many times in succession, and the general good agreement of its calculated with its observed place during any one of its periods of visibility, would lead us to say that its gravitation toward the sun and planets is the sole and sufficient cause of all the phenomena of its orbitual motion; but when the effect of this cause is strictly calculated and subducted from the observed motion, there is found to remain behind a *residual phenomenon*, which would never have been otherwise ascertained to exist, which is a small anticipation of the time of its re-appearance, or a diminution of its periodic time, which can not be accounted for by gravity, and whose cause is therefore to be inquired into. Such an anticipation would be caused by the resistance of a medium disseminated through the celestial regions; and as there are other good reasons for believing this to be a *vera causa*" (an actually existing antecedent), "it has therefore been ascribed to such a resistance.[142]

"M. Arago, having suspended a magnetic needle by a silk thread, and set it in vibration, observed, that it came much [306]

[142] In his subsequent work, *Outlines of Astronomy* (§ 570), Sir John Herschel suggests another possible explanation of the acceleration of the revolution of a comet.

sooner to a state of rest when suspended over a plate of copper, than when no such plate was beneath it. Now, in both cases there were two *veræ causæ*" (antecedents known to exist) "why it *should* come at length to rest, viz., the resistance of the air, which opposes, and at length destroys, all motions performed in it; and the want of perfect mobility in the silk thread. But the effect of these causes being exactly known by the observation made in the absence of the copper, and being thus allowed for and subducted, a residual phenomenon appeared, in the fact that a retarding influence was exerted by the copper itself; and this fact, once ascertained, speedily led to the knowledge of an entirely new and unexpected class of relations." This example belongs, however, not to the Method of Residues but to the Method of Difference, the law being ascertained by a direct comparison of the results of two experiments, which differed in nothing but the presence or absence of the plate of copper. To have made it exemplify the Method of Residues, the effect of the resistance of the air and that of the rigidity of the silk should have been calculated *a priori*, from the laws obtained by separate and foregone experiments.

"Unexpected and peculiarly striking confirmations of inductive laws frequently occur in the form of residual phenomena, in the course of investigations of a widely different nature from those which gave rise to the inductions themselves. A very elegant example may be cited in the unexpected confirmation of the law of the development of heat in elastic fluids by compression, which is afforded by the phenomena of sound. The inquiry into the cause of sound had led to conclusions respecting its mode of propagation, from which its velocity in the air could be precisely calculated. The calculations were performed; but, when compared with fact, though the agreement was quite sufficient to show the general correctness of the cause and mode of propagation assigned, yet the *whole* velocity could not be shown to arise from this theory. There was still a residual velocity to be accounted for, which placed dynamical philosophers for a long time in great dilemma. At length Laplace struck on the happy idea, that this might arise from the *heat* developed in the act of that condensation which necessarily takes place at every vibration by which sound is conveyed. The matter was subjected to exact calculation, and the result was at once the complete explanation of the residual phenomenon, and a striking confirmation of the general law of the development of heat by compression, under circumstances beyond artificial imitation."

"Many of the new elements of chemistry have been detected in the investigation of residual phenomena. Thus Arfwedson discovered lithia by perceiving an excess of weight in the sulphate produced from a small portion of what he considered as magnesia present in a mineral he had analyzed. It is on this principle, too, that the small concentrated residues of great operations in the arts are almost sure to be the lurking-places of new chemical ingredients: witness iodine, brome, selenium, and the new metals accompanying platina in the experiments of Wollaston and Tennant. It was a happy thought of Glauber to examine what every body else threw away."[143]

"Almost all the greatest discoveries in Astronomy," says the same author,[144] "have resulted from the consideration of residual phenomena of a quantitative or numerical kind.... It was thus that the grand discovery of the precession of the equinoxes resulted [307]
as a residual phenomenon, from the imperfect explanation of the return of the seasons by the return of the sun to the same apparent place among the fixed stars. Thus, also, aberration and nutation resulted as residual phenomena from that portion of the changes of the apparent places of the fixed stars which was left unaccounted for by precession. And thus again the apparent proper motions of the stars are the observed residues of their apparent movements outstanding and unaccounted for by strict calculation of the effects of precession, nutation, and

[143] Discourse, pp. 156-8, and 171.
[144] Outlines of Astronomy, § 856.

aberration. The nearest approach which human theories can make to perfection is to diminish this residue, this *caput mortuum* of observation, as it may be considered, as much as practicable, and, if possible, to reduce it to nothing, either by showing that something has been neglected in our estimation of known causes, or by reasoning upon it as a new fact, and on the principle of the inductive philosophy ascending from the effect to its cause or causes."

The disturbing effects mutually produced by the earth and planets upon each other's motions were first brought to light as residual phenomena, by the difference which appeared between the observed places of those bodies, and the places calculated on a consideration solely of their gravitation toward the sun. It was this which determined astronomers to consider the law of gravitation as obtaining between all bodies whatever, and therefore between all particles of matter; their first tendency having been to regard it as a force acting only between each planet or satellite and the central body to whose system it belonged. Again, the catastrophists, in geology, be their opinion right or wrong, support it on the plea, that after the effect of all causes now in operation has been allowed for, there remains in the existing constitution of the earth a large residue of facts, proving the existence at former periods either of other forces, or of the same forces in a much greater degree of intensity. To add one more example: those who assert, what no one has shown any real ground for believing, that there is in one human individual, one sex, or one race of mankind over another, an inherent and inexplicable superiority in mental faculties, could only substantiate their proposition by subtracting from the differences of intellect which we in fact see, all that can be traced by known laws either to the ascertained differences of physical organization, or to the differences which have existed in the outward circumstances in which the subjects of the comparison have hitherto been placed. What these causes might fail to account for would constitute a residual phenomenon, which and which alone would be evidence of an ulterior original distinction, and the measure of its amount. But the asserters of such supposed differences have not provided themselves with these necessary logical conditions of the establishment of their doctrine.

The spirit of the Method of Residues being, it is hoped, sufficiently intelligible from these examples, and the other three methods having already been so fully exemplified, we may here close our exposition of the four methods, considered as employed in the investigation of the simpler and more elementary order of the combinations of phenomena.

§ 6. Dr. Whewell has expressed a very unfavorable opinion of the utility of the Four Methods, as well as of the aptness of the examples by which I have attempted to illustrate them. His words are these:[145] [308]

"Upon these methods, the obvious thing to remark is, that they take for granted the very thing which is most difficult to discover, the reduction of the phenomena to formulæ such as are here presented to us. When we have any set of complex facts offered to us; for instance, those which were offered in the cases of discovery which I have mentioned—the facts of the planetary paths, of falling bodies, of refracted rays, of cosmical motions, of chemical analysis; and when, in any of these cases, we would discover the law of nature which governs them, or, if any one chooses so to term it, the feature in which all the cases agree, where are we to look for our A, B, C, and *a*, *b*, *c*? Nature does not present to us the cases in this form; and how are we to reduce them to this form? You say *when* we find the combination of A B C with *a b c* and A B D with *a b d*, then we may draw our inference. Granted; but when and where are we to find such combinations? Even now that the discoveries are made, who will point out to us what are the A, B, C, and *a*, *b*, *c*, elements of the

[145] *Philosophy of Discovery*, pp. 263, 264.

cases which have just been enumerated? Who will tell us which of the methods of inquiry those historically real and successful inquiries exemplify? Who will carry these formulæ through the history of the sciences, as they have really grown up, and show us that these four methods have been operative in their formation; or that any light is thrown upon the steps of their progress by reference to these formulæ?"

He adds that, in this work, the methods have not been applied "to a large body of conspicuous and undoubted examples of discovery, extending along the whole history of science;" which ought to have been done in order that the methods might be shown to possess the "advantage" (which he claims as belonging to his own) of being those "by which all great discoveries in science have really been made."—(P. 277.)

There is a striking similarity between the objections here made against Canons of Induction, and what was alleged, in the last century, by as able men as Dr. Whewell, against the acknowledged Canon of Ratiocination. Those who protested against the Aristotelian Logic said of the Syllogism, what Dr. Whewell says of the Inductive Methods, that it "takes for granted the very thing which is most difficult to discover, the reduction of the argument to formulæ such as are here presented to us." The grand difficulty, they said, is to obtain your syllogism, not to judge of its correctness when obtained. On the matter of fact, both they and Dr. Whewell are right. The greatest difficulty in both cases is, first, that of obtaining the evidence, and next, of reducing it to the form which tests its conclusiveness. But if we try to reduce it without knowing what it is to be reduced to, we are not likely to make much progress. It is a more difficult thing to solve a geometrical problem, than to judge whether a proposed solution is correct: but if people were not able to judge of the solution when found, they would have little chance of finding it. And it can not be pretended that to judge of an induction when found is perfectly easy, is a thing for which aids and instruments are superfluous; for erroneous inductions, false inferences from experience, are quite as common, on some subjects much commoner than true ones. The business of Inductive Logic is to provide rules and models (such as the Syllogism and its rules are for ratiocination) to which if inductive arguments conform, those arguments are conclusive, and not otherwise. This is what the Four Methods profess to be, and what I believe they are universally considered to be by experimental philosophers, who had practiced all of them long before any one sought to reduce the practice to theory. [309]

The assailants of the Syllogism had also anticipated Dr. Whewell in the other branch of his argument. They said that no discoveries were ever made by syllogism; and Dr. Whewell says, or seems to say, that none were ever made by the Four Methods of Induction. To the former objectors, Archbishop Whately very pertinently answered, that their argument, if good at all, was good against the reasoning process altogether; for whatever can not be reduced to syllogism, is not reasoning. And Dr. Whewell's argument, if good at all, is good against all inferences from experience. In saying that no discoveries were ever made by the Four Methods, he affirms that none were ever made by observation and experiment; for assuredly if any were, it was by processes reducible to one or other of those methods.

This difference between us accounts for the dissatisfaction which my examples give him; for I did not select them with a view to satisfy any one who required to be convinced that observation and experiment are modes of acquiring knowledge: I confess that in the choice of them I thought only of illustration, and of facilitating the *conception* of the Methods by concrete instances. If it had been my object to justify the processes themselves as means of investigation, there would have been no need to look far off, or make use of recondite or complicated instances. As a specimen of a truth ascertained by the Method of Agreement, I might have chosen the proposition, “Dogs bark.”

This dog, and that dog, and the other dog, answer to A B C, A D E, A F G. The circumstance of being a dog answers to A. Barking answers to *a*. As a truth made known by the Method of Difference, "Fire burns" might have sufficed. Before I touch the fire I am not burned; this is B C: I touch it, and am burned; this is A B C, *a* B C.

Such familiar experimental processes are not regarded as inductions by Dr. Whewell; but they are perfectly homogeneous with those by which, even on his own showing, the pyramid of science is supplied with its base. In vain he attempts to escape from this conclusion by laying the most arbitrary restrictions on the choice of examples admissible as instances of Induction: they must neither be such as are still matter of discussion (p. 265), nor must any of them be drawn from mental and social subjects (p. 269), nor from ordinary observation and practical life (pp. 241-247). They must be taken exclusively from the generalizations by which scientific thinkers have ascended to great and comprehensive laws of natural phenomena. Now it is seldom possible, in these complicated inquiries, to go much beyond the initial steps, without calling in the instrument of Deduction, and the temporary aid of hypothesis; as I myself, in common with Dr. Whewell, have maintained against the purely empirical school. Since, therefore, such cases could not conveniently be selected to illustrate the principles of mere observation and experiment, Dr. Whewell is misled by their absence into representing the Experimental Methods as serving no purpose in scientific investigation; forgetting that if those methods had not supplied the first generalizations, there would have been no materials for his own conception of Induction to work upon.

His challenge, however, to point out which of the four methods are exemplified in certain important cases of scientific inquiry, is easily answered. "The planetary paths," as far as they are a case of induction at all,[146] fall under the Method of Agreement. The law of "falling bodies," namely, that they describe spaces proportional to the squares of the times, was historically a [310] deduction from the first law of motion; but the experiments by which it was verified, and by which it might have been discovered, were examples of the Method of Agreement; and the apparent variation from the true law, caused by the resistance of the air, was cleared up by experiments *in vacuo*, constituting an application of the Method of Difference. The law of "refracted rays" (the constancy of the ratio between the sines of incidence and of refraction for each refracting substance) was ascertained by direct measurement, and therefore by the Method of Agreement. The "cosmical motions" were determined by highly complex processes of thought, in which Deduction was predominant, but the Methods of Agreement and of Concomitant Variations had a large part in establishing the empirical laws. Every case without exception of "chemical analysis" constitutes a well-marked example of the Method of Difference. To any one acquainted with the subjects—to Dr. Whewell himself, there would not be the smallest difficulty in setting out "the A B C and *a b c* elements" of these cases.

If discoveries are ever made by observation and experiment without Deduction, the four methods are methods of discovery: but even if they were not methods of discovery, it would not be the less true that they are the sole methods of Proof; and in that character, even the results of deduction are amenable to them. The great generalizations which begin as Hypotheses, must end by being proved, and are in reality (as will be shown hereafter) proved, by the Four Methods. Now it is with Proof, as such, that Logic is principally concerned. This distinction has indeed no chance of finding favor with Dr. Whewell; for it is the peculiarity of his system, not to recognize, in cases of Induction,

[146] See, on this point, the second chapter of the present book.

any necessity for proof. If, after assuming an hypothesis and carefully collating it with facts, nothing is brought to light inconsistent with it, that is, if experience does not *dis*prove it, he is content: at least until a simpler hypothesis, equally consistent with experience, presents itself. If this be Induction, doubtless there is no necessity for the four methods. But to suppose that it is so, appears to me a radical misconception of the nature of the evidence of physical truths.

So real and practical is the need of a test for induction, similar to the syllogistic test of ratiocination, that inferences which bid defiance to the most elementary notions of inductive logic are put forth without misgiving by persons eminent in physical science, as soon as they are off the ground on which they are conversant with the facts, and not reduced to judge only by the arguments; and as for educated persons in general, it may be doubted if they are better judges of a good or a bad induction than they were before Bacon wrote. The improvement in the results of thinking has seldom extended to the processes; or has reached, if any process, that of investigation only, not that of proof. A knowledge of many laws of nature has doubtless been arrived at, by framing hypotheses and finding that the facts corresponded to them; and many errors have been got rid of by coming to a knowledge of facts which were inconsistent with them, but not by discovering that the mode of thought which led to the errors was itself faulty, and might have been known to be such independently of the facts which disproved the specific conclusion. Hence it is, that while the thoughts of mankind have on many subjects worked themselves practically right, the thinking power remains as weak as ever: and on all subjects on which the facts which would check the result are not accessible, as in what relates to the invisible world, and even, as has been seen lately, to the visible world of the planetary regions, men
[311] of the greatest scientific acquirements argue as pitiably as the merest ignoramus. For though they have made many sound

inductions, they have not learned from them (and Dr. Whewell thinks there is no necessity that they should learn) the principles of inductive *evidence*.

Chapter X.

Of Plurality Of Causes, And Of The Intermixture Of Effects.

§ 1. In the preceding exposition of the four methods of observation and experiment, by which we contrive to distinguish among a mass of co-existent 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 co-existent 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 mechanical 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.

§ 2. One of the principal consequences of this fact of Plurality of Causes is, to render the first of the inductive methods, that of
[312] 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 very numerous; 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 favor 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 toward a solution of this doubt. For if we try A F G, A H K, etc., 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 favor of A. The inquirer, of course, will not
[313] 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 can not 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 analyzing 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 recognized 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 can not 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
[314] *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.

§ 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
[315] 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, etc., the ultimate source is one and the same. But if (as continually happens) we can not 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.

§ 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 analyzed, can not 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 [316]
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 toward 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 re-appear 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 connection 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 [317]
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.

In these cases, where the heteropathic effect (as we called it in a former chapter)[147] 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,

[147] Ante, chap. vii., § 1.

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 can not be reproduced from the product; just as a youth can grow into an old man, but an old man can not grow into a youth. We can not 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.

§ 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 recognizing 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*, ½*b*, -*b*, 2*b*, etc.; some of which cancel one another, [318]
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 toward 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 toward 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 co-existed 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 [319] 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 downward 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[148] 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
[320] falling, constitutes the balloon an exception to that pretended law of nature. But the real law is, that all heavy bodies *tend* to

[148] It seems hardly necessary to say that the word *impinge*, as a general term to express collision of forces, is here used by a figure of speech, and not as expressive of any theory respecting the nature of force.

fall; and to this there is no exception, not even the sun and moon; for even they, as every astronomer knows, tend toward the earth, with a force exactly equal to that with which the earth tends toward 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."[149]

§ 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

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[149] *Essays on some Unsettled Questions of Political Economy*, Essay V.

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 favorite 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
[321] 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 would tend 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, etc., remarking in which of these cases it was attended with 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.

§ 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 can not 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 favorable 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 [322] the subject.[150]

§ 8. The inapplicability of the method of simple observation to

[150] It is justly remarked by Professor Bain, that though the Methods of Agreement and Difference are not applicable to these cases, they are not wholly inaccessible to the Method of Concomitant Variations. "If a cause happens to vary alone, the effect will also vary alone: a cause and effect may be thus singled out under the greatest complications. Thus, when the appetite for food increases with the cold, we have a strong evidence of connection between these two facts, although other circumstances may operate in the same direction. The assigning of the respective parts of the sun and moon in the action of the tides may be effected, to a certain degree of exactness, by the variations of the amount according to the positions of the two attractive bodies. By a series of experiments of Concomitant Variations, directed to ascertain the elimination of nitrogen from the human body under varieties of muscular exercise, Dr. Parkes obtained the remarkable conclusion, that a muscle grows during exercise, and loses bulk during the subsequent rest." (*Logic*, ii., 83.)

It is, no doubt, often possible to single out the influencing causes from among a great number of mere concomitants, by noting what are the antecedents, a variation in which is followed by a variation in the effect. But when there are many influencing causes, no one of them greatly predominating over the rest, and especially when some of these are continually changing, it is scarcely ever possible to trace such a relation between the variations of the effect and those of any one cause as would enable us to assign to that cause its real share in the ascertain the conditions of effects dependent on many concurring causes, being thus recognized, 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 every thing 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

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production of the effect.

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,
[323] 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 agents 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 can not 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.

Any thing like a scientific use of the method of experiment, in these complicated cases, is therefore out of the question. We can generally, even in the most favorable 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 usually, 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. Some causes indeed there are which are more potent than any counteracting causes to which they are commonly exposed; and accordingly there are some truths in medicine which are sufficiently proved by direct experiment. Of these the most familiar are those that relate to the efficacy of the substances known as Specifics for particular diseases, “quinine,

colchicum, lime-juice, cod-liver oil,”[151] and a few others. Even
[324] these are not invariably followed by success; but they succeed in so large a proportion of cases, and against such powerful obstacles, that their *tendency* to restore health in the disorders for which they are prescribed may be regarded as an experimental truth.[152]

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

[151] Bain's *Logic*, ii., 360.

[152] What is said in the text on the applicability of the experimental methods to resolve particular questions of medical treatment, does not detract from their efficacy in ascertaining the general laws of the animal or human system. The functions, for example, of the different classes of nerves have been discovered, and probably could only have been discovered, by experiments on living animals. Observation and experiment are the ultimate basis of all knowledge: from them we obtain the elementary laws of life, as we obtain all other elementary truths. It is in dealing with the complex combinations that the experimental methods are for the most part illusory, and the deductive mode of investigation must be invoked to disentangle the complexity.

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 [325] induction, so many of the other influencing circumstances will have changed as to vitiate the experiment.[153]

[153] Professor Bain, though concurring generally in the views expressed in this chapter, seems to estimate more highly than I do the scope for specific

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.

Chapter XI.

Of The Deductive Method.

§ 1. The mode of investigation which, from the proved inapplicability of direct methods of observation and experiment, remains to us as the main source of the knowledge we

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experimental evidence in politics. (*Logic*, ii., 333-337.) There are, it is true, as he remarks (p. 336), some cases "when an agent suddenly introduced is almost instantaneously followed by some other changes, as when the announcement of a diplomatic rupture between two nations is followed the same day by a derangement of the money-market." But this experiment would be quite inconclusive merely as an experiment. It can only serve, as any experiment may, to verify the conclusion of a deduction. Unless we already knew by our knowledge of the motives which act on business men, that the prospect of war *tends* to derange the money-market, we should never have been able to prove a connection between the two facts, unless after having ascertained historically that the one followed the other in too great a number of instances to be consistent with their having been recorded with due precautions. Whoever has carefully examined any of the attempts continually made to prove economic doctrines by such a recital of instances, knows well how futile they are. It always turns out that the circumstances of scarcely any of the cases have been fully stated; and that cases, in equal or greater numbers, have been omitted which would have tended to an opposite conclusion.

possess or can acquire respecting the conditions and laws of recurrence, of the more complex phenomena, is called, in its most general expression, the Deductive Method; and consists of three operations: the first, one of direct induction; the second, of ratiocination; the third, of verification.

I call the first step in the process an inductive operation, because there must be a direct induction as the basis of the whole; though in many particular investigations the place of the induction may be supplied by a prior deduction; but the premises of this prior deduction must have been derived from induction.

The problem of the Deductive Method is, to find the law of an effect, from the laws of the different tendencies of which it is the joint result. The first requisite, therefore, is to know the laws of those tendencies; the law of each of the concurrent causes: and this supposes a previous process of observation or experiment upon each cause separately; or else a previous deduction, which also must depend for its ultimate premises on observation or experiment. Thus, if the subject be social or historical phenomena, the premises of the Deductive Method must be the laws of the causes which determine that class of phenomena; and those causes are human actions, together with the general outward circumstances under the influence of which [326]
mankind are placed, and which constitute man's position on the earth. The Deductive Method, applied to social phenomena, must begin, therefore, by investigating, or must suppose to have been already investigated, the laws of human action, and those properties of outward things by which the actions of human beings in society are determined. Some of these general truths will naturally be obtained by observation and experiment, others by deduction: the more complex laws of human action, for example, may be deduced from the simpler ones; but the simple or elementary laws will always, and necessarily, have been obtained by a directly inductive process.

To ascertain, then, the laws of each separate cause which takes a share in producing the effect, is the first desideratum of the Deductive Method. To know what the causes are which must be subjected to this process of study, may or may not be difficult. In the case last mentioned, this first condition is of easy fulfillment. That social phenomena depend on the acts and mental impressions of human beings, never could have been a matter of any doubt, however imperfectly it may have been known either by what laws those impressions and actions are governed, or to what social consequences their laws naturally lead. Neither, again, after physical science had attained a certain development, could there be any real doubt where to look for the laws on which the phenomena of life depend, since they must be the mechanical and chemical laws of the solid and fluid substances composing the organized body and the medium in which it subsists, together with the peculiar vital laws of the different tissues constituting the organic structure. In other cases, really far more simple than these, it was much less obvious in what quarter the causes were to be looked for: as in the case of the celestial phenomena. Until, by combining the laws of certain causes, it was found that those laws explained all the facts which experience had proved concerning the heavenly motions, and led to predictions which it always verified, mankind never knew that those *were* the causes. But whether we are able to put the question before, or not until after, we have become capable of answering it, in either case it must be answered; the laws of the different causes must be ascertained, before we can proceed to deduce from them the conditions of the effect.

The mode of ascertaining those laws neither is, nor can be any other than the fourfold method of experimental inquiry, already discussed. A few remarks on the application of that method to cases of the Composition of Causes are all that is requisite.

It is obvious that we can not expect to find the law of a tendency by an induction from cases in which the tendency is counteracted. The laws of motion could never have been brought to light from the observation of bodies kept at rest by the equilibrium of opposing forces. Even where the tendency is not, in the ordinary sense of the word, counteracted, but only modified, by having its effects compounded with the effects arising from some other tendency or tendencies, we are still in an unfavorable position for tracing, by means of such cases, the law of the tendency itself. It would have been scarcely possible to discover the law that every body in motion tends to continue moving in a straight line, by an induction from instances in which the motion is deflected into a curve, by being compounded with the effect of an accelerating force. Notwithstanding the resources afforded in this description of cases by the Method of Concomitant Variations, the principles of a judicious experimentation prescribe that the law of each of the tendencies should be studied, if possible, in cases in which [327] that tendency operates alone, or in combination with no agencies but those of which the effect can, from previous knowledge, be calculated and allowed for.

Accordingly, in the cases, unfortunately very numerous and important, in which the causes do not suffer themselves to be separated and observed apart, there is much difficulty in laying down with due certainty the inductive foundation necessary to support the deductive method. This difficulty is most of all conspicuous in the case of physiological phenomena; it being seldom possible to separate the different agencies which collectively compose an organized body, without destroying the very phenomena which it is our object to investigate:

> ——following life, in creatures we dissect,
> We lose it, in the moment we detect.

And for this reason I am inclined to the opinion that physiology (greatly and rapidly progressive as it now is) is embarrassed by greater natural difficulties, and is probably susceptible of a less degree of ultimate perfection, than even the social science;

inasmuch as it is possible to study the laws and operations of one human mind apart from other minds, much less imperfectly than we can study the laws of one organ or tissue of the human body apart from the other organs or tissues.

It has been judiciously remarked that pathological facts, or, to speak in common language, diseases in their different forms and degrees afford in the case of physiological investigation the most valuable equivalent to experimentation properly so called; inasmuch as they often exhibit to us a definite disturbance in some one organ or organic function, the remaining organs and functions being, in the first instance at least, unaffected. It is true that from the perpetual actions and reactions which are going on among all parts of the organic economy, there can be no prolonged disturbance in any one function without ultimately involving many of the others; and when once it has done so, the experiment for the most part loses its scientific value. All depends on observing the early stages of the derangement; which, unfortunately, are of necessity the least marked. If, however, the organs and functions not disturbed in the first instance become affected in a fixed order of succession, some light is thereby thrown upon the action which one organ exercises over another: and we occasionally obtain a series of effects which we can refer with some confidence to the original local derangement; but for this it is necessary that we should know that the original derangement *was* local. If it was what is termed constitutional; that is, if we do not know in what part of the animal economy it took its rise, or the precise nature of the disturbance which took place in that part, we are unable to determine which of the various derangements was cause and which effect; which of them were produced by one another, and which by the direct, though perhaps tardy, action of the original cause.

Besides natural pathological facts, we can produce pathological facts artificially: we can try experiments, even in the popular sense of the term, by subjecting the living being to some external agent, such as the mercury of our former example, or the section of a nerve to ascertain the functions of different parts of the nervous system. As this experimentation is not intended to obtain a direct solution of any practical question, but to discover general laws, from which afterward the conditions of any particular effect may be obtained by deduction, the best cases to select are those of which the circumstances can be best ascertained: and such are generally not those in which there is [328] any practical object in view. The experiments are best tried, not in a state of disease, which is essentially a changeable state, but in the condition of health, comparatively a fixed state. In the one, unusual agencies are at work, the results of which we have no means of predicting: in the other, the course of the accustomed physiological phenomena would, it may generally be presumed, remain undisturbed, were it not for the disturbing cause which we introduce.

Such, with the occasional aid of the Method of Concomitant Variations (the latter not less encumbered than the more elementary methods by the peculiar difficulties of the subject), are our inductive resources for ascertaining the laws of the causes considered separately, when we have it not in our power to make trial of them in a state of actual separation. The insufficiency of these resources is so glaring, that no one can be surprised at the backward state of the science of physiology; in which indeed our knowledge of causes is so imperfect, that we can neither explain, nor could without specific experience have predicted, many of the facts which are certified to us by the most ordinary observation. Fortunately, we are much better informed as to the empirical laws of the phenomena, that is, the uniformities respecting which we can not yet decide whether they are cases of causation, or mere results of it. Not only has the order in which the facts of organization and life successively manifest themselves, from the first germ of existence to death, been found to be uniform, and very accurately ascertainable; but, by a great application of the Method of Concomitant Variations to the entire facts of comparative anatomy and physiology, the characteristic organic structure corresponding to each class of functions has been determined with considerable precision. Whether these organic conditions are the whole of the conditions, and in many cases whether they are conditions at all, or mere collateral effects of some common cause, we are quite ignorant; nor are we ever likely to know, unless we could construct an organized body and try whether it would live.

Under such disadvantages do we, in cases of this description, attempt the initial, or inductive step, in the application of the Deductive Method to complex phenomena. But such, fortunately, is not the common case. In general, the laws of the causes on which the effect depends may be obtained by an induction from comparatively simple instances, or, at the worst, by deduction from the laws of simpler causes, so obtained. By simple instances are meant, of course, those in which the action of each cause was not intermixed or interfered with, or not to any great extent, by other causes whose laws were unknown. And only when the induction which furnished the premises to the Deductive method rested on such instances has the application of such a method to the ascertainment of the laws of a complex effect, been attended with brilliant results.

§ 2. When the laws of the causes have been ascertained, and the first stage of the great logical operation now under discussion satisfactorily accomplished, the second part follows; that of determining from the laws of the causes what effect any given combination of those causes will produce. This is a process of calculation, in the wider sense of the term; and very often involves processes of calculation in the narrowest sense. It is a ratiocination; and when our knowledge of the causes is so perfect as to extend to the exact numerical laws which they observe in producing their effects, the ratiocination may reckon among its premises the theorems of the science of number, in the whole

immense extent of that science. Not only are the most advanced [329]
truths of mathematics often required to enable us to compute an effect, the numerical law of which we already know; but, even by the aid of those most advanced truths, we can go but a little way. In so simple a case as the common problem of three bodies gravitating toward one another, with a force directly as their mass and inversely as the square of the distance, all the resources of the calculus have not hitherto sufficed to obtain any general solution, but an approximate one. In a case a little more complex, but still one of the simplest which arise in practice, that of the motion of a projectile, the causes which affect the velocity and range (for example) of a cannon-ball may be all known and estimated: the force of the gunpowder, the angle of elevation, the density of the air, the strength and direction of the wind; but it is one of the most difficult of mathematical problems to combine all these, so as to determine the effect resulting from their collective action.

Besides the theorems of number, those of geometry also come in as premises, where the effects take place in space, and involve motion and extension, as in mechanics, optics, acoustics, astronomy. But when the complication increases, and the effects are under the influence of so many and such shifting causes as to give no room either for fixed numbers, or for straight lines and regular curves (as in the case of physiological, to say nothing of mental and social phenomena), the laws of number and extension are applicable, if at all, only on that large scale on which precision of details becomes unimportant. Although these laws play a conspicuous part in the most striking examples of the investigation of nature by the Deductive Method, as for example in the Newtonian theory of the celestial motions, they are by no means an indispensable part of every such process. All that is essential in it is reasoning from a general law to a particular case, that is, determining by means of the particular circumstances of that case, what result is required in that instance to fulfill the law. Thus in the Torricellian experiment, if the fact that air has weight had been previously known, it would have been easy, without any numerical data, to deduce from the general law of equilibrium, that the mercury would stand in the tube at such a height that the column of mercury would exactly balance a column of the atmosphere of equal diameter; because, otherwise, equilibrium would not exist.

By such ratiocinations from the separate laws of the causes, we may, to a certain extent, succeed in answering either of the following questions: Given a certain combination of causes, what effect will follow? and, What combination of causes, if it existed, would produce a given effect? In the one case, we determine the effect to be expected in any complex circumstances of which the different elements are known: in the other case we learn, according to what law—under what antecedent conditions—a given complex effect will occur.

§ 3. But (it may here be asked) are not the same arguments by which the methods of direct observation and experiment were set aside as illusory when applied to the laws of complex phenomena, applicable with equal force against the Method of Deduction? When in every single instance a multitude, often an unknown multitude, of agencies, are clashing and combining, what security have we that in our computation *a priori* we have taken all these into our reckoning? How many must we not generally be ignorant of? Among those which we know, how probable that some have been overlooked; and, even were all
[330] included, how vain the pretense of summing up the effects of many causes, unless we know accurately the numerical law of each—a condition in most cases not to be fulfilled; and even when it is fulfilled, to make the calculation transcends, in any but very simple cases, the utmost power of mathematical science with all its most modern improvements.

These objections have real weight, and would be altogether unanswerable, if there were no test by which, when we employ the Deductive Method, we might judge whether an error of any of the above descriptions had been committed or not. Such a test, however, there is: and its application forms, under the name of Verification, the third essential component part of the Deductive Method; without which all the results it can give have little other value than that of conjecture. To warrant reliance on the general conclusions arrived at by deduction, these conclusions must be found, on careful comparison, to accord with the results of direct observation wherever it can be had. If, when we have experience to compare with them, this experience confirms them, we may safely trust to them in other cases of which our specific experience is yet to come. But if our deductions have led to the conclusion that from a particular combination of causes a given effect would result, then in all known cases where that combination can be shown to have existed, and where the effect has not followed, we must be able to show (or at least to make a probable surmise) what frustrated it: if we can not, the theory is imperfect, and not yet to be relied upon. Nor is the verification complete, unless some of the cases in which the theory is borne out by the observed result are of at least equal complexity with any other cases in which its application could be called for.

If direct observation and collation of instances have furnished us with any empirical laws of the effect (whether true in all observed cases, or only true for the most part), the most effectual verification of which the theory could be susceptible, would be, that it led deductively to those empirical laws; that the uniformities, whether complete or incomplete, which were observed to exist among the phenomena, were accounted for by the laws of the causes—were such as could not but exist if those be really the causes by which the phenomena are produced. Thus it was very reasonably deemed an essential requisite of any true theory of the causes of the celestial motions, that it should lead by deduction to Kepler's laws; which, accordingly, the Newtonian theory did.

In order, therefore, to facilitate the verification of theories obtained by deduction, it is important that as many as possible of the empirical laws of the phenomena should be ascertained, by a comparison of instances, conformably to the Method of Agreement: as well as (it must be added) that the phenomena themselves should be described, in the most comprehensive as well as accurate manner possible; by collecting from the observation of parts, the simplest possible correct expressions for the corresponding wholes: as when the series of the observed places of a planet was first expressed by a circle, then by a system of epicycles, and subsequently by an ellipse.

It is worth remarking, that complex instances which would have been of no use for the discovery of the simple laws into which we ultimately analyze their phenomena, nevertheless, when they have served to verify the analysis, become additional evidence of the laws themselves. Although we could not have got at the law from complex cases, still when the law, got at otherwise, is
[331] found to be in accordance with the result of a complex case, that case becomes a new experiment on the law, and helps to confirm what it did not assist to discover. It is a new trial of the principle in a different set of circumstances; and occasionally serves to eliminate some circumstance not previously excluded, and the exclusion of which might require an experiment impossible to be executed. This was strikingly conspicuous in the example formerly quoted, in which the difference between the observed and the calculated velocity of sound was ascertained to result from the heat extricated by the condensation which takes place in each sonorous vibration. This was a trial, in new circumstances, of the law of the development of heat by compression; and it added materially to the proof of the universality of that law. Accordingly, any law of nature is deemed to have gained in point of certainty, by being found to explain some complex case which had not previously been thought of in connection with it; and this indeed is a consideration to which it is the habit of scientific inquirers to attach rather too much value than too little.

To the Deductive Method, thus characterized in its three constituent parts, Induction, Ratiocination, and Verification, the human mind is indebted for its most conspicuous triumphs in the investigation of nature. To it we owe all the theories by which vast and complicated phenomena are embraced under a few simple laws, which, considered as the laws of those great phenomena, could never have been detected by their direct study. We may form some conception of what the method has done for us from the case of the celestial motions: one of the simplest among the greater instances of the Composition of Causes, since (except in a few cases not of primary importance) each of the heavenly bodies may be considered, without material inaccuracy, to be never at one time influenced by the attraction of more than two bodies, the sun and one other planet or satellite; making, with the reaction of the body itself, and the force generated by the body's own motion and acting in the direction of the tangent, only four different agents on the concurrence of which the motions of that body depend; a much smaller number, no doubt, than that by which any other of the great phenomena of nature is determined or modified. Yet how could we ever have ascertained the combination of forces on which the motions of the earth and planets are dependent, by merely comparing the orbits or velocities of different planets, or the different velocities or positions of the same planet? Notwithstanding the regularity which manifests itself in those motions, in a degree so rare among the effects of concurrence of causes; and although the periodical recurrence of exactly the same effect, affords positive proof that all the combinations of causes which occur at all, recur periodically; we should not have known what the causes were, if the existence of agencies precisely similar on our own earth had not, fortunately, brought the causes themselves within the reach of experimentation under simple circumstances. As we shall have occasion to analyze, further on, this great example of the Method of Deduction, we shall not occupy any time with it here, but shall proceed to that secondary application of the Deductive Method, the result of which is not to prove laws of phenomena, but to explain them.

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Chapter XII.

Of The Explanation Of Laws Of Nature.

§ 1. The deductive operation by which we derive the law of an effect from the laws of the causes, the concurrence of which gives rise to it, may be undertaken either for the purpose of discovering the law, or of explaining a law already discovered. The word *explanation* occurs so continually, and holds so important a place in philosophy, that a little time spent in fixing the meaning of it will be profitably employed.

An individual fact is said to be explained, by pointing out its cause, that is, by stating the law or laws of causation, of which its production is an instance. Thus, a conflagration is explained, when it is proved to have arisen from a spark falling into the midst of a heap of combustibles. And in a similar manner, a law or uniformity in nature is said to be explained, when another law or laws are pointed out, of which that law itself is but a case, and from which it could be deduced.

§ 2. There are three distinguishable sets of circumstances in which a law of causation may be explained from, or, as it also is often expressed, resolved into, other laws.

The first is the case already so fully considered; an intermixture of laws, producing a joint effect equal to the sum of the effects of the causes taken separately. The law of the complex effect is