A SYSTEM OF LOGIC,

RATIOCINATIVE AND INDUCTIVE:

BEING A

CONNECTED VIEW OF THE PRINCIPLES OF EVIDENCE

AND THE

METHODS OF SCIENTIFIC INVESTIGATION.

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OF INDUCTION.

"According to the doctrine now stated, the highest, or rather the only proper object of physics, is to ascertain those established conjunctions of successive events, which constitute the order of the universe; to record the phenomena which it exhibits to our observations, or which it discloses to our experiments; and to refer these phenomena to their general laws."—D. Stewart, *Elements of the Philosophy of the Human Mind*, vol. ii., chap. iv., sect. 1.

"In such cases the inductive and deductive methods of inquiry may be said to go hand in hand, the one verifying the conclusions deduced by the other; and the combination of experiment and theory, which may thus be brought to bear in such cases, forms an engine of discovery infinitely more powerful than either taken separately. This state of any department of science is perhaps of all others the most interesting, and that which promises the most to research."—Sir J. Herschel, *Discourse on the Study of Natural Philosophy*.

CHAPTER I.

PRELIMINARY OBSERVATIONS ON INDUCTION IN GENERAL.

§ 1. The portion of the present inquiry upon which we are now about to enter, may be considered as the principal, both from its surpassing in intricacy all the other branches, and because it relates to a process which has been shown in the preceding Book to be that in which the investigation of nature essentially consists. We have found that all Inference, consequently all Proof, and all discovery of truths not self-evident, consists of inductions, and the interpretation of inductions: that all our knowledge, not intuitive, comes to us exclusively from that source. What Induction is, therefore, and what conditions render it legitimate, can not but be deemed the main question of the science of logic—the question which includes all others. It is, however, one which professed writers on logic have almost entirely passed over. The generalities of the subject have not been altogether neglected by metaphysicians; but, for want of sufficient acquaintance with the processes by which science has actually succeeded in establishing general truths, their analysis of the inductive operation, even when unexceptionable as to correctness, has not been specific enough to be made the foundation of practical rules, which might be for induction itself what the rules of the syllogism are for the interpretation of induction: while those by whom physical science has been carried to its present state of improvement—and who, to arrive at a complete theory of the process, needed only to generalize, and adapt to all varieties of problems, the methods which they themselves employed in their habitual pursuits—never until very lately made any serious attempt to philosophize on the subject, nor regarded the mode in which they arrived at their conclusions as deserving of study, independently of the conclusions themselves.

§ 2. For the purposes of the present inquiry, Induction may be defined, the operation of discovering and proving general propositions. It is true that (as already shown) the process of indirectly ascertaining individual facts, is as truly inductive as that by which we establish general truths. But it is not a different kind of induction; it is a form of the very same process: since, on the one hand, generals are but collections of particulars, definite in kind but indefinite in number; and on the other hand, whenever the evidence which we derive from observation of known cases justifies us in drawing an inference respecting even one unknown case, we should on the same evidence be justified in drawing a similar inference with respect to a whole class of cases. The inference either does not hold at all, or it holds in all cases of a certain description; in all cases which, in certain definable respects, resemble those we have observed.

If these remarks are just; if the principles and rules of inference are the same whether we infer general propositions or individual facts; it follows that a complete logic of the sciences would be also a complete logic of practical business and common life. Since there is no case of legitimate inference from experience, in which the conclusion may not legitimately be a general proposition; an analysis of the process by which general truths are arrived at, is virtually an analysis of all induction whatever. Whether we are inquiring into a scientific principle or into an individual fact, and whether we proceed by experiment or by ratiocination, every step in the train of inferences is essentially inductive, and the legitimacy of the induction depends in both cases on the same conditions.

True it is that in the case of the practical inquirer, who is endeavoring to ascertain facts not for the purposes of science but for those of business, such, for instance, as the advocate or the judge, the chief difficulty is one in which the principles of induction will afford him no assistance. It lies not in making his inductions, but in the selection of them; in choosing from among all general propositions ascertained to be true, those which furnish marks by which he may trace whether the given subject possesses or not the predicate in question. In arguing a doubtful question of fact before a jury, the general propositions or principles to which the advocate appeals are mostly, in themselves, sufficiently trite, and assented to as soon as stated: his skill lies in bringing his case under those propositions or principles; in calling to mind such of the known or received maxims of probability as admit of application to the case in hand, and selecting from among them those best adapted to his object. Success is here dependent on natural or acquired sagacity, aided by knowledge of the particular subject, and of subjects allied with it. Invention, though it can be cultivated, can not be reduced to rule; there is no science which will enable a man to bethink himself of that which will suit his purpose.

But when he *has* thought of something, science can tell him whether that which he has thought of will suit his purpose or not. The inquirer or arguer must be guided by his own knowledge and sagacity in the choice of the inductions out of which he will construct his argument. But the validity of the argument when constructed, depends on principles, and must be tried by tests which are the same for all descriptions of inquiries, whether the result be to give A an estate, or to enrich science with a new general truth. In the one case and in the other, the senses, or testimony, must decide on the individual facts; the rules of the syllogism will determine whether, those facts being supposed correct, the case really falls within the formulæ of the different inductions under which it has been successively brought; and finally, the legitimacy of the inductions themselves must be decided by other rules, and these it is now our purpose to investigate. If this third part of the operation be, in many of the questions of practical life, not the most, but the least arduous portion of it, we have seen that this is also the case in some great departments of the field of science; in all those which are principally deductive, and most of all in mathematics; where the inductions themselves are few in number, and so obvious and elementary that they seem to stand in no need of the evidence of experience, while to combine them so as to prove a given theorem or solve a problem, may call for the utmost powers of invention and contrivance with which our species is gifted.

If the identity of the logical processes which prove particular facts and those which establish general scientific truths, required any additional confirmation, it would be sufficient to consider that in many branches of science, single facts have to be proved, as well as principles; facts as completely individual as any that are debated in a court of justice; but which are proved in the same manner as the other truths of the science, and without disturbing in any degree the homogeneity of its method. A remarkable example of this is afforded by astronomy. The individual facts on which that science grounds its most important deductions, such facts as the magnitudes of the bodies of the solar system, their distances from one another, the figure of the earth, and its rotation, are scarcely any of them accessible to our means of direct observation: they are proved indirectly, by the aid of inductions founded on other facts which we can more easily reach. For example, the distance of the moon from the earth was determined by a very circuitous process. The share which direct observation had in the work consisted in ascertaining, at one and the same instant, the zenith distances of the moon, as seen from two points very remote from one another on the earth's surface. The ascertainment of these angular distances ascertained their supplements; and since the angle at the earth's centre subtended by the distance between the two places of observation was deducible by spherical trigonometry from the latitude and longitude of those places, the angle at the moon subtended by the same line became the fourth angle of a quadrilateral of which the other three angles were known. The four angles being thus ascertained, and two sides of the quadrilateral being radii of the earth; the two remaining sides and the diagonal, or, in other words, the moon's distance from the two places of observation and from the centre of the earth, could be ascertained, at least in terms of the earth's radius, from elementary theorems of geometry. At each step in this demonstration a new induction is taken in, represented in the aggregate of its results by a general proposition.

Not only is the process by which an individual astronomical fact was thus ascertained, exactly similar to those by which the same science establishes its general truths, but also (as we have shown to be the case in all legitimate reasoning) a general proposition might have been concluded instead of a single fact. In strictness, indeed, the result of the reasoning *is* a general proposition; a theorem respecting the distance, not of the moon in particular, but of any inaccessible object; showing in what relation that distance stands to certain other quantities. And although the moon is almost the only heavenly body the distance of which from the earth can really be thus ascertained, this is merely owing to the accidental circumstances of the other heavenly bodies, which render them incapable of affording such data as the application of the theorem requires; for the theorem itself is as true of them as it is of the moon.*

We shall fall into no error, then, if in treating of Induction, we limit our attention to the establishment of general propositions. The principles and rules of Induction as directed to this end, are the principles and rules of all Induction; and the logic of Science is the universal Logic, applicable to all inquiries in which man can engage.

CHAPTER II.

OF INDUCTIONS IMPROPERLY SO CALLED.

§ 1. Induction, then, is that operation of the mind, by which we infer that what we know to be true in a particular case or cases, will be true in all cases which resemble the former in certain assignable respects. In other words, Induction is the process by which we conclude that what is true of certain individuals of a class is true of the whole class, or that what is true at certain times will be true in similar circumstances at all times.

This definition excludes from the meaning of the term Induction, various logical operations, to which it is not unusual to apply that name.

Induction, as above defined, is a process of inference; it proceeds from the known to the unknown; and any operation involving no inference, any process in which what seems the conclusion is no wider than the premises from which it is drawn, does not fall within the meaning of the term. Yet

* Dr. Whewell thinks it improper to apply the term Induction to any operation not terminating in the establishment of a general truth. Induction, he says (*Philosophy of Discovery*, p. 245), "is not the same thing as experience and observation. Induction is experience or observation *consciously* looked at in a *general* form. This consciousness and generality are necessary parts of that knowledge which is science." And he objects (p. 241) to the mode in which the word Induction is employed in this work, as an undue extension of that term "not only to the cases in which the general induction is consciously applied to a particular instance, but to the cases in which the particular instance is dealt with by means of experience in that rude sense in which experience can be asserted of brutes, and in which of course we can in no way imagine that the law is possessed or understood as a general proposition." This use of the term he deems a "confusion of knowledge with practical tendencies."

I disclaim, as strongly as Dr. Whewell can do, the application of such terms as induction, inference, or reasoning, to operations performed by mere instinct, that is, from an animal impulse, without the exertion of any intelligence. But I perceive no ground for confining the use of those terms to cases in which the inference is drawn in the forms and with the precautions required by scientific propriety. To the idea of Science, an express recognition and distinct apprehension of general laws as such, is essential: but nine-tenths of the conclusions drawn from experience in the course of practical life, are drawn without any such recognition: they are direct inferences from known cases, to a case supposed to be similar. I have endeavored to show that this is not only as legitimate an operation, but substantially the same operation, as that of ascending from known cases to a general proposition; except that the latter process has one great security for correctness which the former does not possess. In science, the inference must necessarily pass through the intermediate stage of a general proposition, because Science wants its conclusions for record, and not for instantaneous use. But the inferences drawn for the guidance of practical affairs, by persons who would often be quite incapable of expressing in unexceptionable terms the corresponding generalizations, may and frequently do exhibit intellectual powers quite equal to any which have ever been displayed in science; and if these inferences are not inductive, what are they? The limitation imposed on the term by Dr. Whewell seems perfectly arbitrary; neither justified by any fundamental distinction between what he includes and what he desires to exclude, nor sanctioned by usage, at least from the time of Reid and Stewart, the principal legislators (as far as the English language is concerned) of modern metaphysical terminology.

in the common books of Logic we find this laid down as the most perfect, indeed the only quite perfect, form of induction. In those books, every process which sets out from a less general and terminates in a more general expression—which admits of being stated in the form, "This and that A are B, therefore every A is B"—is called an induction, whether any thing be really concluded or not: and the induction is asserted not to be perfect, unless every single individual of the class A is included in the antecedent, or premise: that is, unless what we affirm of the class has already been ascertained to be true of every individual in it, so that the nominal conclusion is not really a conclusion, but a mere re-assertion of the premises. If we were to say, All the planets shine by the sun's light, from observation of each separate planet, or All the Apostles were Jews, because this is true of Peter, Paul, John, and every other apostle—these, and such as these, would, in the phraseology in question, be called perfect, and the only perfect, Inductions. This, however, is a totally different kind of induction from ours; it is not an inference from facts known to facts unknown, but a mere short-hand registration of facts known. The two simulated arguments which we have quoted, are not generalizations; the propositions purporting to be conclusions from them, are not really general propositions. A general proposition is one in which the predicate is affirmed or denied of an unlimited number of individuals; namely, all, whether few or many, existing or capable of existing, which possess the properties connoted by the subject of the proposition. "All men are mortal" does not mean all now living, but all men past, present, and to come. When the signification of the term is limited so as to render it a name not for any and every individual falling under a certain general description, but only for each of a number of individuals, designated as such, and as it were counted off individually, the proposition, though it may be general in its language, is no general proposition, but merely that number of singular propositions, written in an abridged character. The operation may be very useful, as most forms of abridged notation are; but it is no part of the investigation of truth, though often bearing an important part in the preparation of the materials for that investigation.

As we may sum up a definite number of singular propositions in one proposition, which will be apparently, but not really, general, so we may sum up a definite number of general propositions in one proposition, which will be apparently, but not really, more general. If by a separate induction applied to every distinct species of animals, it has been established that each possesses a nervous system, and we affirm thereupon that all animals have a nervous system; this looks like a generalization, though as the conclusion merely affirms of all what has already been affirmed of each, it seems to tell us nothing but what we knew before. A distinction, however, must be made. If in concluding that all animals have a nervous system, we mean the same thing and no more as if we had said "all known animals," the proposition is not general, and the process by which it is arrived at is not induction. But if our meaning is that the observations made of the various species of animals have discovered to us a law of animal nature, and that we are in a condition to say that a nervous system will be found even in animals yet undiscovered, this indeed is an induction; but in this case the general proposition contains more than the sum of the special propositions from which it is inferred. The distinction is still more forcibly brought out when we consider, that if this real generalization be legitimate at all, its legitimacy probably does not require that we should have examined without exception every known species. It is the number and nature of the instances, and not their being the whole of those which happen to be known, that makes them sufficient evidence to prove a general law: while the more limited assertion, which stops at all known animals, can not be made unless we have rigorously verified it in every species. In like manner (to return to a former example) we might have inferred, not that all *the* planets, but that all *planets*, shine by reflected light: the former is no induction; the latter is an induction, and a bad one, being disproved by the case of double stars — self-luminous bodies which are properly planets, since they revolve round a centre.

§ 2. There are several processes used in mathematics which require to be distinguished from Induction, being not unfrequently called by that name, and being so far similar to Induction properly so called, that the propositions they lead to are really general propositions. For example, when we have proved with respect to the circle, that a straight line can not meet it in more than two points, and when the same thing has been successively proved of the ellipse, the parabola, and the hyperbola, it may be laid down as a universal property of the sections of the cone. The distinction drawn in the two previous examples can have no place here, there being no difference between all *known* sections of the cone and *all* sections, since a cone demonstrably can not be intersected by a plane except in one of these four lines. It would be difficult, therefore, to refuse to the proposition arrived at, the name of a generalization, since there is no room for any generalization beyond it. But there is no induction, because there is no inference: the conclusion is a mere summing up of what was asserted in the various propositions from which it is drawn. A case somewhat, though not altogether, similar, is the proof of a geometrical theorem by means of a diagram. Whether the diagram be on paper or only in the imagination, the demonstration (as formerly observed*) does not prove directly the general theorem; it proves only that the conclusion, which the theorem asserts generally, is true of the particular triangle or circle exhibited in the diagram; but since we perceive that in the same way in which we have proved it of that circle, it might also be proved of any other circle, we gather up into one general expression all the singular propositions susceptible of being thus proved, and embody them in a universal proposition. Having shown that the three angles of the triangle ABC are together equal to two right angles, we conclude that this is true of every other triangle, not because it is true of ABC, but for the same reason which proved it to be true of ABC. If this were to be called Induction, an appropriate name for it would be, induction by parity of reasoning. But the term can not properly belong to it; the characteristic quality of Induction is wanting, since the truth obtained, though really general, is not believed on the evidence of particular instances. We do not conclude that all triangles have the property because some triangles have, but from the ulterior demonstrative evidence which was the ground of our conviction in the particular instances.

There are nevertheless, in mathematics, some examples of so-called Induction, in which the conclusion does bear the appearance of a generalization grounded on some of the particular cases included in it. A mathematician, when he has calculated a sufficient number of the terms of an al-

* Supra, p. 145.

gebraical or arithmetical series to have ascertained what is called the *law* of the series, does not hesitate to fill up any number of the succeeding terms without repeating the calculations. But I apprehend he only does so when it is apparent from *a priori* considerations (which might be exhibited in the form of demonstration) that the mode of formation of the subsequent terms, each from that which preceded it, must be similar to the formation of the terms which have been already calculated. And when the attempt has been hazarded without the sanction of such general considerations, there are instances on record in which it has led to false results.

It is said that Newton discovered the binomial theorem by induction; by raising a binomial successively to a certain number of powers, and comparing those powers with one another until he detected the relation in which the algebraic formula of each power stands to the exponent of that power, and to the two terms of the binomial. The fact is not improbable: but a mathematician like Newton, who seemed to arrive *per saltum* at principles and conclusions that ordinary mathematicians only reached by a succession of steps, certainly could not have performed the comparison in question without being led by it to the *a priori* ground of the law; since any one who understands sufficiently the nature of multiplication to venture upon multiplying several lines of symbols at one operation, can not but perceive that in raising a binomial to a power, the co-efficients must depend on the laws of permutation and combination: and as soon as this is recognized, the theorem is demonstrated. Indeed, when once it was seen that the law prevailed in a few of the lower powers, its identity with the law of permutation would at once suggest the considerations which prove it to obtain universally. Even, therefore, such cases as these, are but examples of what I have called Induction by parity of reasoning, that is, not really Induction, because not involving inference of a general proposition from particular instances.

§ 3. There remains a third improper use of the term Induction, which it is of real importance to clear up, because the theory of Induction has been, in no ordinary degree, confused by it, and because the confusion is exemplified in the most recent and elaborate treatise on the inductive philosophy which exists in our language. The error in question is that of confounding a mere description, by general terms, of a set of observed phenomena, with an induction from them.

Suppose that a phenomenon consists of parts, and that these parts are only capable of being observed separately, and as it were piecemeal. When the observations have been made, there is a convenience (amounting for many purposes to a necessity) in obtaining a representation of the phenomenon as a whole, by combining, or as we may say, piecing these detached fragments together. A navigator sailing in the midst of the ocean discovers land: he can not at first, or by any one observation, determine whether it is a continent or an island; but he coasts along it, and after a few days finds himself to have sailed completely round it: he then pronounces it an island. Now there was no particular time or place of observation at which he could perceive that this land was entirely surrounded by water: he ascertained the fact by a succession of partial observations, and then selected a general expression which summed up in two or three words the whole of what he so observed. But is there any thing of the nature of an induction in this process? Did he infer any thing that had not been observed, from something else which had? Certainly not. He had observed the whole of what the proposition asserts. That the land in question is an island, is not an inference from the partial facts which the navigator saw in the course of his circumnavigation; it is the facts themselves; it is a summary of those facts; the description of a complex fact, to which those simpler ones are as the parts of a whole.

Now there is, I conceive, no difference in kind between this simple operation, and that by which Kepler ascertained the nature of the planetary orbits: and Kepler's operation, all at least that was characteristic in it, was not more an inductive act than that of our supposed navigator.

The object of Kepler was to determine the real path described by each of the planets, or let us say by the planet Mars (since it was of that body that he first established the two of his three laws which did not require a comparison of planets). To do this there was no other mode than that of direct observation: and all which observation could do was to ascertain a great number of the successive places of the planet; or rather, of its apparent places. That the planet occupied successively all these positions, or at all events, positions which produced the same impressions on the eye, and that it passed from one of these to another insensibly, and without any apparent breach of continuity; thus much the senses, with the aid of the proper instruments, could ascertain. What Kepler did more than this, was to find what sort of a curve these different points would make, supposing them to be all joined together. He expressed the whole series of the observed places of Mars by what Dr. Whewell calls the general conception of an ellipse. This operation was far from being as easy as that of the navigator who expressed the series of his observations on successive points of the coast by the general conception of an island. But it is the very same sort of operation; and if the one is not an induction but a description, this must also be true of the other.

The only real induction concerned in the case, consisted in inferring that because the observed places of Mars were correctly represented by points in an imaginary ellipse, therefore Mars would continue to revolve in that same ellipse; and in concluding (before the gap had been filled up by further observations) that the positions of the planet during the time which intervened between two observations, must have coincided with the intermediate points of the curve. For these were facts which had not been directly observed. They were inferences from the observations; facts inferred, as distinguished from facts seen. But these inferences were so far from being a part of Kepler's philosophical operation, that they had been drawn long before he was born. Astronomers had long known that the planets periodically returned to the same places. When this had been ascertained, there was no induction left for Kepler to make, nor did he make any further induction. He merely applied his new conception to the facts inferred, as he did to the facts observed. Knowing already that the planets continued to move in the same paths; when he found that an ellipse correctly represented the past path, he knew that it would represent the future path. In finding a compendious expression for the one set of facts, he found one for the other: but he found the expression only, not the inference; nor did he (which is the true test of a general truth) add any thing to the power of prediction already possessed.

§ 4. The descriptive operation which enables a number of details to be summed up in a single proposition, Dr. Whewell, by an aptly chosen expression, has termed the Colligation of Facts. In most of his observations concerning that mental process I fully agree, and would gladly transfer all that portion of his book into my own pages. I only think him mistaken in setting up this kind of operation, which according to the old and received meaning of the term, is not induction at all, as the type of induction generally; and laying down, throughout his work, as principles of induction, the principles of mere colligation.

Dr. Whewell maintains that the general proposition which binds together the particular facts, and makes them, as it were, one fact, is not the mere sum of those facts, but something more, since there is introduced a conception of the mind, which did not exist in the facts themselves. "The particular facts," says he,* "are not merely brought together, but there is a new element added to the combination by the very act of thought by which they are combined. .... When the Greeks, after long observing the motions of the planets, saw that these motions might be rightly considered as produced by the motion of one wheel revolving in the inside of another wheel, these wheels were creations of their minds, added to the facts which they perceived by sense. And even if the wheels were no longer supposed to be material, but were reduced to mere geometrical spheres or circles, they were not the less products of the mind alone—something additional to the facts observed. The same is the case in all other discoveries. The facts are known, but they are insulated and unconnected, till the discoverer supplies from his own store a principle of connection. The pearls are there, but they will not hang together till some one provides the string."

Let me first remark that Dr. Whewell, in this passage, blends together, indiscriminately, examples of both the processes which I am endeavoring to distinguish from one another. When the Greeks abandoned the supposition that the planetary motions were produced by the revolution of material wheels, and fell back upon the idea of "mere geometrical spheres or circles," there was more in this change of opinion than the mere substitution of an ideal curve for a physical one. There was the abandonment of a theory, and the replacement of it by a mere description. No one would think of calling the doctrine of material wheels a mere description. That doctrine was an attempt to point out the force by which the planets were acted upon, and compelled to move in their orbits. But when, by a great step in philosophy, the materiality of the wheels was discarded, and the geometrical forms alone retained, the attempt to account for the motions was given up, and what was left of the theory was a mere description of the orbits. The assertion that the planets were carried round by wheels revolving in the inside of other wheels, gave place to the proposition, that they moved in the same lines which would be traced by bodies so carried: which was a mere mode of representing the sum of the observed facts; as Kepler's was another and a better mode of representing the same observations.

It is true that for these simply descriptive operations, as well as for the erroneous inductive one, a conception of the mind was required. The conception of an ellipse must have presented itself to Kepler's mind, before he could identify the planetary orbits with it. According to Dr. Whewell, the conception was something added to the facts. He expresses himself as if Kepler had put something into the facts by his mode of conceiving them. But Kepler did no such thing. The ellipse was in the facts before Kepler recognized it; just as the island was an island before it had been

* *Novum Organum Renovatum*, pp. 72, 73.

sailed round. Kepler did not *put* what he had conceived into the facts, but *saw* it in them. A conception implies, and corresponds to, something conceived: and though the conception itself is not in the facts, but in our mind, yet if it is to convey any knowledge relating to them, it must be a conception *of* something which really is in the facts, some property which they actually possess, and which they would manifest to our senses, if our senses were able to take cognizance of it. If, for instance, the planet left behind it in space a visible track, and if the observer were in a fixed position at such a distance from the plane of the orbit as would enable him to see the whole of it at once, he would see it to be an ellipse; and if gifted with appropriate instruments and powers of locomotion, he could prove it to be such by measuring its different dimensions. Nay, further: if the track were visible, and he were so placed that he could see all parts of it in succession, but not all of them at once, he might be able, by piecing together his successive observations, to discover both that it was an ellipse and that the planet moved in it. The case would then exactly resemble that of the navigator who discovers the land to be an island by sailing round it. If the path was visible, no one I think would dispute that to identify it with an ellipse is to describe it: and I can not see why any difference should be made by its not being directly an object of sense, when every point in it is as exactly ascertained as if it were so.

Subject to the indispensable condition which has just been stated, I do not conceive that the part which conceptions have in the operation of studying facts, has ever been overlooked or undervalued. No one ever disputed that in order to reason about any thing we must have a conception of it; or that when we include a multitude of things under a general expression, there is implied in the expression a conception of something common to those things. But it by no means follows that the conception is necessarily pre-existent, or constructed by the mind out of its own materials. If the facts are rightly classed under the conception, it is because there is in the facts themselves something of which the conception is itself a copy; and which if we can not directly perceive, it is because of the limited power of our organs, and not because the thing itself is not there. The conception itself is often obtained by abstraction from the very facts which, in Dr. Whewell's language, it is afterward called in to connect. This he himself admits, when he observes (which he does on several occasions), how great a service would be rendered to the science of physiology by the philosopher "who should establish a precise, tenable, and consistent conception of life."* Such a conception can only be abstracted from the phenomena of life itself; from the very facts which it is put in requisition to connect. In other cases, no doubt, instead of collecting the conception from the very phenomena which we are attempting to colligate, we select it from among those which have been previously collected by abstraction from other facts. In the instance of Kepler's laws, the latter was the case. The facts being out of the reach of being observed, in any such manner as would have enabled the senses to identify directly the path of the planet, the conception requisite for framing a general description of that path could not be collected by abstraction from the observations themselves; the mind had to supply hypothetically, from among the conceptions it had obtained from other portions of its experience, some one which would correctly represent the series of the observed facts. It had

* *Novum Organum Renovatum*, p. 32.

to frame a supposition respecting the general course of the phenomenon, and ask itself, If this be the general description, what will the details be? and then compare these with the details actually observed. If they agreed, the hypothesis would serve for a description of the phenomenon: if not, it was necessarily abandoned, and another tried. It is such a case as this which gives rise to the doctrine that the mind, in framing the descriptions, adds something of its own which it does not find in the facts.

Yet it is a fact surely, that the planet does describe an ellipse; and a fact which we could see, if we had adequate visual organs and a suitable position. Not having these advantages, but possessing the conception of an ellipse, or (to express the meaning in less technical language) knowing what an ellipse was, Kepler tried whether the observed places of the planet were consistent with such a path. He found they were so; and he, consequently, asserted as a fact that the planet moved in an ellipse. But this fact, which Kepler did not add to, but found in, the motions of the planet, namely, that it occupied in succession the various points in the circumference of a given ellipse, was the very fact, the separate parts of which had been separately observed; it was the sum of the different observations.

Having stated this fundamental difference between my opinion and that of Dr. Whewell, I must add, that his account of the manner in which a conception is selected, suitable to express the facts, appears to me perfectly just. The experience of all thinkers will, I believe, testify that the process is tentative; that it consists of a succession of guesses; many being rejected, until one at last occurs fit to be chosen. We know from Kepler himself that before hitting upon the "conception" of an ellipse, he tried nineteen other imaginary paths, which, finding them inconsistent with the observations, he was obliged to reject. But as Dr. Whewell truly says, the successful hypothesis, though a guess, ought generally to be called, not a lucky, but a skillful guess. The guesses which serve to give mental unity and wholeness to a chaos of scattered particulars, are accidents which rarely occur to any minds but those abounding in knowledge and disciplined in intellectual combinations.

How far this tentative method, so indispensable as a means to the colligation of facts for purposes of description, admits of application to Induction itself, and what functions belong to it in that department, will be considered in the chapter of the present Book which relates to Hypotheses. On the present occasion we have chiefly to distinguish this process of Colligation from Induction properly so called; and that the distinction may be made clearer, it is well to advert to a curious and interesting remark, which is as strikingly true of the former operation, as it appears to me unequivocally false of the latter.

In different stages of the progress of knowledge, philosophers have employed, for the colligation of the same order of facts, different conceptions. The early rude observations of the heavenly bodies, in which minute precision was neither attained nor sought, presented nothing inconsistent with the representation of the path of a planet as an exact circle, having the earth for its centre. As observations increased in accuracy, facts were disclosed which were not reconcilable with this simple supposition: for the colligation of those additional facts, the supposition was varied; and varied again and again as facts became more numerous and precise. The earth was removed from the centre to some other point within the circle; the planet was supposed to revolve in a smaller circle called an epicycle, round an imaginary point which revolved in a circle round the earth: in proportion as observation elicited fresh facts contradictory to these representations, other epicycles and other eccentrics were added, producing additional complication; until at last Kepler swept all these circles away, and substituted the conception of an exact ellipse. Even this is found not to represent with complete correctness the accurate observations of the present day, which disclose many slight deviations from an orbit exactly elliptical. Now Dr. Whewell has remarked that these successive general expressions, though apparently so conflicting, were all correct: they all answered the purpose of colligation; they all enabled the mind to represent to itself with facility, and by a simultaneous glance, the whole body of facts at the time ascertained: each in its turn served as a correct description of the phenomena, so far as the senses had up to that time taken cognizance of them. If a necessity afterward arose for discarding one of these general descriptions of the planet's orbit, and framing a different imaginary line, by which to express the series of observed positions, it was because a number of new facts had now been added, which it was necessary to combine with the old facts into one general description. But this did not affect the correctness of the former expression, considered as a general statement of the only facts which it was intended to represent. And so true is this, that, as is well remarked by M. Comte, these ancient generalizations, even the rudest and most imperfect of them, that of uniform movement in a circle, are so far from being entirely false, that they are even now habitually employed by astronomers when only a rough approximation to correctness is required. "L'astronomie moderne, en détruisant sans retour les hypothèses primitives, envisagées comme lois réelles du monde, a soigneusement maintenu leur valeur positive et permanente, la propriété de représenter commodément les phénomènes quand il s'agit d'une première ébauche. Nos ressources à cet égard sont même bien plus étendues, précisément à cause que nous ne nous faisons aucune illusion sur la réalité des hypothèses; ce qui nous permet d'employer sans scrupule, en chaque cas, celle que nous jugeons la plus avantageuse."*

Dr. Whewell's remark, therefore, is philosophically correct. Successive expressions for the colligation of observed facts, or, in other words, successive descriptions of a phenomenon as a whole, which has been observed only in parts, may, though conflicting, be all correct as far as they go. But it would surely be absurd to assert this of conflicting inductions.

The scientific study of facts may be undertaken for three different purposes: the simple description of the facts; their explanation; or their prediction: meaning by prediction, the determination of the conditions under which similar facts may be expected again to occur. To the first of these three operations the name of Induction does not properly belong: to the other two it does. Now, Dr. Whewell's observation is true of the first alone. Considered as a mere description, the circular theory of the heavenly motions represents perfectly well their general features: and by adding epicycles without limit, those motions, even as now known to us, might be expressed with any degree of accuracy that might be required. The elliptical theory, as a mere description, would have a great advantage in point of simplicity, and in the consequent facility of conceiving it and reasoning about it; but it would not really be more true than the other. Different descriptions, therefore, may be all true: but not, surely, different explanations. The doctrine that the heavenly bodies moved by a virtue inherent

* *Cours de Philosophie Positive*, vol. ii., p. 202.

in their celestial nature; the doctrine that they were moved by impact (which led to the hypothesis of vortices as the only impelling force capable of whirling bodies in circles), and the Newtonian doctrine, that they are moved by the composition of a centripetal with an original projectile force; all these are explanations, collected by real induction from supposed parallel cases; and they were all successively received by philosophers, as scientific truths on the subject of the heavenly bodies. Can it be said of these, as was said of the different descriptions, that they are all true as far as they go? Is it not clear that only one can be true in any degree, and the other two must be altogether false? So much for explanations: let us now compare different predictions: the first, that eclipses will occur when one planet or satellite is so situated as to cast its shadow upon another; the second, that they will occur when some great calamity is impending over mankind. Do these two doctrines only differ in the degree of their truth, as expressing real facts with unequal degrees of accuracy? Assuredly the one is true, and the other absolutely false.*

* Dr. Whewell, in his reply, contests the distinction here drawn, and maintains, that not only different descriptions, but different explanations of a phenomenon, may all be true. Of the three theories respecting the motions of the heavenly bodies, he says (*Philosophy of Discovery*, p. 231): "Undoubtedly all these explanations may be true and consistent with each other, and would be so if each had been followed out so as to show in what manner it could be made consistent with the facts. And this was, in reality, in a great measure done. The doctrine that the heavenly bodies were moved by vortices was successfully modified, so that it came to coincide in its results with the doctrine of an inverse-quadratic centripetal force. ...... When this point was reached, the vortex was merely a machinery, well or ill devised, for producing such a centripetal force, and therefore did not contradict the doctrine of a centripetal force. Newton himself does not appear to have been averse to explaining gravity by impulse. So little is it true that if one theory be true the other must be false. The attempt to explain gravity by the impulse of streams of particles flowing through the universe in all directions, which I have mentioned in the *Philosophy*, is so far from being inconsistent with the Newtonian theory, that it is founded entirely upon it. And even with regard to the doctrine, that the heavenly bodies move by an inherent virtue; if this doctrine had been maintained in any such way that it was brought to agree with the facts, the inherent virtue must have had its laws determined; and then it would have been found that the virtue had a reference to the central body; and so, the 'inherent virtue' must have coincided in its effect with the Newtonian force; and then, the two explanations would agree, except so far as the word 'inherent' was concerned. And if such a part of an earlier theory as this word *inherent* indicates, is found to be untenable, it is of course rejected in the transition to later and more exact theories, in Inductions of this kind, as well as in what Mr. Mill calls Descriptions. There is, therefore, still no validity discoverable in the distinction which Mr. Mill attempts to draw between descriptions like Kepler's law of elliptical orbits, and other examples of induction."

If the doctrine of vortices had meant, not that vortices existed, but only that the planets moved *in the same manner* as if they had been whirled by vortices; if the hypothesis had been merely a mode of representing the facts, not an attempt to account for them; if, in short, it had been only a Description; it would, no doubt, have been reconcilable with the Newtonian theory. The vortices, however, were not a mere aid to conceiving the motions of the planets, but a supposed physical agent, actively impelling them; a material fact, which might be true or not true, but could not be both true and not true. According to Descartes's theory it was true, according to Newton's it was not true. Dr. Whewell probably means that since the phrases, centripetal and projectile force, do not declare the nature but only the direction of the forces, the Newtonian theory does not absolutely contradict any hypothesis which may be framed respecting the mode of their production. The Newtonian theory, regarded as a mere *description* of the planetary motions, does not; but the Newtonian theory as an *explanation* of them does. For in what does the explanation consist? In ascribing those motions to a general law which obtains between all particles of matter, and in identifying this with the law by which bodies fall to the ground. If the planets are kept in their orbits by a force which draws the particles composing them toward every other particle of matter in the solar system, they are not kept in those orbits by the impulsive force of certain streams of matter which whirl them round. The one explanation absolutely excludes the other. Either the planets are not moved by vortices, or they do not move by a law common to all matter. It is im-

In every way, therefore, it is evident that to explain induction as the colligation of facts by means of appropriate conceptions, that is, conceptions which will really express them, is to confound mere description of the observed facts with inference from those facts, and ascribe to the latter what is a characteristic property of the former.

There is, however, between Colligation and Induction, a real correlation, which it is important to conceive correctly. Colligation is not always induction; but induction is always colligation. The assertion that the planets move in ellipses, was but a mode of representing observed facts; it was but a colligation; while the assertion that they are drawn, or tend, toward the sun, was the statement of a new fact, inferred by induction. But the induction, once made, accomplishes the purposes of colligation likewise. It brings the same facts, which Kepler had connected by his conception of an ellipse, under the additional conception of bodies acted upon by a central force, and serves, therefore, as a new bond of connection for those facts; a new principle for their classification.

Further, the descriptions which are improperly confounded with induction, are nevertheless a necessary preparation for induction; no less necessary than correct observation of the facts themselves. Without the previous colligation of detached observations by means of one general conception, we could never have obtained any basis for an induction, except in the case of phenomena of very limited compass. We should not be able

possible that both opinions can be true. As well might it be said that there is no contradiction between the assertions, that a man died because somebody killed him, and that he died a natural death.

So, again, the theory that the planets move by a virtue inherent in their celestial nature, is incompatible with either of the two others: either that of their being moved by vortices, or that which regards them as moving by a property which they have in common with the earth and all terrestrial bodies. Dr. Whewell says that the theory of an inherent virtue agrees with Newton's when the word inherent is left out, which of course it would be (he says) if "found to be untenable." But leave that out, and where is the theory? The word inherent *is* the theory. When that is omitted, there remains nothing except that the heavenly bodies move "by a virtue," *i. e.*, by a power of some sort; or by virtue of their celestial nature, which directly contradicts the doctrine that terrestrial bodies fall by the same law.

If Dr. Whewell is not yet satisfied, any other subject will serve equally well to test his doctrine. He will hardly say that there is no contradiction between the emission theory and the undulatory theory of light; or that there can be both one and two electricities; or that the hypothesis of the production of the higher organic forms by development from the lower, and the supposition of separate and successive acts of creation, are quite reconcilable; or that the theory that volcanoes are fed from a central fire, and the doctrines which ascribe them to chemical action at a comparatively small depth below the earth's surface, are consistent with one another, and all true as far as they go.

If different explanations of the same fact can not both be true, still less, surely, can different predictions. Dr. Whewell quarrels (on what ground it is not necessary here to consider) with the example I had chosen on this point, and thinks an objection to an illustration a sufficient answer to a theory. Examples not liable to his objection are easily found, if the proposition that conflicting predictions can not both be true, can be made clearer by any examples. Suppose the phenomenon to be a newly-discovered comet, and that one astronomer predicts its return once in every 300 years—another once in every 400: can they both be right? When Columbus predicted that by sailing constantly westward he should in time return to the point from which he set out, while others asserted that he could never do so except by turning back, were both he and his opponents true prophets? Were the predictions which foretold the wonders of railways and steamships, and those which averred that the Atlantic could never be crossed by steam navigation, nor a railway train propelled ten miles an hour, both (in Dr. Whewell's words) "true, and consistent with one another?"

Dr. Whewell sees no distinction between holding contradictory opinions on a question of fact, and merely employing different analogies to facilitate the conception of the same fact. The case of different Inductions belongs to the former class, that of different Descriptions to the latter.

to affirm any predicates at all, of a subject incapable of being observed otherwise than piecemeal: much less could we extend those predicates by induction to other similar subjects. Induction, therefore, always presupposes, not only that the necessary observations are made with the necessary accuracy, but also that the results of these observations are, so far as practicable, connected together by general descriptions, enabling the mind to represent to itself as wholes whatever phenomena are capable of being so represented.

§ 5. Dr. Whewell has replied at some length to the preceding observations, restating his opinions, but without (as far as I can perceive) adding any thing material to his former arguments. Since, however, mine have not had the good fortune to make any impression upon him, I will subjoin a few remarks, tending to show more clearly in what our difference of opinion consists, as well as, in some measure, to account for it.

Nearly all the definitions of induction, by writers of authority, make it consist in drawing inferences from known cases to unknown; affirming of a class, a predicate which has been found true of some cases belonging to the class; concluding because some things have a certain property, that other things which resemble them have the same property—or because a thing has manifested a property at a certain time, that it has and will have that property at other times.

It will scarcely be contended that Kepler's operation was an Induction in this sense of the term. The statement, that Mars moves in an elliptical orbit, was no generalization from individual cases to a class of cases. Neither was it an extension to all time, of what had been found true at some particular time. The whole amount of generalization which the case admitted of, was already completed, or might have been so. Long before the elliptic theory was thought of, it had been ascertained that the planets returned periodically to the same apparent places; the series of these places was, or might have been, completely determined, and the apparent course of each planet marked out on the celestial globe in an uninterrupted line. Kepler did not extend an observed truth to other cases than those in which it had been observed: he did not widen the *subject* of the proposition which expressed the observed facts. The alteration he made was in the predicate. Instead of saying, the successive places of Mars are so and so, he summed them up in the statement, that the successive places of Mars are points in an ellipse. It is true, this statement, as Dr. Whewell says, was not the sum of the observations *merely;* it was the sum of the observations *seen under a new point of view.** But it was not the sum of *more* than the observations, as a real induction is. It took in no cases but those which had been actually observed, or which could have been inferred from the observations before the new point of view presented itself. There was not that transition from known cases to unknown, which constitutes Induction in the original and acknowledged meaning of the term.

Old definitions, it is true, can not prevail against new knowledge: and if the Keplerian operation, as a logical process, be really identical with what takes place in acknowledged induction, the definition of induction ought to be so widened as to take it in; since scientific language ought to adapt itself to the true relations which subsist between the things it is employed to designate. Here then it is that I am at issue with Dr. Whewell. He

* *Phil. of Discov.*, p. 256.

does think the operations identical. He allows of no logical process in any case of induction, other than what there was in Kepler's case, namely, guessing until a guess is found which tallies with the facts; and accordingly, as we shall see hereafter, he rejects all canons of induction, because it is not by means of them that we guess. Dr. Whewell's theory of the logic of science would be very perfect if it did not pass over altogether the question of Proof. But in my apprehension there is such a thing as proof, and inductions differ altogether from descriptions in their relation to that element. Induction is proof; it is inferring something unobserved from something observed: it requires, therefore, an appropriate test of proof; and to provide that test, is the special purpose of inductive logic. When, on the contrary, we merely collate known observations, and, in Dr. Whewell's phraseology, connect them by means of a new conception; if the conception does serve to connect the observations, we have all we want. As the proposition in which it is embodied pretends to no other truth than what it may share with many other modes of representing the same facts, to be consistent with the facts is all it requires: it neither needs nor admits of proof; though it may serve to prove other things, inasmuch as, by placing the facts in mental connection with other facts, not previously seen to resemble them, it assimilates the case to another class of phenomena, concerning which real Inductions have already been made. Thus Kepler's so-called law brought the orbit of Mars into the class ellipse, and by doing so, proved all the properties of an ellipse to be true of the orbit: but in this proof Kepler's law supplied the minor premise, and not (as is the case with real Inductions) the major.

Dr. Whewell calls nothing Induction where there is not a new mental conception introduced, and every thing induction where there is. But this is to confound two very different things, Invention and Proof. The introduction of a new conception belongs to Invention: and invention may be required in any operation, but is the essence of none. A new conception may be introduced for descriptive purposes, and so it may for inductive purposes. But it is so far from constituting induction, that induction does not necessarily stand in need of it. Most inductions require no conception but what was present in every one of the particular instances on which the induction is grounded. That all men are mortal is surely an inductive conclusion; yet no new conception is introduced by it. Whoever knows that any man has died, has all the conceptions involved in the inductive generalization. But Dr. Whewell considers the process of invention which consists in framing a new conception consistent with the facts, to be not merely a necessary part of all induction, but the whole of it.

The mental operation which extracts from a number of detached observations certain general characters in which the observed phenomena resemble one another, or resemble other known facts, is what Bacon, Locke, and most subsequent metaphysicians, have understood by the word Abstraction. A general expression obtained by abstraction, connecting known facts by means of common characters, but without concluding from them to unknown, may, I think, with strict logical correctness, be termed a Description; nor do I know in what other way things can ever be described. My position, however, does not depend on the employment of that particular word; I am quite content to use Dr. Whewell's term Colligation, or the more general phrases, "mode of representing, or of expressing, phenomena:" provided it be clearly seen that the process is not Induction, but something radically different.

What more may usefully be said on the subject of Colligation, or of the correlative expression invented by Dr. Whewell, the Explication of Conceptions, and generally on the subject of ideas and mental representations as connected with the study of facts, will find a more appropriate place in the Fourth Book, on the Operations Subsidiary to Induction: to which I must refer the reader for the removal of any difficulty which the present discussion may have left.

CHAPTER III.

OF THE GROUND OF INDUCTION.

§ 1. Induction properly so called, as distinguished from those mental operations, sometimes, though improperly, designated by the name, which I have attempted in the preceding chapter to characterize, may, then, be summarily defined as Generalization from Experience. It consists in inferring from some individual instances in which a phenomenon is observed to occur, that it occurs in all instances of a certain class; namely, in all which *resemble* the former, in what are regarded as the material circumstances.

In what way the material circumstances are to be distinguished from those which are immaterial, or why some of the circumstances are material and others not so, we are not yet ready to point out. We must first observe, that there is a principle implied in the very statement of what Induction is; an assumption with regard to the course of nature and the order of the universe; namely, that there are such things in nature as parallel cases; that what happens once, will, under a sufficient degree of similarity of circumstances, happen again, and not only again, but as often as the same circumstances recur. This, I say, is an assumption, involved in every case of induction. And, if we consult the actual course of nature, we find that the assumption is warranted. The universe, so far as known to us, is so constituted, that whatever is true in any one case, is true in all cases of a certain description; the only difficulty is, to find what description.

This universal fact, which is our warrant for all inferences from experience, has been described by different philosophers in different forms of language: that the course of nature is uniform; that the universe is governed by general laws; and the like. One of the most usual of these modes of expression, but also one of the most inadequate, is that which has been brought into familiar use by the metaphysicians of the school of Reid and Stewart. The disposition of the human mind to generalize from experience—a propensity considered by these philosophers as an instinct of our nature—they usually describe under some such name as "our intuitive conviction that the future will resemble the past." Now it has been well pointed out by Mr. Bailey,* that (whether the tendency be or not an original and ultimate element of our nature), Time, in its modifications of past, present, and future, has no concern either with the belief itself, or with the grounds of it. We believe that fire will burn to-morrow, because it burned to-day and yesterday; but we believe, on precisely the same grounds, that it burned before we were born, and that it burns this very day in Cochin-China. It is not from the past to the future, as past and future, that we

* *Essays on the Pursuit of Truth.*

infer, but from the known to the unknown; from facts observed to facts unobserved; from what we have perceived, or been directly conscious of, to what has not come within our experience. In this last predicament is the whole region of the future; but also the vastly greater portion of the present and of the past.

Whatever be the most proper mode of expressing it, the proposition that the course of nature is uniform, is the fundamental principle, or general axiom, of Induction. It would yet be a great error to offer this large generalization as any explanation of the inductive process. On the contrary, I hold it to be itself an instance of induction, and induction by no means of the most obvious kind. Far from being the first induction we make, it is one of the last, or at all events one of those which are latest in attaining strict philosophical accuracy. As a general maxim, indeed, it has scarcely entered into the minds of any but philosophers; nor even by them, as we shall have many opportunities of remarking, have its extent and limits been always very justly conceived. The truth is, that this great generalization is itself founded on prior generalizations. The obscurer laws of nature were discovered by means of it, but the more obvious ones must have been understood and assented to as general truths before it was ever heard of. We should never have thought of affirming that all phenomena take place according to general laws, if we had not first arrived, in the case of a great multitude of phenomena, at some knowledge of the laws themselves; which could be done no otherwise than by induction. In what sense, then, can a principle, which is so far from being our earliest induction, be regarded as our warrant for all the others? In the only sense, in which (as we have already seen) the general propositions which we place at the head of our reasonings when we throw them into syllogisms, ever really contribute to their validity. As Archbishop Whately remarks, every induction is a syllogism with the major premise suppressed; or (as I prefer expressing it) every induction may be thrown into the form of a syllogism, by supplying a major premise. If this be actually done, the principle which we are now considering, that of the uniformity of the course of nature, will appear as the ultimate major premise of all inductions, and will, therefore, stand to all inductions in the relation in which, as has been shown at so much length, the major proposition of a syllogism always stands to the conclusion; not contributing at all to prove it, but being a necessary condition of its being proved; since no conclusion is proved, for which there can not be found a true major premise.*

* In the first edition a note was appended at this place, containing some criticism on Archbishop Whately's mode of conceiving the relation between Syllogism and Induction. In a subsequent issue of his *Logic*, the Archbishop made a reply to the criticism, which induced me to cancel part of the note, incorporating the remainder in the text. In a still later edition, the Archbishop observes in a tone of something like disapprobation, that the objections, "doubtless from their being fully answered and found untenable, were silently suppressed," and that hence he might appear to some of his readers to be combating a shadow. On this latter point, the Archbishop need give himself no uneasiness. His readers, I make bold to say, will fully credit his mere affirmation that the objections have actually been made.

But as he seems to think that what he terms the suppression of the objections ought not to have been made "silently," I now break that silence, and state exactly what it is that I suppressed, and why. I suppressed that alone which might be regarded as personal criticism on the Archbishop. I had imputed to him the having omitted to ask himself a particular question. I found that he had asked himself the question, and could give it an answer consistent with his own theory. I had also, within the compass of a parenthesis, hazarded some remarks on certain general characteristics of Archbishop Whately as a philosopher. These remarks, though their tone, I hope, was neither disrespectful nor arrogant, I felt, on reconsideration, that I was hardly entitled to make; least of all, when the instance which I had re-

The statement, that the uniformity of the course of nature is the ultimate major premise in all cases of induction, may be thought to require some explanation. The immediate major premise in every inductive argument, it certainly is not. Of that, Archbishop Whately's must be held to be the correct account. The induction, "John, Peter, etc., are mortal, therefore all mankind are mortal," may, as he justly says, be thrown into a syllogism by prefixing as a major premise (what is at any rate a necessary condition of the validity of the argument), namely, that what is true of John, Peter, etc., is true of all mankind. But how came we by this major premise? It is not self-evident; nay, in all cases of unwarranted generalization, it is not true. How, then, is it arrived at? Necessarily either by induction or ratiocination; and if by induction, the process, like all other inductive arguments, may be thrown into the form of a syllogism. This previous syllogism it is, therefore, necessary to construct. There is, in the long run, only one possible construction. The real proof that what is true of John, Peter, etc., is true of all mankind, can only be, that a different supposition would be inconsistent with the uniformity which we know to exist in the course of nature. Whether there would be this inconsistency or not, may be a matter of long and delicate inquiry; but unless there would, we have no sufficient ground for the major of the inductive syllogism. It hence appears, that if we throw the whole course of any inductive argument into a series of syllogisms, we shall arrive by more or fewer steps at an ultimate syllogism, which will have for its major premise the principle, or axiom, of the uniformity of the course of nature.*

It was not to be expected that in the case of this axiom, any more than of other axioms, there should be unanimity among thinkers with respect to the grounds on which it is to be received as true. I have already stated that I regard it as itself a generalization from experience. Others hold it to be a principle which, antecedently to any verification by experience, we

garded as an illustration of them, failed, as I now saw, to bear them out. The real matter at the bottom of the whole dispute, the different view we take of the function of the major premise, remains exactly where it was; and so far was I from thinking that my opinion had been fully "answered" and was "untenable," that in the same edition in which I canceled the note, I not only enforced the opinion by further arguments, but answered (though without naming him) those of the Archbishop.

For not having made this statement before, I do not think it needful to apologize. It would be attaching very great importance to one's smallest sayings, to think a formal retractation requisite every time that one falls into an error. Nor is Archbishop Whately's well-earned fame of so tender a quality as to require that in withdrawing a slight criticism on him I should have been bound to offer a public *amende* for having made it.

* But though it is a condition of the validity of every induction that there be uniformity in the course of nature, it is not a necessary condition that the uniformity should pervade all nature. It is enough that it pervades the particular class of phenomena to which the induction relates. An induction concerning the motions of the planets, or the properties of the magnet, would not be vitiated though we were to suppose that wind and weather are the sport of chance, provided it be assumed that astronomical and magnetic phenomena are under the dominion of general laws. Otherwise the early experience of mankind would have rested on a very weak foundation; for in the infancy of science it could not be known that *all* phenomena are regular in their course.

Neither would it be correct to say that every induction by which we infer any truth, implies the general fact of uniformity *as foreknown*, even in reference to the kind of phenomena concerned. It implies, *either* that this general fact is already known, *or* that we may now know it: as the conclusion, the Duke of Wellington is mortal, drawn from the instances A, B, and C, implies either that we have already concluded all men to be mortal, or that we are now entitled to do so from the same evidence. A vast amount of confusion and paralogism respecting the grounds of Induction would be dispelled by keeping in view these simple considerations.

are compelled by the constitution of our thinking faculty to assume as true. Having so recently, and at so much length, combated a similar doctrine as applied to the axioms of mathematics, by arguments which are in a great measure applicable to the present case, I shall defer the more particular discussion of this controverted point in regard to the fundamental axiom of induction, until a more advanced period of our inquiry.* At present it is of more importance to understand thoroughly the import of the axiom itself. For the proposition, that the course of nature is uniform, possesses rather the brevity suitable to popular, than the precision requisite in philosophical language: its terms require to be explained, and a stricter than their ordinary signification given to them, before the truth of the assertion can be admitted.

§ 2. Every person's consciousness assures him that he does not always expect uniformity in the course of events; he does not always believe that the unknown will be similar to the known, that the future will resemble the past. Nobody believes that the succession of rain and fine weather will be the same in every future year as in the present. Nobody expects to have the same dreams repeated every night. On the contrary, every body mentions it as something extraordinary, if the course of nature is constant, and resembles itself, in these particulars. To look for constancy where constancy is not to be expected, as for instance that a day which has once brought good fortune will always be a fortunate day, is justly accounted superstition.

The course of nature, in truth, is not only uniform, it is also infinitely various. Some phenomena are always seen to recur in the very same combinations in which we met with them at first; others seem altogether capricious; while some, which we had been accustomed to regard as bound down exclusively to a particular set of combinations, we unexpectedly find detached from some of the elements with which we had hitherto found them conjoined, and united to others of quite a contrary description. To an inhabitant of Central Africa, fifty years ago, no fact probably appeared to rest on more uniform experience than this, that all human beings are black. To Europeans, not many years ago, the proposition, All swans are white, appeared an equally unequivocal instance of uniformity in the course of nature. Further experience has proved to both that they were mistaken; but they had to wait fifty centuries for this experience. During that long time, mankind believed in a uniformity of the course of nature where no such uniformity really existed.

According to the notion which the ancients entertained of induction, the foregoing were cases of as legitimate inference as any inductions whatever. In these two instances, in which, the conclusion being false, the ground of inference must have been insufficient, there was, nevertheless, as much ground for it as this conception of induction admitted of. The induction of the ancients has been well described by Bacon, under the name of "Inductio per enumerationem simplicem, ubi non reperitur instantia contradictoria." It consists in ascribing the character of general truths to all propositions which are true in every instance that we happen to know of. This is the kind of induction which is natural to the mind when unaccustomed to scientific methods. The tendency, which some call an instinct, and which others account for by association, to infer the future from the past,

* Infra, chap. xxi.

the known from the unknown, is simply a habit of expecting that what has been found true once or several times, and never yet found false, will be found true again. Whether the instances are few or many, conclusive or inconclusive, does not much affect the matter: these are considerations which occur only on reflection; the unprompted tendency of the mind is to generalize its experience, provided this points all in one direction; provided no other experience of a conflicting character comes unsought. The notion of seeking it, of experimenting for it, of *interrogating* nature (to use Bacon's expression) is of much later growth. The observation of nature, by uncultivated intellects, is purely passive: they accept the facts which present themselves, without taking the trouble of searching for more: it is a superior mind only which asks itself what facts are needed to enable it to come to a safe conclusion, and then looks out for these.

But though we have always a propensity to generalize from unvarying experience, we are not always warranted in doing so. Before we can be at liberty to conclude that something is universally true because we have never known an instance to the contrary, we must have reason to believe that if there were in nature any instances to the contrary, we should have known of them. This assurance, in the great majority of cases, we can not have, or can have only in a very moderate degree. The possibility of having it, is the foundation on which we shall see hereafter that induction by simple enumeration may in some remarkable cases amount practically to proof.* No such assurance, however, can be had, on any of the ordinary subjects of scientific inquiry. Popular notions are usually founded on induction by simple enumeration; in science it carries us but a little way. We are forced to begin with it; we must often rely on it provisionally, in the absence of means of more searching investigation. But, for the accurate study of nature, we require a surer and a more potent instrument.

It was, above all, by pointing out the insufficiency of this rude and loose conception of Induction, that Bacon merited the title so generally awarded to him, of Founder of the Inductive Philosophy. The value of his own contributions to a more philosophical theory of the subject has certainly been exaggerated. Although (along with some fundamental errors) his writings contain, more or less fully developed, several of the most important principles of the Inductive Method, physical investigation has now far outgrown the Baconian conception of Induction. Moral and political inquiry, indeed, are as yet far behind that conception. The current and approved modes of reasoning on these subjects are still of the same vicious description against which Bacon protested; the method almost exclusively employed by those professing to treat such matters inductively, is the very *inductio per enumerationem simplicem* which he condemns; and the experience which we hear so confidently appealed to by all sects, parties, and interests, is still, in his own emphatic words, *mera palpatio*.

§ 3. In order to a better understanding of the problem which the logician must solve if he would establish a scientific theory of Induction, let us compare a few cases of incorrect inductions with others which are acknowledged to be legitimate. Some, we know, which were believed for centuries to be correct, were nevertheless incorrect. That all swans are white, can not have been a good induction, since the conclusion has turned out erroneous. The experience, however, on which the conclusion rested, was genu-

* Infra, chap. xxi., xxii.

ine. From the earliest records, the testimony of the inhabitants of the known world was unanimous on the point. The uniform experience, therefore, of the inhabitants of the known world, agreeing in a common result, without one known instance of deviation from that result, is not always sufficient to establish a general conclusion.

But let us now turn to an instance apparently not very dissimilar to this. Mankind were wrong, it seems, in concluding that all swans were white: are we also wrong, when we conclude that all men's heads grow above their shoulders, and never below, in spite of the conflicting testimony of the naturalist Pliny? As there were black swans, though civilized people had existed for three thousand years on the earth without meeting with them, may there not also be "men whose heads do grow beneath their shoulders," notwithstanding a rather less perfect unanimity of negative testimony from observers? Most persons would answer No; it was more credible that a bird should vary in its color, than that men should vary in the relative position of their principal organs. And there is no doubt that in so saying they would be right: but to say why they are right, would be impossible, without entering more deeply than is usually done, into the true theory of Induction.

Again, there are cases in which we reckon with the most unfailing confidence upon uniformity, and other cases in which we do not count upon it at all. In some we feel complete assurance that the future will resemble the past, the unknown be precisely similar to the known. In others, however invariable may be the result obtained from the instances which have been observed, we draw from them no more than a very feeble presumption that the like result will hold in all other cases. That a straight line is the shortest distance between two points, we do not doubt to be true even in the region of the fixed stars.* When a chemist announces the existence and properties of a newly-discovered substance, if we confide in his accuracy, we feel assured that the conclusions he has arrived at will hold universally, though the induction be founded but on a single instance. We do not withhold our assent, waiting for a repetition of the experiment; or if we do, it is from a doubt whether the one experiment was properly made, not whether if properly made it would be conclusive. Here, then, is a general law of nature, inferred without hesitation from a single instance; a universal proposition from a singular one. Now mark another case, and contrast it with this. Not all the instances which have been observed since the beginning of the world, in support of the general proposition that all crows are black, would be deemed a sufficient presumption of the truth of the proposition, to outweigh the testimony of one unexceptionable witness who should affirm that in some region of the earth not fully explored, he had caught and examined a crow, and had found it to be gray.

Why is a single instance, in some cases, sufficient for a complete induction, while in others, myriads of concurring instances, without a single exception known or presumed, go such a very little way toward establishing a universal proposition? Whoever can answer this question knows more of the philosophy of logic than the wisest of the ancients, and has solved the problem of induction.

* In strictness, wherever the present constitution of space exists; which we have ample reason to believe that it does in the region of the fixed stars.

CHAPTER IV.

OF LAWS OF NATURE.

§ 1. In the contemplation of that uniformity in the course of nature, which is assumed in every inference from experience, one of the first observations that present themselves is, that the uniformity in question is not properly uniformity, but uniformities. The general regularity results from the co-existence of partial regularities. The course of nature in general is constant, because the course of each of the various phenomena that compose it is so. A certain fact invariably occurs whenever certain circumstances are present, and does not occur when they are absent; the like is true of another fact; and so on. From these separate threads of connection between parts of the great whole which we term nature, a general tissue of connection unavoidably weaves itself, by which the whole is held together. If A is always accompanied by D, B by E, and C by F, it follows that A B is accompanied by D E, A C by D F, B C by E F, and finally A B C by D E F; and thus the general character of regularity is produced, which, along with and in the midst of infinite diversity, pervades all nature.

The first point, therefore, to be noted in regard to what is called the uniformity of the course of nature, is, that it is itself a complex fact, compounded of all the separate uniformities which exist in respect to single phenomena. These various uniformities, when ascertained by what is regarded as a sufficient induction, we call, in common parlance, Laws of Nature. Scientifically speaking, that title is employed in a more restricted sense, to designate the uniformities when reduced to their most simple expression. Thus in the illustration already employed, there were seven uniformities; all of which, if considered sufficiently certain, would, in the more lax application of the term, be called laws of nature. But of the seven, three alone are properly distinct and independent: these being presupposed, the others follow of course. The first three, therefore, according to the stricter acceptation, are called laws of nature; the remainder not; because they are in truth mere *cases* of the first three; virtually included in them; said, therefore, to *result* from them: whoever affirms those three has already affirmed all the rest.

To substitute real examples for symbolical ones, the following are three uniformities, or call them laws of nature: the law that air has weight, the law that pressure on a fluid is propagated equally in all directions, and the law that pressure in one direction, not opposed by equal pressure in the contrary direction, produces motion, which does not cease until equilibrium is restored. From these three uniformities we should be able to predict another uniformity, namely, the rise of the mercury in the Torricellian tube. This, in the stricter use of the phrase, is not a law of nature. It is the result of laws of nature. It is a *case* of each and every one of the three laws: and is the only occurrence by which they could all be fulfilled. If the mercury were not sustained in the barometer, and sustained at such a height that the column of mercury were equal in weight to a column of the atmosphere of the same diameter; here would be a case, either of the air not pressing upon the surface of the mercury with the force which is called its weight, or of the downward pressure on the mercury not being propagated equally in an upward direction, or of a body pressed in one direction and not in the direction opposite, either not moving in the direction in which it is pressed, or stopping before it had attained equilibrium. If we knew, therefore, the three simple laws, but had never tried the Torricellian experiment, we might *deduce* its result from those laws. The known weight of the air, combined with the position of the apparatus, would bring the mercury within the first of the three inductions; the first induction would bring it within the second, and the second within the third, in the manner which we characterized in treating of Ratiocination. We should thus come to know the more complex uniformity, independently of specific experience, through our knowledge of the simpler ones from which it results; though, for reasons which will appear hereafter, *verification* by specific experience would still be desirable, and might possibly be indispensable.

Complex uniformities which, like this, are mere cases of simpler ones, and have, therefore, been virtually affirmed in affirming those, may with propriety be called *laws*, but can scarcely, in the strictness of scientific speech, be termed Laws of Nature. It is the custom in science, wherever regularity of any kind can be traced, to call the general proposition which expresses the nature of that regularity, a law; as when, in mathematics, we speak of the law of decrease of the successive terms of a converging series. But the expression *law of nature* has generally been employed with a sort of tacit reference to the original sense of the word law, namely, the expression of the will of a superior. When, therefore, it appeared that any of the uniformities which were observed in nature, would result spontaneously from certain other uniformities, no separate act of creative will being supposed necessary for the production of the derivative uniformities, these have not usually been spoken of as laws of nature. According to one mode of expression, the question, What are the laws of nature? may be stated thus: What are the fewest and simplest assumptions, which being granted, the whole existing order of nature would result? Another mode of stating it would be thus: What are the fewest general propositions from which all the uniformities which exist in the universe might be deduced or inferred?

Every [illegible] advance which marks an epoch in the progress of science, has c[illegible] a step made toward the solution of this problem. Even a simple colligation of inductions already made, without any fresh extension of the inductive inference, is already an advance in that direction. When Kepler expressed the regularity which exists in the observed motions of the heavenly bodies, by the three general propositions called his laws, he, in so doing, pointed out three simple suppositions which, instead of a much greater number, would suffice to construct the whole scheme of the heavenly motions, so far as it was known up to that time. A similar and still greater step was made when these laws, which at first did not seem to be included in any more general truths, were discovered to be cases of the three laws of motion, as obtaining among bodies which mutually tend toward one another with a certain force, and have had a certain instantaneous impulse originally impressed upon them. After this great discovery, Kepler's three propositions, though still called laws, would hardly, by any person accustomed to use language with precision, be termed laws of nature: that phrase would be reserved for the simpler and more general laws into which Newton is said to have resolved them.

According to this language, every well-grounded inductive generalization is either a law of nature, or a result of laws of nature, capable, if those laws are known, of being predicted from them. And the problem of Inductive Logic may be summed up in two questions: how to ascertain the laws of nature; and how, after having ascertained them, to follow them into their results. On the other hand, we must not suffer ourselves to imagine that this mode of statement amounts to a real analysis, or to any thing but a mere verbal transformation of the problem; for the expression, Laws of Nature, *means* nothing but the uniformities which exist among natural phenomena) or, in other words, the results of induction), when reduced to their simplest expression. It is, however, something to have advanced so far, as to see that the study of nature is the study of laws, not *a* law; of uniformities, in the plural number: that the different natural phenomena have their separate rules or modes of taking place, which, though much intermixed and entangled with one another, may, to a certain extent, be studied apart: that (to resume our former metaphor) the regularity which exists in nature is a web composed of distinct threads, and only to be understood by tracing each of the threads separately; for which purpose it is often necessary to unravel some portion of the web, and exhibit the fibres apart. The rules of experimental inquiry are the contrivances for unraveling the web.

§ 2. In thus attempting to ascertain the general order of nature by ascertaining the particular order of the occurrence of each one of the phenomena of nature, the most scientific proceeding can be no more than an improved form of that which was primitively pursued by the human understanding, while undirected by science. When mankind first formed the idea of studying phenomena according to a stricter and surer method than that which they had in the first instance spontaneously adopted, they did not, conformably to the well-meant but impracticable precept of Descartes, set out from the supposition that nothing had been already ascertained. Many of the uniformities existing among phenomena are so constant, and so open to observation, as to force themselves upon involuntary recognition. Some facts are so perpetually and familiarly accompanied by certain others, that mankind learned, as children learn, to expect the one where they found the other, long before they knew how to put their expectation into words by asserting, in a proposition, the existence of a connection between those phenomena. No science was needed to teach that food nourishes, that water drowns, or quenches thirst, that the sun gives light and heat, that bodies fall to the ground. The first scientific inquirers assumed these and the like as known truths, and set out from them to discover others which were unknown: nor were they wrong in so doing, subject, however, as they afterward began to see, to an ulterior revision of these spontaneous generalizations themselves, when the progress of knowledge pointed out limits to them, or showed their truth to be contingent on some circumstance not originally attended to. It will appear, I think, from the subsequent part of our inquiry, that there is no logical fallacy in this mode of proceeding; but we may see already that any other mode is rigorously impracticable: since it is impossible to frame any scientific method of induction, or test of the correctness of inductions, unless on the hypothesis that some inductions deserving of reliance have been already made.

Let us revert, for instance, to one of our former illustrations, and consider why it is that, with exactly the same amount of evidence, both negative and positive, we did not reject the assertion that there are black swans, while we should refuse credence to any testimony which asserted that there were men wearing their heads underneath their shoulders. The first assertion was more credible than the latter. But why more credible? So long as neither phenomenon had been actually witnessed, what reason was there for finding the one harder to be believed than the other? Apparently because there is less constancy in the colors of animals, than in the general structure of their anatomy. But how do we know this? Doubtless, from experience. It appears, then, that we need experience to inform us, in what degree, and in what cases, or sorts of cases, experience is to be relied on. Experience must be consulted in order to learn from it under what circumstances arguments from it will be valid. We have no ulterior test to which we subject experience in general; but we make experience its own test. Experience testifies, that among the uniformities which it exhibits or seems to exhibit, some are more to be relied on than others; and uniformity, therefore, may be presumed, from any given number of instances, with a greater degree of assurance, in proportion as the case belongs to a class in which the uniformities have hitherto been found more uniform.

This mode of correcting one generalization by means of another, a narrower generalization by a wider, which common sense suggests and adopts in practice, is the real type of scientific Induction. All that art can do is but to give accuracy and precision to this process, and adapt it to all varieties of cases, without any essential alteration in its principle.

There are of course no means of applying such a test as that above described, unless we already possess a general knowledge of the prevalent character of the uniformities existing throughout nature. The indispensable foundation, therefore, of a scientific formula of induction, must be a survey of the inductions to which mankind have been conducted in unscientific practice; with the special purpose of ascertaining what kinds of uniformities have been found perfectly invariable, pervading all nature, and what are those which have been found to vary with difference of time, place, or other changeable circumstances.

§ 3. The necessity of such a survey is confirmed by the consideration, that the stronger inductions are the touch-stone to which we always endeavor to bring the weaker. If we find any means of deducing one of the less strong inductions from stronger ones, it acquires, at once, all the strength of those from which it is deduced; and even adds to that strength; since the independent experience on which the weaker induction previously rested, becomes additional evidence of the truth of the better established law in which it is now found to be included. We may have inferred, from historical evidence, that the uncontrolled power of a monarch, of an aristocracy, or of the majority, will often be abused: but we are entitled to rely on this generalization with much greater assurance when it is shown to be a corollary from still better established facts; the very low degree of elevation of character ever yet attained by the average of mankind, and the little efficacy, for the most part, of the modes of education hitherto practiced, in maintaining the predominance of reason and conscience over the selfish propensities. It is at the same time obvious that even these more general facts derive an accession of evidence from the testimony which history bears to the effects of despotism. The strong induction becomes still stronger when a weaker one has been bound up with it.

On the other hand, if an induction conflicts with stronger inductions, or with conclusions capable of being correctly deduced from them, then, unless on reconsideration it should appear that some of the stronger inductions have been expressed with greater universality than their evidence warrants, the weaker one must give way. The opinion so long prevalent that a comet, or any other unusual appearance in the heavenly regions, was the precursor of calamities to mankind, or to those at least who witnessed it; the belief in the veracity of the oracles of Delphi or Dodona; the reliance on astrology, or on the weather-prophecies in almanacs, were doubtless inductions supposed to be grounded on experience:* and faith in such delusions seems quite capable of holding out against a great multitude of failures, provided it be nourished by a reasonable number of casual coincidences between the prediction and the event. What has really put an end to these insufficient inductions, is their inconsistency with the stronger inductions subsequently obtained by scientific inquiry, respecting the causes on which terrestrial events really depend; and where those scientific truths have not yet penetrated, the same or similar delusions still prevail.

It may be affirmed as a general principle, that all inductions, whether strong or weak, which can be connected by ratiocination, are confirmatory of one another; while any which lead deductively to consequences that are incompatible, become mutually each other's test, showing that one or other must be given up, or at least more guardedly expressed. In the case of inductions which confirm each other, the one which becomes a conclusion from ratiocination rises to at least the level of certainty of the weakest of those from which it is deduced; while in general all are more or less increased in certainty. Thus the Torricellian experiment, though a mere case of three more general laws, not only strengthened greatly the evidence on which those laws rested, but converted one of them (the weight of the atmosphere) from a still doubtful generalization into a completely established doctrine.

If, then, a survey of the uniformities which have been ascertained to exist in nature, should point out some which, as far as any human purpose requires certainty, may be considered quite certain and quite universal; then by means of these uniformities we may be able to raise multitudes of other inductions to the same point in the scale. For if we can show, with re-

* Dr. Whewell (*Phil. of Discov.*, p. 246) will not allow these and similar erroneous judgments to be called inductions; inasmuch as such superstitious fancies "were not collected from the facts by seeking a law of their occurrence, but were suggested by an imagination of the anger of superior powers, shown by such deviations from the ordinary course of nature." I conceive the question to be, not in what manner these notions were at first suggested, but by what evidence they have, from time to time, been supposed to be substantiated. If the believers in these erroneous opinions had been put on their defense, they would have referred to experience: to the comet which preceded the assassination of Julius Cæsar, or to oracles and other prophecies known to have been fulfilled. It is by such appeals to facts that all analogous superstitions, even in our day, attempt to justify themselves; the supposed evidence of experience is necessary to their hold on the mind. I quite admit that the influence of such coincidences would not be what it is, if strength were not lent to it by an antecedent presumption; but this is not peculiar to such cases; preconceived notions of probability form part of the explanation of many other cases of belief on insufficient evidence. The *a priori* prejudice does not prevent the erroneous opinion from being sincerely regarded as a legitimate conclusion from experience; though it improperly predisposes the mind to that interpretation of experience.

Thus much in defense of the sort of examples objected to. But it would be easy to produce instances, equally adapted to the purpose, and in which no antecedent prejudice is at all concerned. "For many ages," says Archbishop Whately, "all farmers and gardeners were firmly convinced—and convinced of their knowing it by experience—that the crops would never turn out good unless the seed were sown during the increase of the moon." This was induction, but bad induction; just as a vicious syllogism is reasoning, but bad reasoning.

spect to any inductive inference, that either it must be true, or one of these certain and universal inductions must admit of an exception; the former generalization will attain the same certainty, and indefeasibleness within the bounds assigned to it, which are the attributes of the latter. It will be proved to be a law; and if not a result of other and simpler laws, it will be a law of nature.

There are such certain and universal inductions; and it is because there are such, that a Logic of Induction is possible.

CHAPTER V.

OF THE LAW OF UNIVERSAL CAUSATION.

§ 1. The phenomena of nature exist in two distinct relations to one another; that of simultaneity, and that of succession. Every phenomenon is related, in a uniform manner, to some phenomena that co-exist with it, and to some that have preceded and will follow it.

Of the uniformities which exist among synchronous phenomena, the most important, on every account, are the laws of number; and next to them those of space, or, in other words, of extension and figure. The laws of number are common to synchronous and successive phenomena. That two and two make four, is equally true whether the second two follow the first two or accompany them. It is as true of days and years as of feet and inches. The laws of extension and figure (in other words, the theorems of geometry, from its lowest to its highest branches) are, on the contrary, laws of simultaneous phenomena only. The various parts of space, and of the objects which are said to fill space, co-exist; and the unvarying laws which are the subject of the science of geometry, are an expression of the mode of their co-existence.

This is a class of laws, or in other words, of uniformities, for the comprehension and proof of which it is not necessary to suppose any lapse of time, any variety of facts or events succeeding one another. The propositions of geometry are independent of the succession of events. All things which possess extension, or, in other words, which fill space, are subject to geometrical laws. Possessing extension, they possess figure; possessing figure, they must possess some figure in particular, and have all the properties which geometry assigns to that figure. If one body be a sphere and another a cylinder, of equal height and diameter, the one will be exactly two-thirds of the other, let the nature and quality of the material be what it will. Again, each body, and each point of a body, must occupy some place or position among other bodies; and the position of two bodies relatively to each other, of whatever nature the bodies be, may be unerringly inferred from the position of each of them relatively to any third body.

In the laws of number, then, and in those of space, we recognize in the most unqualified manner, the rigorous universality of which we are in quest. Those laws have been in all ages the type of certainty, the standard of comparison for all inferior degrees of evidence. Their invariability is so perfect, that it renders us unable even to conceive any exception to them; and philosophers have been led, though (as I have endeavored to show) erroneously, to consider their evidence as lying not in experience, but in the original constitution of the intellect. If, therefore, from the laws of space and number, we were able to deduce uniformities of any other description, this would be conclusive evidence to us that those other uniformities possessed the same rigorous certainty. But this we can not do. From laws of space and number alone, nothing can be deduced but laws of space and number.

Of all truths relating to phenomena, the most valuable to us are those which relate to the order of their succession. On a knowledge of these is founded every reasonable anticipation of future facts, and whatever power we possess of influencing those facts to our advantage. Even the laws of geometry are chiefly of practical importance to us as being a portion of the premises from which the order of the succession of phenomena may be inferred. Inasmuch as the motion of bodies, the action of forces, and the propagation of influences of all sorts, take place in certain lines and over definite spaces, the properties of those lines and spaces are an important part of the laws to which those phenomena are themselves subject. Again, motions, forces, or other influences, and times, are numerable quantities; and the properties of number are applicable to them as to all other things. But though the laws of number and space are important elements in the ascertainment of uniformities of succession, they can do nothing toward it when taken by themselves. They can only be made instrumental to that purpose when we combine with them additional premises, expressive of uniformities of succession already known. By taking, for instance, as premises these propositions, that bodies acted upon by an instantaneous force move with uniform velocity in straight lines; that bodies acted upon by a continuous force move with accelerated velocity in straight lines; and that bodies acted upon by two forces in different directions move in the diagonal of a parallelogram, whose sides represent the direction and quantity of those forces; we may by combining these truths with propositions relating to the properties of straight lines and of parallelograms (as that a triangle is half a parallelogram of the same base and altitude), deduce another important uniformity of succession, viz., that a body moving round a centre of force describes areas proportional to the times. But unless there had been laws of succession in our premises, there could have been no truths of succession in our conclusions. A similar remark might be extended to every other class of phenomena really peculiar; and, had it been attended to, would have prevented many chimerical attempts at demonstrations of the indemonstrable, and explanations which do not explain.

It is not, therefore, enough for us that the laws of space, which are only laws of simultaneous phenomenon, and the laws of number, which though true of successive phenomena do not relate to their succession, possess the rigorous certainty and universality of which we are in search. We must endeavor to find some law of succession which has those same attributes, and is therefore fit to be made the foundation of processes for discovering, and of a test for verifying, all other uniformities of succession. This fundamental law must resemble the truths of geometry in their most remarkable peculiarity, that of never being, in any instance whatever, defeated or suspended by any change of circumstances.

Now among all those uniformities in the succession of phenomena, which common observation is sufficient to bring to light, there are very few which have any, even apparent, pretension to this rigorous indefeasibility: and of those few, one only has been found capable of completely sustaining it. In that one, however, we recognize a law which is universal also in another sense; it is co-extensive with the entire field of successive phenomena, all instances whatever of succession being examples of it. This law is the

Law of Causation. The truth that every fact which has a beginning has a cause, is co-extensive with human experience.

This generalization may appear to some minds not to amount to much, since after all it asserts only this: "it is a law, that every event depends on some law:" "it is a law, that there is a law for every thing." We must not, however, conclude that the generality of the principle is merely verbal; it will be found on inspection to be no vague or unmeaning assertion, but a most important and really fundamental truth.

§ 2. The notion of Cause being the root of the whole theory of Induction, it is indispensable that this idea should, at the very outset of our inquiry, be, with the utmost practicable degree of precision, fixed and determined. If, indeed, it were necessary for the purpose of inductive logic that the strife should be quelled, which has so long raged among the different schools of metaphysicians, respecting the origin and analysis of our idea of causation; the promulgation, or at least the general reception, of a true theory of induction, might be considered desperate for a long time to come. But the science of the Investigation of Truth by means of Evidence, is happily independent of many of the controversies which perplex the science of the ultimate constitution of the human mind, and is under no necessity of pushing the analysis of mental phenomenon to that extreme limit which alone ought to satisfy a metaphysician.

I premise, then, that when in the course of this inquiry I speak of the cause of any phenomenon, I do not mean a cause which is not itself a phenomenon; I make no research into the ultimate or ontological cause of any thing. To adopt a distinction familiar in the writings of the Scotch metaphysicians, and especially of Reid, the causes with which I concern myself are not *efficient*, but *physical* causes. They are causes in that sense alone, in which one physical fact is said to be the cause of another. Of the efficient causes of phenomena, or whether any such causes exist at all, I am not called upon to give an opinion. The notion of causation is deemed, by the schools of metaphysics most in vogue at the present moment, to imply a mysterious and most powerful tie, such as can not, or at least does not, exist between any physical fact and that other physical fact on which it is invariably consequent, and which is popularly termed its cause: and thence is deduced the supposed necessity of ascending higher, into the essences and inherent constitution of things, to find the true cause, the cause which is not only followed by, but actually produces, the effect. No such necessity exists for the purposes of the present inquiry, nor will any such doctrine be found in the following pages. The only notion of a cause, which the theory of induction requires, is such a notion as can be gained from experience. The Law of Causation, the recognition of which is the main pillar of inductive science, is but the familiar truth, that invariability of succession is found by observation to obtain between every fact in nature and some other fact which has preceded it; independently of all considerations respecting the ultimate mode of production of phenomena, and of every other question regarding the nature of "Things in themselves."

Between the phenomena, then, which exist at any instant, and the phenomena which exist at the succeeding instant, there is an invariable order of succession; and, as we said in speaking of the general uniformity of the course of nature, this web is composed of separate fibres; this collective order is made up of particular sequences, obtaining invariably among the separate parts. To certain facts, certain facts always do, and, as we believe, will continue to, succeed. The invariable antecedent is termed the cause; the invariable consequent, the effect. And the universality of the law of causation consists in this, that every consequent is connected in this manner with some particular antecedent, or set of antecedents. Let the fact be what it may, if it has begun to exist, it was preceded by some fact or facts, with which it is invariably connected. For every event there exists some combination of objects or events, some given concurrence of circumstances, positive and negative, the occurrence of which is always followed by that phenomenon. We may not have found out what this concurrence of circumstances may be; but we never doubt that there is such a one, and that it never occurs without having the phenomenon in question as its effect or consequence. On the universality of this truth depends the possibility of reducing the inductive process to rules. The undoubted assurance we have that there is a law to be found if we only knew how to find it, will be seen presently to be the source from which the canons of the Inductive Logic derive their validity.

§ 3. It is seldom, if ever, between a consequent and a single antecedent, that this invariable sequence subsists. It is usually between a consequent and the sum of several antecedents; the concurrence of all of them being requisite to produce, that is, to be certain of being followed by, the consequent. In such cases it is very common to single out one only of the antecedents under the denomination of Cause, calling the others merely Conditions. Thus, if a person eats of a particular dish, and dies in consequence, that is, would not have died if he had not eaten of it, people would be apt to say that eating of that dish was the cause of his death. There needs not, however, be any invariable connection between eating of the dish and death; but there certainly is, among the circumstances which took place, some combination or other on which death is invariably consequent: as, for instance, the act of eating of the dish, combined with a particular bodily constitution, a particular state of present health, and perhaps even a certain state of the atmosphere; the whole of which circumstances perhaps constituted in this particular case the *conditions* of the phenomenon, or, in other words, the set of antecedents which determined it, and but for which it would not have happened. The real Cause, is the whole of these antecedents; and we have, philosophically speaking, no right to give the name of cause to one of them, exclusively of the others. What, in the case we have supposed, disguises the incorrectness of the expression, is this: that the various conditions, except the single one of eating the food, were not *events* (that is, instantaneous changes, or successions of instantaneous changes) but *states*, possessing more or less of permanency; and might therefore have preceded the effect by an indefinite length of duration, for want of the event which was requisite to complete the required concurrence of conditions: while as soon as that event, eating the food, occurs, no other cause is waited for, but the effect begins immediately to take place: and hence the appearance is presented of a more immediate and close connection between the effect and that one antecedent, than between the effect and the remaining conditions. But though we may think proper to give the name of cause to that one condition, the fulfillment of which completes the tale, and brings about the effect without further delay; this condition has really no closer relation to the effect than any of the other conditions has. All the conditions were equally indispensable to the production of the consequent; and the statement of the cause is incomplete, unless in some shape or other we introduce them all. A man takes mercury, goes out-of-doors, and catches cold. We say, perhaps, that the cause of his taking cold was exposure to the air. It is clear, however, that his having taken mercury may have been a necessary condition of his catching cold; and though it might consist with usage to say that the cause of his attack was exposure to the air, to be accurate we ought to say that the cause was exposure to the air while under the effect of mercury.

If we do not, when aiming at accuracy, enumerate all the conditions, it is only because some of them will in most cases be understood without being expressed, or because for the purpose in view they may without detriment be overlooked. For example, when we say, the cause of a man's death was that his foot slipped in climbing a ladder, we omit as a thing unnecessary to be stated the circumstance of his weight, though quite as indispensable a condition of the effect which took place. When we say that the assent of the crown to a bill makes it law, we mean that the assent, being never given until all the other conditions are fulfilled, makes up the sum of the conditions, though no one now regards it as the principal one. When the decision of a legislative assembly has been determined by the casting vote of the chairman, we sometimes say that this one person was the cause of all the effects which resulted from the enactment. Yet we do not really suppose that his single vote contributed more to the result than that of any other person who voted in the affirmative; but, for the purpose we have in view, which is to insist on his individual responsibility, the part which any other person had in the transaction is not material.

In all these instances the fact which was dignified with the name of cause, was the one condition which came last into existence. But it must not be supposed that in the employment of the term this or any other rule is always adhered to. Nothing can better show the absence of any scientific ground for the distinction between the cause of a phenomenon and its conditions, than the capricious manner in which we select from among the conditions that which we choose to denominate the cause. However numerous the conditions may be, there is hardly any of them which may not, according to the purpose of our immediate discourse, obtain that nominal pre-eminence. This will be seen by analyzing the conditions of some one familiar phenomenon. For example, a stone thrown into water falls to the bottom. What are the conditions of this event? In the first place there must be a stone, and water, and the stone must be thrown into the water; but these suppositions forming part of the enunciation of the phenomenon itself, to include them also among the conditions would be a vicious tautology; and this class of conditions, therefore, have never received the name of cause from any but the Aristotelians, by whom they were called the *material* cause, *causa materialis*. The next condition is, there must be an earth: and accordingly it is often said, that the fall of a stone is caused by the earth; or by a power or property of the earth, or a force exerted by the earth, all of which are merely roundabout ways of saying that it is caused by the earth; or, lastly, the earth's attraction; which also is only a technical mode of saying that the earth causes the motion, with the additional particularity that the motion is toward the earth, which is not a character of the cause, but of the effect. Let us now pass to another condition. It is not enough that the earth should exist; the body must be within that distance from it, in which the earth's attraction preponderates over that of any other body. Accordingly we may say, and the expression would be confessedly correct, that the cause of the stone's falling is its being *within the sphere* of the earth's attraction. We proceed to a further condition. The stone is immersed in water: it is therefore a condition of its reaching the ground, that its specific gravity exceed that of the surrounding fluid, or in other words that it surpass in weight an equal volume of water. Accordingly any one would be acknowledged to speak correctly who said, that the cause of the stone's going to the bottom is its exceeding in specific gravity the fluid in which it is immersed.

Thus we see that each and every condition of the phenomenon may be taken in its turn, and, with equal propriety in common parlance, but with equal impropriety in scientific discourse, may be spoken of as if it were the entire cause. And in practice, that particular condition is usually styled the cause, whose share in the matter is superficially the most conspicuous, or whose requisiteness to the production of the effect we happen to be insisting on at the moment. So great is the force of this last consideration, that it sometimes induces us to give the name of cause even to one of the negative conditions. We say, for example, The army was surprised because the sentinel was off his post. But since the sentinel's absence was not what created the enemy, or put the soldiers asleep, how did it cause them to be surprised? All that is really meant is, that the event would not have happened if he had been at his duty. His being off his post was no producing cause, but the mere absence of a preventing cause: it was simply equivalent to his non-existence. From nothing, from a mere negation, no consequences can proceed. All effects are connected, by the law of causation, with some set of *positive* conditions; negative ones, it is true, being almost always required in addition. In other words, every fact or phenomenon which has a beginning, invariably arises when some certain combination of positive facts exists, provided certain other positive facts do not exist.

There is, no doubt, a tendency (which our first example, that of death from taking a particular food, sufficiently illustrates) to associate the idea of causation with the proximate antecedent *event*, rather than with any of the antecedent *states*, or permanent facts, which may happen also to be conditions of the phenomenon; the reason being that the event not only exists, but begins to exist immediately previous; while the other conditions may have pre-existed for an indefinite time. And this tendency shows itself very visibly in the different logical fictions which are resorted to, even by men of science, to avoid the necessity of giving the name of cause to any thing which had existed for an indeterminate length of time before the effect. Thus, rather than say that the earth causes the fall of bodies, they ascribe it to a *force* exerted by the earth, or an *attraction* by the earth, abstractions which they can represent to themselves as exhausted by each effort, and therefore constituting at each successive instant a fresh fact, simultaneous with, or only immediately preceding, the effect. Inasmuch as the coming of the circumstance which completes the assemblage of conditions, is a change or event, it thence happens that an event is always the antecedent in closest apparent proximity to the consequent: and this may account for the illusion which disposes us to look upon the proximate event as standing more peculiarly in the position of a cause than any of the antecedent states. But even this peculiarity, of being in closer proximity to the effect than any other of its conditions, is, as we have already seen, far from being necessary to the common notion of a cause; with which notion, on the contrary, any one of the conditions, either positive or negative, is found, on occasion, completely to accord.*

* The assertion, that any and every one of the conditions of a phenomenon may be and is, on some occasions and for some purposes, spoken of as the cause, has been disputed by an intelligent reviewer of this work in the *Prospective Review* (the predecessor of the justly esteemed *National Review*), who maintains that "we always apply the word cause rather to that element in the antecedents which exercises *force*, and which would *tend* at all times to produce the same or a similar effect to that which, under certain conditions, it would actually produce." And he says, that "every one would feel" the expression, that the cause of a surprise was the sentinel's being off his post, to be incorrect; but that the "allurement or force which *drew* him off his post, might be so called, because in doing so it removed a resisting power which would have prevented the surprise." I can not think that it would be wrong to say, that the event took place because the sentinel was absent, and yet right to say that it took place because he was bribed to be absent. Since the only direct effect of the bribe was his absence, the bribe could be called the remote cause of the surprise, only on the supposition that the absence was the proximate cause; nor does it seem to me that any one (who had not a theory to support) would use the one expression and reject the other.

The reviewer observes, that when a person dies of poison, his possession of bodily organs is a necessary condition, but that no one would ever speak of it as the cause. I admit the fact; but I believe the reason to be, that the occasion could never arise for so speaking of it; for when in the inaccuracy of common discourse we are led to speak of some one condition of a phenomenon as its cause, the condition so spoken of is always one which it is at least possible that the hearer may require to be informed of. The possession of bodily organs is a known condition, and to give that as the answer, when asked the cause of a person's death, would not supply the information sought. Once conceive that a doubt could exist as to his having bodily organs, or that he were to be compared with some being who had them not, and cases may be imagined in which it might be said that his possession of them was the cause of his death. If Faust and Mephistopheles together took poison, it might be said that Faust died because he was a human being, and had a body, while Mephistopheles survived because he was a spirit.

It is for the same reason that no one (as the reviewer remarks) "calls the cause of a leap, the muscles or sinews of the body, though they are necessary conditions; nor the cause of a self-sacrifice, the knowledge which was necessary for it; nor the cause of writing a book, that a man has time for it, which is a necessary condition." These conditions (besides that they are antecedent *states*, and not proximate antecedent *events*, and are therefore never the conditions in closest apparent proximity to the effect) are all of them so obviously implied, that it is hardly possible there should exist that necessity for insisting on them, which alone gives occasion for speaking of a single condition as if it were the cause. Wherever this necessity exists in regard to some one condition, and does not exist in regard to any other, I conceive that it is consistent with usage, when scientific accuracy is not aimed at, to apply the name cause to that one condition. If the only condition which can be supposed to be unknown is a negative condition, the negative condition may be spoken of as the cause. It might be said that a person died for want of medical advice: though this would not be likely to be said, unless the person was already understood to be ill, and in order to indicate that this negative circumstance was what made the illness fatal, and not the weakness of his constitution, or the original virulence of the disease. It might be said that a person was drowned because he could not swim; the positive condition, namely, that he fell into the water, being already implied in the word drowned. And here let me remark, that his falling into the water is in this case the only positive condition: all the conditions not expressly or virtually included in this (as that he could not swim, that nobody helped him, and so forth) are negative. Yet, if it were simply said that the cause of a man's death was falling into the water, there would be quite as great a sense of impropriety in the expression, as there would be if it were said that the cause was his inability to swim; because, though the one condition is positive and the other negative, it would be felt that neither of them was sufficient, without the other, to produce death.

With regard to the assertion that nothing is termed the cause, except the element which exerts active force; I waive the question as to the meaning of active force, and accepting the phrase in its popular sense, I revert to a former example, and I ask, would it be more agreeable to custom to say that a man fell because his foot slipped in climbing a ladder, or that he fell because of his weight? for his weight, and not the motion of his foot, was the active force which determined his fall. If a person walking out in a frosty day, stumbled and fell, it might be said that he stumbled because the ground was slippery, or because he was not sufficiently careful: but few people, I suppose, would say, that he stumbled because he walked. Yet the only active force concerned was that which he exerted in walking: the others were

The cause, then, philosophically speaking, is the sum total of the conditions, positive and negative taken together; the whole of the contingencies of every description, which being realized, the consequent invariably follows. The negative conditions, however, of any phenomenon, a special enumeration of which would generally be very prolix, may be all summed up under one head, namely, the absence of preventing or counteracting causes. The convenience of this mode of expression is mainly grounded on the fact, that the effects of any cause in counteracting another cause may in most cases be, with strict scientific exactness, regarded as a mere extension of its own proper and separate effects. If gravity retards the upward motion of a projectile, and deflects it into a parabolic trajectory, it produces, in so doing, the very same kind of effect, and even (as mathematicians know) the same quantity of effect, as it does in its ordinary operation of causing the fall of bodies when simply deprived of their support. If an alkaline solution mixed with an acid destroys its sourness, and prevents it from reddening vegetable blues, it is because the specific effect of the alkali is to combine with the acid, and form a compound with totally different qualities. This property, which causes of all descriptions possess, of preventing the effects of other causes by virtue (for the most part) of the same laws according to which they produce their own,* enables us, by establishing the general axiom that all causes are liable to be counteracted in their effects by one another, to dispense with the consideration of negative conditions entirely, and limit the notion of cause to the assemblage of the positive conditions of the phenomenon: one negative condition invariably understood, and the same in all instances (namely, the absence of counteracting causes) being sufficient, along with the sum of the positive conditions, to make up the whole set of circumstances on which the phenomenon is dependent.

§ 4. Among the positive conditions, as we have seen that there are some

mere negative conditions; but they happened to be the only ones which there could be any necessity to state; for he walked, most likely, in exactly his usual manner, and the negative conditions made all the difference. Again, if a person were asked why the army of Xerxes defeated that of Leonidas, he would probably say, because they were a thousand times the number; but I do not think he would say, it was because they fought, though that was the element of active force. To borrow another example, used by Mr. Grove and by Mr. Baden Powell, the opening of flood-gates is said to be the cause of the flow of water; yet the active force is exerted by the water itself, and opening the flood-gates merely supplies a negative condition. The reviewer adds, "There are some conditions absolutely passive, and yet absolutely necessary to physical phenomena, viz., the relations of space and time; and to these no one ever applies the word cause without being immediately arrested by those who hear him." Even from this statement I am compelled to dissent. Few persons would feel it incongruous to say (for example) that a secret became known because it was spoken of when A. B. was within hearing; which is a condition of space: or that the cause why one of two particular trees is taller than the other, is that it has been longer planted; which is a condition of time.

* There are a few exceptions; for there are some properties of objects which seem to be purely preventive; as the property of opaque bodies, by which they intercept the passage of light. This, as far as we are able to understand it, appears an instance not of one cause counteracting another by the same law whereby it produces its own effects, but of an agency which manifests itself in no other way than in defeating the effects of another agency. If we knew on what other relations to light, or on what peculiarities of structure, opacity depends, we might find that this is only an apparent, not a real, exception to the general proposition in the text. In any case it needs not affect the practical application. The formula which includes all the negative conditions of an effect in the single one of the absence of counteracting causes, is not violated by such cases as this; though, if all counteracting agencies were of this description, there would be no purpose served by employing the formula.

to which, in common parlance, the term cause is more readily and frequently awarded, so there are others to which it is, in ordinary circumstances, refused. In most cases of causation a distinction is commonly drawn between something which acts, and some other thing which is acted upon; between an *agent* and a *patient.* Both of these, it would be universally allowed, are conditions of the phenomenon; but it would be thought absurd to call the latter the cause, that title being reserved for the former. The distinction, however, vanishes on examination, or rather is found to be only verbal; arising from an incident of mere expression, namely, that the object said to be acted upon, and which is considered as the scene in which the effect takes place, is commonly included in the phrase by which the effect is spoken of, so that if it were also reckoned as part of the cause, the seeming incongruity would arise of its being supposed to cause itself. In the instance which we have already had, of falling bodies, the question was thus put: What is the cause which makes a stone fall? and if the answer had been "the stone itself," the expression would have been in apparent contradiction to the meaning of the word cause. The stone, therefore, is conceived as the patient, and the earth (or, according to the common and most unphilosophical practice, an occult quality of the earth) is represented as the agent or cause. But that there is nothing fundamental in the distinction may be seen from this, that it is quite possible to conceive the stone as causing its own fall, provided the language employed be such as to save the mere verbal incongruity. We might say that the stone moves toward the earth by the properties of the matter composing it; and according to this mode of presenting the phenomenon, the stone itself might without impropriety be called the agent; though, to save the established doctrine of the inactivity of matter, men usually prefer here also to ascribe the effect to an occult quality, and say that the cause is not the stone itself, but the *weight* or *gravitation* of the stone.

Those who have contended for a radical distinction between agent and patient, have generally conceived the agent as that which causes some state of, or some change in the state of, another object which is called the patient. But a little reflection will show that the license we assume of speaking of phenomena as *states* of the various objects which take part in them (an artifice of which so much use has been made by some philosophers, Brown in particular, for the apparent explanation of phenomena), is simply a sort of logical fiction, useful sometimes as one among several modes of expression, but which should never be supposed to be the enunciation of a scientific truth. Even those attributes of an object which might seem with greatest propriety to be called states of the object itself, its sensible qualities, its color, hardness, shape, and the like, are in reality (as no one has pointed out more clearly than Brown himself) phenomena of causation, in which the substance is distinctly the agent, or producing cause, the patient being our own organs, and those of other sentient beings. What we call states of objects, are always sequences into which the objects enter, generally as antecedents or causes; and things are never more active than in the production of those phenomena in which they are said to be acted upon. Thus, in the example of a stone falling to the earth, according to the theory of gravitation the stone is as much an agent as the earth, which not only attracts, but is itself attracted by, the stone. In the case of a sensation produced in our organs, the laws of our organization, and even those of our minds, are as directly operative in determining the effect produced, as the laws of the outward object. Though we call prussic acid the agent of a person's death, the whole of the vital and organic properties of the patient are as actively instrumental as the poison, in the chain of effects which so rapidly terminates his sentient existence. In the process of education, we may call the teacher the agent, and the scholar only the material acted upon; yet in truth all the facts which pre-existed in the scholar's mind exert either co-operating or counteracting agencies in relation to the teacher's efforts. It is not light alone which is the agent in vision, but light coupled with the active properties of the eye and brain, and with those of the visible object. The distinction between agent and patient is merely verbal: patients are always agents; in a great proportion, indeed, of all natural phenomena, they are so to such a degree as to react forcibly on the causes which acted upon them: and even when this is not the case, they contribute, in the same manner as any of the other conditions, to the production of the effect of which they are vulgarly treated as the mere theatre. All the positive conditions of a phenomenon are alike agents, alike active; and in any expression of the cause which professes to be complete, none of them can with reason be excluded, except such as have already been implied in the words used for describing the effect; nor by including even these would there be incurred any but a merely verbal impropriety.

§ 5. There is a case of causation which calls for separate notice, as it possesses a peculiar feature, and presents a greater degree of complexity than the common case. It often happens that the effect, or one of the effects, of a cause, is, not to produce of itself a certain phenomenon, but to fit something else for producing it. In other words, there is a case of causation in which the effect is to invest an object with a certain property. When sulphur, charcoal, and nitre are put together in certain proportions and in a certain manner, the effect is, not an explosion, but that the mixture acquires a property by which, in given circumstances, it will explode. The various causes, natural and artificial, which educate the human body or the human mind, have for their principal effect, not to make the body or mind immediately do any thing, but to endow it with certain properties—in other words, to give assurance that in given circumstances certain results will take place in it, or as consequences of it. Physiological agencies often have for the chief part of their operation to *predispose* the constitution to some mode of action. To take a simpler instance than all these: putting a coat of white paint upon a wall does not merely produce in those who see it done, the sensation of white; it confers on the wall the permanent property of giving that kind of sensation. Regarded in reference to the sensation, the putting on of the paint is a condition of a condition; it is a condition of the wall's causing that particular fact. The wall may have been painted years ago, but it has acquired a property which has lasted till now, and will last longer; the antecedent condition necessary to enable the wall to become in its turn a condition, has been fulfilled once for all. In a case like this, where the immediate consequent in the sequence is a property produced in an object, no one now supposes the property to be a substantive entity "inherent" in the object. What has been produced is what, in other language, may be called a state of preparation in an object for producing an effect. The ingredients of the gunpowder have been brought into a state of preparation for exploding as soon as the other conditions of an explosion shall have occurred. In the case of the gunpowder, this state of preparation consists in a certain collocation of its particles relatively to one another. In the example of the wall, it consists in a new collocation of two things relatively to each other—the wall and the paint. In the example of the molding influences on the human mind, its being a collocation at all is only conjectural; for, even on the materialistic hypothesis, it would remain to be proved that the increased facility with which the brain sums up a column of figures when it has been long trained to calculation, is the result of a permanent new arrangement of some of its material particles. We must, therefore, content ourselves with what we know, and must include among the effects of causes, the capacities given to objects of being causes of other effects. This capacity is not a real thing existing in the objects; it is but a name for our conviction that they will act in a particular manner when certain new circumstances arise. We may invest this assurance of future events with a fictitious objective existence, by calling it a *state* of the object. But unless the state consists, as in the case of the gunpowder it does, in a collocation of particles, it expresses no present fact; it is but the contingent future fact brought back under another name.

It may be thought that this form of causation requires us to admit an exception to the doctrine that the conditions of a phenomenon—the antecedents required for calling it into existence—must all be found among the facts immediately, not remotely, preceding its commencement. But what we have arrived at is not a correction, it is only an explanation, of that doctrine. In the enumeration of the conditions required for the occurrence of any phenomenon, it always has to be included that objects must be present, possessed of given properties. It is a condition of the phenomenon explosion that an object should be present, of one or other of certain kinds, which for that reason are called explosive. The presence of one of these objects is a condition immediately precedent to the explosion. The condition which is not immediately precedent is the cause which produced, not the explosion, but the explosive property. The conditions of the explosion itself were all present immediately before it took place, and the general law, therefore, remains intact.

§ 6. It now remains to advert to a distinction which is of first-rate importance both for clearing up the notion of cause, and for obviating a very specious objection often made against the view which we have taken of the subject.

When we define the cause of any thing (in the only sense in which the present inquiry has any concern with causes) to be "the antecedent which it invariably follows," we do not use this phrase as exactly synonymous with "the antecedent which it invariably *has* followed in our past experience." Such a mode of conceiving causation would be liable to the objection very plausibly urged by Dr. Reid, namely, that according to this doctrine night must be the cause of day, and day the cause of night; since these phenomena have invariably succeeded one another from the beginning of the world. But it is necessary to our using the word cause, that we should believe not only that the antecedent always *has* been followed by the consequent, but that, as long as the present constitution of things* endures, it always *will* be so. And this would not be true of day and night. We do not believe that night will be followed by day under all imaginable circumstances, but only that it will be so *provided* the sun rises above the

* I mean by this expression, the ultimate laws of nature (whatever they may be) as distinguished from the derivative laws and from the collocations. The diurnal revolution of the earth (for example) is not a part of the constitution of things, because nothing can be so called which might possibly be terminated or altered by natural causes.

horizon. If the sun ceased to rise, which, for aught we know, may be perfectly compatible with the general laws of matter, night would be, or might be, eternal. On the other hand, if the sun is above the horizon, his light not extinct, and no opaque body between us and him, we believe firmly that unless a change takes place in the properties of matter, this combination of antecedents will be followed by the consequent, day; that if the combination of antecedents could be indefinitely prolonged, it would be always day; and that if the same combination had always existed, it would always have been day, quite independently of night as a previous condition. Therefore is it that we do not call night the cause, nor even a condition, of day. The existence of the sun (or some such luminous body), and there being no opaque medium in a straight line* between that body and the part of the earth where we are situated, are the sole conditions; and the union of these, without the addition of any superfluous circumstance, constitutes the cause. This is what writers mean when they say that the notion of cause involves the idea of necessity. If there be any meaning which confessedly belongs to the term necessity, it is *unconditionalness.* That which is necessary, that which *must* be, means that which will be, whatever supposition we may make in regard to all other things. The succession of day and night evidently is not necessary in this sense. It is conditional on the occurrence of other antecedents. That which will be followed by a given consequent when, and only when, some third circumstance also exists, is not the cause, even though no case should ever have occurred in which the phenomenon took place without it.

Invariable sequence, therefore, is not synonymous with causation, unless the sequence, besides being invariable, is unconditional. There are sequences, as uniform in past experience as any others whatever, which yet we do not regard as cases of causation, but as conjunctions in some sort accidental. Such, to an accurate thinker, is that of day and night. The one might have existed for any length of time, and the other not have followed the sooner for its existence; it follows only if certain other antecedents exist; and where those antecedents existed, it would follow in any case. No one, probably, ever called night the cause of day; mankind must so soon have arrived at the very obvious generalization, that the state of general illumination which we call day would follow from the presence of a sufficiently luminous body, whether darkness had preceded or not.

We may define, therefore, the cause of a phenomenon, to be the antecedent, or the concurrence of antecedents, on which it is invariably and *unconditionally* consequent. Or if we adopt the convenient modification of the meaning of the word cause, which confines it to the assemblage of positive conditions without the negative, then instead of "unconditionally," we must say, "subject to no other than negative conditions."

To some it may appear, that the sequence between night and day being invariable in our experience, we have as much ground in this case as experience can give in any case, for recognizing the two phenomena as cause and effect; and that to say that more is necessary—to require a belief that the succession is unconditional, or, in other words, that it would be invariable under all changes of circumstances, is to acknowledge in causation an

* I use the words "straight line" for brevity and simplicity. In reality the line in question is not exactly straight, for, from the effect of refraction, we actually see the sun for a short interval during which the opaque mass of the earth is interposed in a direct line between the sun and our eyes; thus realizing, though but to a limited extent, the coveted desideratum of seeing round a corner.

element of belief not derived from experience. The answer to this is, that it is experience itself which teaches us that one uniformity of sequence is conditional and another unconditional. When we judge that the succession of night and day is a derivative sequence, depending on something else, we proceed on grounds of experience. It is the evidence of experience which convinces us that day could equally exist without being followed by night, and that night could equally exist without being followed by day. To say that these beliefs are "not generated by our mere observation of sequence,"* is to forget that twice in every twenty-four hours, when the sky is clear, we have an *experimentum crucis* that the cause of day is the sun. We have an experimental knowledge of the sun which justifies us on experimental grounds in concluding, that if the sun were always above the horizon there would be day, though there had been no night, and that if the sun were always below the horizon there would be night, though there had been no day. We thus know from experience that the succession of night and day is not unconditional. Let me add, that the antecedent which is only conditionally invariable, is not the invariable antecedent. Though a fact may, in experience, have always been followed by another fact, yet if the remainder of our experience teaches us that it might not always be so followed, or if the experience itself is such as leaves room for a possibility that the known cases may not correctly represent all possible cases, the hitherto invariable antecedent is not accounted the cause; but why? Because we are not sure that it *is* the invariable antecedent.

Such cases of sequence as that of day and night not only do not contradict the doctrine which resolves causation into invariable sequence, but are necessarily implied in that doctrine. It is evident, that from a limited number of unconditional sequences, there will result a much greater number of conditional ones. Certain causes being given, that is, certain antecedents which are unconditionally followed by certain consequents; the mere co-existence of these causes will give rise to an unlimited number of additional uniformities. If two causes exist together, the effects of both will exist together; and if many causes co-exist, these causes (by what we shall term hereafter the intermixture of their laws) will give rise to new effects, accompanying or succeeding one another in some particular order, which order will be invariable while the causes continue to co-exist, but no longer. The motion of the earth in a given orbit round the sun, is a series of changes which follow one another as antecedents and consequents, and will continue to do so while the sun's attraction, and the force with which the earth tends to advance in a direct line through space, continue to co-exist in the same quantities as at present. But vary either of these causes, and this particular succession of motions would cease to take place. The series of the earth's motions, therefore, though a case of sequence invariable within the limits of human experience, is not a case of causation. It is not unconditional.

This distinction between the relations of succession which, so far as we know, are unconditional, and those relations, whether of succession or of co-existence, which, like the earth's motions, or the succession of day and night, depend on the existence or on the co-existence of other antecedent facts — corresponds to the great division which Dr. Whewell and other writers have made of the field of science, into the investigation of what

* *Second Burnett Prize Essay*, by Principal Tulloch, p. 25.

they term the Laws of Phenomena, and the investigation of causes; a phraseology, as I conceive, not philosophically sustainable, inasmuch as the ascertainment of causes, such causes as the human faculties can ascertain, namely, causes which are themselves phenomena, is, therefore, merely the ascertainment of other and more universal Laws of Phenomena. And let me here observe, that Dr. Whewell, and in some degree even Sir John Herschel, seem to have misunderstood the meaning of those writers who, like M. Comté, limit the sphere of scientific investigation to Laws of Phenomena, and speak of the inquiry into causes as vain and futile. The causes which M. Comté designates as inaccessible, are efficient causes. The investigation of physical, as opposed to efficient, causes (including the study of all the active forces in Nature, considered as facts of observation) is as important a part of M. Comté's conception of science as of Dr. Whewell's. His objection to the *word* cause is a mere matter of nomenclature, in which, as a matter of nomenclature, I consider him to be entirely wrong. "Those," it is justly remarked by Mr. Bailey,* "who, like M. Comté, object to designate *events* as causes, are objecting without any real ground to a mere but extremely convenient generalization, to a very useful common name, the employment of which involves, or needs involve, no particular theory." To which it may be added, that by rejecting this form of expression, M. Comté leaves himself without any term for marking a distinction which, however incorrectly expressed, is not only real, but is one of the fundamental distinctions in science; indeed it is on this alone, as we shall hereafter find, that the possibility rests of framing a rigorous Canon of Induction. And as things left without a name are apt to be forgotten, a Canon of that description is not one of the many benefits which the philosophy of Induction has received from M. Comté's great powers.

§ 7. Does a cause always stand with its effect in the relation of antecedent and consequent? Do we not often say of two simultaneous facts that they are cause and effect—as when we say that fire is the cause of warmth, the sun and moisture the cause of vegetation, and the like? Since a cause does not necessarily perish because its effect has been produced, the two things do very generally co-exist; and there are some appearances, and some common expressions, seeming to imply not only that causes may, but that they must, be contemporaneous with their effects. *Cessante causâ cessat et effectus*, has been a dogma of the schools: the necessity for the continued existence of the cause in order to the continuance of the effect, seems to have been once a generally received doctrine. Kepler's numerous attempts to account for the motions of the heavenly bodies on mechanical principles, were rendered abortive by his always supposing that the agency which set those bodies in motion must continue to operate in order to keep up the motion which it at first produced. Yet there were at all times many familiar instances of the continuance of effects, long after their causes had ceased. A *coup de soleil* gives a person brain-fever: will the fever go off as soon as he is moved out of the sunshine? A sword is run through his body: must the sword remain in his body in order that he may continue dead? A plowshare once made, remains a plowshare, without any continuance of heating and hammering, and even after the man who heated and hammered it has been gathered to his fathers. On the other hand, the pressure which forces up the mercury in an exhausted tube must be

* *Letters on the Philosophy of the Human Mind*, First Series, p. 219.

continued in order to sustain it in the tube. This (it may be replied) is because another force is acting without intermission, the force of gravity, which would restore it to its level, unless counterpoised by a force equally constant. But again: a tight bandage causes pain, which pain will sometimes go off as soon as the bandage is removed. The illumination which the sun diffuses over the earth ceases when the sun goes down.

There is, therefore, a distinction to be drawn. The conditions which are necessary for the first production of a phenomenon, are occasionally also necessary for its continuance; though more commonly its continuance requires no condition except negative ones. Most things, once produced, continue as they are, until something changes or destroys them; but some require the permanent presence of the agencies which produced them at first. These may, if we please, be considered as instantaneous phenomena, requiring to be renewed at each instant by the cause by which they were at first generated. Accordingly, the illumination of any given point of space has always been looked upon as an instantaneous fact, which perishes and is perpetually renewed as long as the necessary conditions subsist. If we adopt this language we avoid the necessity of admitting that the continuance of the cause is ever required to maintain the effect. We may say, it is not required to maintain, but to reproduce, the effect, or else to counteract some force tending to destroy it. And this may be a convenient phraseology. But it is only a phraseology. The fact remains, that in some cases (though those are a minority) the continuance of the conditions which produced an effect is necessary to the continuance of the effect.

As to the ulterior question, whether it is strictly necessary that the cause, or assemblage of conditions, should precede, by ever so short an instant, the production of the effect (a question raised and argued with much ingenuity by Sir John Herschel in an Essay already quoted),* the inquiry is of no consequence for our present purpose. There certainly are cases in which the effect follows without any interval perceptible by our faculties; and when there is an interval, we can not tell by how many intermediate links imperceptible to us that inverval may really be filled up. But even granting that an effect may commence simultaneously with its cause, the view I have taken of causation is in no way practically affected. Whether the cause and its effect be necessarily successive or not, the beginning of a phenomenon is what implies a cause, and causation is the law of the succession of phenomena. If these axioms be granted, we can afford, though I see no necessity for doing so, to drop the words antecedent and consequent as applied to cause and effect. I have no objection to define a cause, the assemblage of phenomena, which occurring, some other phenomenon invariably commences, or has its origin. Whether the effect coincides in point of time with, or immediately follows, the hindmost of its conditions, is immaterial. At all events, it does not precede it; and when we are in doubt, between two co-existent phenomena, which is cause and which effect, we rightly deem the question solved if we can ascertain which of them preceded the other.

§ 8. It continually happens that several different phenomena, which are not in the slightest degree dependent or conditional on one another, are found all to depend, as the phrase is, on one and the same agent; in other words, one and the same phenomenon is seen to be followed by several

* *Essays*, pp. 206–208.

sorts of effects quite heterogeneous, but which go on simultaneously one with another; provided, of course, that all other conditions requisite for each of them also exist. Thus, the sun produces the celestial motions; it produces daylight, and it produces heat. The earth causes the fall of heavy bodies, and it also, in its capacity of a great magnet, causes the phenomena of the magnetic needle. A crystal of galena causes the sensations of hardness, of weight, of cubical form, of gray color, and many others between which we can trace no interdependence. The purpose to which the phraseology of Properties and Powers is specially adapted, is the expression of this sort of cases. When the same phenomenon is followed (either subject or not to the presence of other conditions) by effects of different and dissimilar orders, it is usual to say that each different sort of effect is produced by a different property of the cause. Thus we distinguish the attractive or gravitative property of the earth, and its magnetic property: the gravitative, luminiferous, and calorific properties of the sun: the color, shape, weight, and hardness of a crystal. These are mere phrases, which explain nothing, and add nothing to our knowledge of the subject; but, considered as abstract names denoting the connection between the different effects produced and the object which produces them, they are a very powerful instrument of abridgment, and of that acceleration of the process of thought which abridgment accomplishes.

This class of considerations leads to a conception which we shall find to be of great importance, that of a Permanent Cause, or original natural agent. There exist in nature a number of permanent causes, which have subsisted ever since the human race has been in existence, and for an indefinite and probably an enormous length of time previous. The sun, the earth, and planets, with their various constituents, air, water, and other distinguishable substances, whether simple or compound, of which nature is made up, are such Permanent Causes. These have existed, and the effects or consequences which they were fitted to produce have taken place (as often as the other conditions of the production met), from the very beginning of our experience. But we can give no account of the origin of the Permanent Causes themselves. Why these particular natural agents existed originally and no others, or why they are commingled in such and such proportions, and distributed in such and such a manner throughout space, is a question we can not answer. More than this: we can discover nothing regular in the distribution itself; we can reduce it to no uniformity, to no law. There are no means by which, from the distribution of these causes or agents in one part of space, we could conjecture whether a similar distribution prevails in another. The co-existence, therefore, of Primeval Causes ranks, to us, among merely casual concurrences: and all those sequences or co-existences among the effects of several such causes, which, though invariable while those causes co-exist, would, if the co-existence terminated, terminate along with it, we do not class as cases of causation, or laws of nature: we can only calculate on finding these sequences or co-existences where we know by direct evidence, that the natural agents on the properties of which they ultimately depend, are distributed in the requisite manner. These Permanent Causes are not always objects; they are sometimes events, that is to say, periodical cycles of events, that being the only mode in which events can possess the property of permanence. Not only, for instance, is the earth itself a permanent cause, or primitive natural agent, but the earth's rotation is so too: it is a cause which has produced, from the earliest period (by the aid of other necessary conditions), the succession of day and night, the ebb and flow of the sea, and many other effects, while, as we can assign no cause (except conjecturally) for the rotation itself, it is entitled to be ranked as a primeval cause. It is, however, only the *origin* of the rotation which is mysterious to us: once begun, its continuance is accounted for by the first law of motion (that of the permanence of rectilinear motion once impressed) combined with the gravitation of the parts of the earth toward one another.

All phenomena without exception which begin to exist, that is, all except the primeval causes, are effects either immediate or remote of those primitive facts, or of some combination of them. There is no Thing produced, no event happening, in the known universe, which is not connected by a uniformity, or invariable sequence, with some one or more of the phenomena which preceded it; insomuch that it will happen again as often as those phenomena occur again, and as no other phenomenon having the character of a counteracting cause shall co-exist. These antecedent phenomena, again, were connected in a similar manner with some that preceded them; and so on, until we reach, as the ultimate step attainable by us, either the properties of some one primeval cause, or the conjunction of several. The whole of the phenomena of nature were therefore the necessary, or, in other words, the unconditional, consequences of some former collocation of the Permanent Causes.

The state of the whole universe at any instant, we believe to be the consequence of its state at the previous instant; insomuch that one who knew all the agents which exist at the present moment, their collocation in space, and all their properties, in other words, the laws of their agency, could predict the whole subsequent history of the universe, at least unless some new volition of a power capable of controlling the universe should supervene.* And if any particular state of the entire universe could ever recur a second time, all subsequent states would return too, and history would, like a circulating decimal of many figures, periodically repeat itself:

> Jam redit et virgo, redeunt Saturnia regna......
> Alter erit tum Tiphys, et altera quæ vehat Argo
> Delectos heroas; erunt quoque altera bella.
> Atque iterum ad Trojam magnus mittetur Achilles.

And though things do not really revolve in this eternal round, the whole series of events in the history of the universe, past and future, is not the less capable, in its own nature, of being constructed *a priori* by any one

* To the universality which mankind are agreed in ascribing to the Law of Causation, there is one claim of exception, one disputed case, that of the Human Will; the determinations of which, a large class of metaphysicians are not willing to regard as following the causes called motives, according to as strict laws as those which they suppose to exist in the world of mere matter. This controverted point will undergo a special examination when we come to treat particularly of the Logic of the Moral Sciences (Book vi., chap. 2). In the mean time, I may remark that these metaphysicians, who, it must be observed, ground the main part of their objection on the supposed repugnance of the doctrine in question to our consciousness, seem to me to mistake the fact which consciousness testifies against. What is really in contradiction to consciousness, they would, I think, on strict self-examination, find to be, the application to human actions and volitions of the ideas involved in the common use of the term Necessity; which I agree with them in objecting to. But if they would consider that by saying that a person's actions *necessarily* follow from his character, all that is really meant (for no more is meant in any case whatever of causation) is that he invariably *does* act in conformity to his character, and that any one who thoroughly knew his character could certainly predict how he would act in any supposable case; they probably would not find this doctrine either contrary to their experience or revolting to their feelings. And no more than this is contended for by any one but an Asiatic fatalist.

whom we can suppose acquainted with the original distribution of all natural agents, and with the whole of their properties, that is, the laws of succession existing between them and their effects: saving the far more than human powers of combination and calculation which would be required, even in one possessing the data, for the actual performance of the task.

§ 9. Since every thing which occurs is determined by laws of causation and collocations of the original causes, it follows that the co-existences which are observable among effects can not be themselves the subject of any similar set of laws, distinct from laws of causation. Uniformities there are, as well of co-existence as of succession, among effects; but these must in all cases be a mere result either of the identity or of the co-existence of their causes: if the causes did not co-exist, neither could the effects. And these causes being also effects of prior causes, and these of others, until we reach the primeval causes, it follows that (except in the case of effects which can be traced immediately or remotely to one and the same cause) the co-existences of phenomena can in no case be universal, unless the co-existences of the primeval causes to which the effects are ultimately traceable can be reduced to a universal law: but we have seen that they can not. There are, accordingly, no original and independent, in other words no unconditional, uniformities of co-existence, between effects of different causes; if they co-exist, it is only because the causes have casually co-existed. The only independent and unconditional co-existences which are sufficiently invariable to have any claim to the character of laws, are between different and mutually independent effects of the same cause; in other words, between different properties of the same natural agent. This portion of the Laws of Nature will be treated of in the latter part of the present Book, under the name of the Specific Properties of Kinds.

§ 10. Since the first publication of the present treatise, the sciences of physical nature have made a great advance in generalization, through the doctrine known as the Conservation or Persistence of Force. This imposing edifice of theory, the building and laying out of which has for some time been the principal occupation of the most systematic minds among physical inquirers, consists of two stages: one, of ascertained fact, the other containing a large element of hypothesis.

To begin with the first. It is proved by numerous facts, both natural and of artificial production, that agencies which had been regarded as distinct and independent sources of force—heat, electricity, chemical action, nervous and muscular action, momentum of moving bodies — are interchangeable, in definite and fixed quantities, with one another. It had long been known that these dissimilar phenomena had the power, under certain conditions, of producing one another: what is new in the theory is a more accurate estimation of what this production consists in. What happens is, that the whole or part of the one kind of phenomena disappears, and is replaced by phenomena of one of the other descriptions, and that there is an equivalence in quantity between the phenomena that have disappeared and those which have been produced, insomuch that if the process be reversed, the very same quantity which had disappeared will re-appear, without increase or diminution. Thus the amount of heat which will raise the temperature of a pound of water one degree of the thermometer, will, if expended, say in the expansion of steam, lift a weight of 772 pounds one foot, or a weight of one pound 772 feet: and the same exact quantity of heat can, by certain means, be recovered, through the expenditure of exactly that amount of mechanical motion.

The establishment of this comprehensive law has led to a change in the language in which the scientific world had been accustomed to speak of what are called the Forces of nature. Before this correlation between phenomena most unlike one another had been ascertained, their unlikeness had caused them to be referred to so many distinct forces. Now that they are known to be convertible into one another without loss, they are spoken of as all of them results of one and the same force, manifesting itself in different modes. This force (it is said) can only produce a limited and definite quantity of effect, but always does produce that definite quantity; and produces it, according to circumstances, in one or another of the forms, or divides it among several, but so as (according to a scale of numerical equivalents established by experiment) always to make up the same sum; and no one of the manifestations can be produced, save by the disappearance of the equivalent quantity of another, which in its turn, in appropriate circumstances, will re-appear undiminished. This mutual interchangeability of the forces of nature, according to fixed numerical equivalents, is the part of the new doctrine which rests on irrefragable fact.

To make the statement true, however, it is necessary to add, that an indefinite and perhaps immense interval of time may elapse between the disappearance of the force in one form and its re-appearance in another. A stone thrown up into the air with a given force, and falling back immediately, will, by the time it reaches the earth, recover the exact amount of mechanical momentum which was expended in throwing it up, deduction being made of a small portion of motion which has been communicated to the air. But if the stone has lodged on a height, it may not fall back for years, or perhaps ages, and until it does, the force expended in raising it is temporarily lost, being represented only by what, in the language of the new theory, is called potential energy. The coal imbedded in the earth is considered by the theory as a vast reservoir of force, which has remained dormant for many geological periods, and will so remain until, by being burned, it gives out the stored-up force in the form of heat. Yet it is not supposed that this force is a material thing which can be confined by bounds, as used to be thought of latent heat when that important phenomenon was first discovered. What is meant is that when the coal does at last, by combustion, generate a quantity of heat (transformable like all other heat into mechanical momentum, and the other forms of force), this extrication of heat is the re-appearance of a force derived from the sun's rays, expended myriads of ages ago in the vegetation of the organic substances which were the material of the coal.

Let us now pass to the higher stage of the theory of Conservation of Force; the part which is no longer a generalization of proved fact, but a combination of fact and hypothesis. Stated in few words, it is as follows: That the Conservation of Force is really the Conservation of Motion; that in the various interchanges between the forms of force, it is always motion that is transformed into motion. To establish this, it is necessary to assume motions which are hypothetical. The supposition is, that there are motions which manifest themselves to our senses only as heat, electricity, etc., being molecular motions; oscillations, invisible to us, among the minute particles of bodies; and that these molecular motions are transmutable into molar motions (motions of masses), and molar motions into molecular.

Now there is a real basis of fact for this supposition: we have positive evidence of the existence of molecular motion in these manifestations of force. In the case of chemical action, for instance, the particles separate and form new combinations, often with a great visible disturbance of the mass. In the case of heat, the evidence is equally conclusive, since heat expands bodies (that is, causes their particles to move *from* one another); and if of sufficient amount, changes their mode of aggregation from solid to liquid, or from liquid to gaseous. Again, the mechanical actions which produce heat—friction, and the collision of bodies—must from the nature of the case produce a shock, that is, an internal motion of particles, which indeed, we find, is often so violent as to break them permanently asunder. Such facts are thought to warrant the inference, that it is not, as was supposed, heat that causes the motion of particles, but the motion of particles that causes heat; the original cause of both being the previous motion (whether molar or molecular—collision of bodies or combustion of fuel) which formed the heating agency. This inference already contains hypothesis; but at least the supposed cause, the intestine motion of molecules, is a *vera causa*. But in order to reduce the Conservation of Force to Conservation of Motion, it was necessary to attribute to motion the heat propagated, through apparently empty space, from the sun. This required the supposition (already made for the explanation of the laws of light) of a subtle ether pervading space, which, though impalpable to us, must have the property which constitutes matter, that of resistance, since waves are propagated through it by an impulse from a given point. The ether must be supposed (a supposition not required by the theory of light) to penetrate into the minute interstices of all bodies. The vibratory motion supposed to be taking place in the heated mass of the sun, is considered as imparted from that mass to the particles of the surrounding ether, and through them to the particles of the same ether in the interstices of terrestrial bodies; and this, too, with a sufficient mechanical force to throw the particles of those bodies into a state of similar vibration, producing the expansion of their mass, and the sensation of heat in sentient creatures. All this is hypothesis, though, of its legitimacy as hypothesis, I do not mean to express any doubt. It would seem to follow as a consequence from this theory, that Force may and should be defined, matter in motion. This definition, however, will not stand, for, as has already been seen, the matter needs not be in *actual* motion. It is not necessary to suppose that the motion afterward manifested, is actually taking place among the molecules of the coal during its sojourn in the earth;* certainly not in the stone which is at rest on the eminence to which it has been raised. The true definition of Force must be, not motion, but Potentiality of Motion; and what the doctrine, if established, amounts to, is, not that there is at all times the same quantity of actual motion in the universe; but that the possibilities of motion are limited to a definite quantity, which can not be added to, but which can not be exhausted; and that all actual motion which takes place in Nature is a draft upon this limited stock. It needs not all of it have ever existed as actual motion. There is a vast amount of potential motion in the universe in the form of gravitation, which it would be a great abuse of

* I believe, however, the accredited authorities do suppose that molecular motion, equivalent in amount to that which will be manifested in the combustion of the coal, is actually taking place during the whole of the long interval, if not in the coal, yet in the oxygen which will then combine with it. But how purely hypothetical this supposition is, need hardly be remarked; I venture to say, unnecessarily and extravagantly hypothetical.

hypothesis to suppose to have been stored up by the expenditure of an equal amount of actual motion in some former state of the universe. Nor does the motion produced by gravity take place, so far as we know, at the expense of any other motion, either molar or molecular.

It is proper to consider whether the adoption of this theory as a scientific truth, involving as it does a change in the conception hitherto entertained of the most general physical agencies, requires any modification in the view I have taken of Causation as a law of nature. As it appears to me, none whatever. The manifestations which the theory regards as modes of motion, are as much distinct and separate phenomena when referred to a single force, as when attributed to several. Whether the phenomenon is called a transformation of force or the generation of one, it has its own set or sets of antecedents, with which it is connected by invariable and unconditional sequence; and that set, or those sets, of antecedents are its cause. The relation of the Conservation theory to the principle of Causation is discussed in much detail, and very instructively, by Professor Bain, in the second volume of his Logic. The chief practical conclusion drawn by him, bearing on Causation, is, that we must distinguish in the assemblage of conditions which constitutes the Cause of a phenomenon, two elements: one, the presence of a force; the other, the collocation or position of objects which is required in order that the force may undergo the particular transmutation which constitutes the phenomenon. Now, it might always have been said with acknowledged correctness, that a force and a collocation were both of them necessary to produce any phenomenon. The law of causation is, that change can only be produced by change. Along with any number of stationary antecedents, which are collocations, there must be at least one changing antecedent, which is a force. To produce a bonfire, there must not only be fuel, and air, and a spark, which are collocations, but chemical action between the air and the materials, which is a force. To grind corn, there must be a certain collocation of the parts composing a mill, relatively to one another and to the corn; but there must also be the gravitation of water, or the motion of wind, to supply a force. But as the force in these cases was regarded as a property of the objects in which it is embodied, it seemed tautology to say that there must be the collocation *and* the force. As the collocation must be a collocation of objects possessing the force-giving property, the collocation, so understood, included the force.

How, then, shall we have to express these facts, if the theory be finally substantiated that all Force is reducible to a previous Motion? We shall have to say, that one of the conditions of every phenomenon is an antecedent Motion. But it will have to be explained that this needs not be *actual* motion. The coal which supplies the force exerted in combustion is not shown to have been exerting that force in the form of molecular motion in the pit; it was not even exerting pressure. The stone on the eminence *is* exerting a pressure, but only equivalent to its weight, not to the additional momentum it would acquire by falling. The antecedent, therefore, is not a force in action; and we can still only call it a property of the objects, by which they would exert a force on the occurrence of a fresh collocation. The collocation, therefore, still includes the force. The force said to be stored up, is simply a particular property which the object has acquired. The cause we are in search of, is a collocation of objects possessing that particular property. When, indeed, we inquire further into the cause from which they derive that property, the new conception introduced by the Conservation theory comes in: the property is itself an effect, and its cause, according to the theory, is a former motion of exactly equivalent amount, which has been impressed on the particles of the body, perhaps at some very distant period. But the case is simply one of those we have already considered, in which the efficacy of a cause consists in its investing an object with a property. The force said to be laid up, and merely potential, is no more a really existing thing than any other properties of objects are really existing things. The expression is a mere artifice of language, convenient for describing the phenomena: it is unnecessary to suppose that any thing has been in continuous existence except an abstract potentiality. A force suspended in its operation, neither manifesting itself by motion nor by pressure, is not an existing fact, but a name for our conviction that in appropriate circumstances a fact would take place. We know that a pound weight, were it to fall from the earth into the sun, would acquire in falling a momentum equal to millions of pounds; but we do not credit the pound weight with more of actually existing force than is equal to the pressure it is now exerting on the earth, and that is exactly a pound. We might as well say that a force of millions of pounds exists in a pound, as that the force which will manifest itself when the coal is burned is a real thing existing in the coal. What is fixed in the coal is only a certain property: it has become fit to be the antecedent of an effect called combustion, which partly consists in giving out, under certain conditions, a given definite quantity of heat.

We thus see that no new general conception of Causation is introduced by the Conservation theory. The indestructibility of Force no more interferes with the theory of Causation than the indestructibility of Matter, meaning by matter the element of resistance in the sensible world. It only enables us to understand better than before the nature and laws of some of the sequences.

This better understanding, however, enables us, with Mr. Bain, to admit, as one of the tests for distinguishing causation from mere concomitance, the expenditure or transfer of energy. If the effect, or any part of the effect, to be accounted for, consists in putting matter in motion, then any of the objects present which has lost motion has contributed to the effect; and this is the true meaning of the proposition that the cause is that one of the antecedents which exerts active force.

§ 11. It is proper in this place to advert to a rather ancient doctrine respecting causation, which has been revived during the last few years in many quarters, and at present gives more signs of life than any other theory of causation at variance with that set forth in the preceding pages.

According to the theory in question, Mind, or to speak more precisely, Will, is the only cause of phenomena. The type of Causation, as well as the exclusive source from which we derive the idea, is our own voluntary agency. Here, and here only (it is said), we have direct evidence of causation. We know that we can move our bodies. Respecting the phenomena of inanimate nature, we have no other direct knowledge than that of antecedence and sequence. But in the case of our voluntary actions, it is affirmed that we are conscious of power before we have experience of results. An act of volition, whether followed by an effect or not, is accompanied by a consciousness of effort, "of force exerted, of power in action, which is necessarily causal, or causative." This feeling of energy or force, inherent in an act of will, is knowledge *a priori;* assurance, prior to experience, that we have the power of causing effects. Volition, therefore, it is asserted, is something more than an unconditional antecedent; it is a cause, in a different sense from that in which physical phenomena are said to cause one another: it is an Efficient Cause. From this the transition is easy to the further doctrine, that Volition is the *sole* Efficient Cause of all phenomena. "It is inconceivable that dead force could continue unsupported for a moment beyond its creation. We can not even conceive of change or phenomena without the energy of a mind." "The word *action*" itself, says another writer of the same school, "has no real significance except when applied to the doings of an intelligent agent. Let any one conceive, if he can, of any power, energy, or force inherent in a lump of matter." Phenomena may have the semblance of being produced by physical causes, but they are in reality produced, say these writers, by the immediate agency of mind. All things which do not proceed from a human (or, I suppose, an animal) will proceed, they say, directly from divine will. The earth is not moved by the combination of a centripetal and a projectile force; this is but a mode of speaking, which serves to facilitate our conceptions. It is moved by the direct volition of an omnipotent Being, in a path coinciding with that which we deduce from the hypothesis of these two forces.

As I have so often observed, the general question of the existence of Efficient Causes does not fall within the limits of our subject; but a theory which represents them as capable of being subjects of human knowledge, and which passes off as efficient causes what are only physical or phenomenal causes, belongs as much to Logic as to metaphysics, and is a fit subject for discussion here.

To my apprehension, a volition is not an efficient, but simply a physical cause. Our will causes our bodily actions in the same sense, and in no other, in which cold causes ice, or a spark causes an explosion of gunpowder. The volition, a state of our mind, is the antecedent; the motion of our limbs in conformity to the volition, is the consequent. This sequence I conceive to be not a subject of direct consciousness, in the sense intended by the theory. The antecedent, indeed, and the consequent, are subjects of consciousness. But the connection between them is a subject of experience. I can not admit that our consciousness of the volition contains in itself any *a priori* knowledge that the muscular motion will follow. If our nerves of motion were paralyzed, or our muscles stiff and inflexible, and had been so all our lives, I do not see the slightest ground for supposing that we should ever (unless by information from other people) have known any thing of volition as a physical power, or been conscious of any tendency in feelings of our mind to produce motions of our body, or of other bodies. I will not undertake to say whether we should in that case have had the physical feeling which I suppose is meant when these writers speak of "consciousness of effort:" I see no reason why we should not; since that physical feeling is probably a state of nervous sensation beginning and ending in the brain, without involving the motory apparatus: but we certainly should not have designated it by any term equivalent to effort, since effort implies consciously aiming at an end, which we should not only in that case have had no reason to do, but could not even have had the idea of doing. If conscious at all of this peculiar sensation, we should have been conscious of it, I conceive, only as a kind of uneasiness, accompanying our feelings of desire.

It is well argued by Sir William Hamilton against the theory in question, that it "is refuted by the consideration that between the overt fact of corporeal movement of which we are cognizant, and the internal act of mental determination of which we are also cognizant, there intervenes a numerous series of intermediate agencies of which we have no knowledge; and, consequently, that we can have no consciousness of any causal connection between the extreme links of this chain, the volition to move and the limb moving, as this hypothesis asserts. No one is immediately conscious, for example, of moving his arm through his volition. Previously to this ultimate movement, muscles, nerves, a multitude of solid and fluid parts, must be set in motion by the will, but of this motion we know, from consciousness, absolutely nothing. A person struck with paralysis is conscious of no inability in his limb to fulfill the determinations of his will; and it is only after having willed, and finding that his limbs do not obey his volition, that he learns by this experience, that the external movement does not follow the internal act. But as the paralytic learns after the volition that his limbs do not obey his mind; so it is only after volition that the man in health learns, that his limbs do obey the mandates of his will."*

Those against whom I am contending have never produced, and do not pretend to produce, any positive evidence† that the power of our will to move our bodies would be known to us independently of experience. What they have to say on the subject is, that the production of physical events by a will seems to carry its own explanation with it, while the action of matter upon matter seems to require something else to explain it; and is even, according to them, "inconceivable" on any other supposition than that some will intervenes between the apparent cause and its apparent effect. They thus rest their case on an appeal to the inherent laws of our conceptive faculty; mistaking, as I apprehend, for the laws of that faculty its acquired habits, grounded on the spontaneous tendencies of its uncultured state. The succession between the will to move a limb and the actual motion is one of the most direct and instantaneous of all sequences which come under our observation, and is familiar to every moment's experience from our earliest infancy; more familiar than any succession of events ex-

* *Lectures on Metaphysics*, vol. ii., Lect. xxxix., pp. 391–2.

I regret that I can not invoke the authority of Sir William Hamilton in favor of my own opinions on Causation, as I can against the particular theory which I am now combating. But that acute thinker has a theory of Causation peculiar to himself, which has never yet, as far as I know, been analytically examined, but which, I venture to think, admits of as complete refutation as any one of the false or insufficient psychological theories which strew the ground in such numbers under his potent metaphysical scythe. (Since examined and controverted in the sixteenth chapter of *An Examination of Sir William Hamilton's Philosophy.*)

† Unless we are to consider as such the following statement, by one of the writers quoted in the text: "In the case of mental exertion, the result to be accomplished is *preconsidered* or meditated, and is therefore known *a priori*, or before experience."—(Bowen's *Lowell Lectures on the Application of Metaphysical and Ethical Science to the Evidence of Religion*. Boston, 1849.) This is merely saying that when we will a thing we have an idea of it. But to have an idea of what we wish to happen, does not imply a prophetic knowledge that it will happen. Perhaps it will be said that the *first time* we exerted our will, when we had of course no experience of any of the powers residing in us, we nevertheless must already have known that we possessed them, since we can not will that which we do not believe to be in our power. But the impossibility is perhaps in the words only, and not in the facts; for we may *desire* what we do not know to be in our power; and finding by experience that our bodies move according to our *desire*, we may then, and only then, pass into the more complicated mental state which is termed will.

After all, even if we had an instinctive knowledge that our actions would follow our will, this, as Brown remarks, would prove nothing as to the nature of Causation. Our knowing, previous to experience, that an antecedent will be followed by a certain consequent, would not prove the relation between them to be any thing more than antecedence and consequence.

terior to our bodies, and especially more so than any other case of the apparent origination (as distinguished from the mere communication) of motion. Now, it is the natural tendency of the mind to be always attempting to facilitate its conception of unfamiliar facts by assimilating them to others which are familiar. Accordingly, our voluntary acts, being the most familiar to us of all cases of causation, are, in the infancy and early youth of the human race, spontaneously taken as the type of causation in general, and all phenomena are supposed to be directly produced by the will of some sentient being. This original Fetichism I shall not characterize in the words of Hume, or of any follower of Hume, but in those of a religious metaphysician, Dr. Reid, in order more effectually to show the unanimity which exists on the subject among all competent thinkers.

"When we turn our attention to external objects, and begin to exercise our rational faculties about them, we find that there are some motions and changes in them which we have power to produce, and that there are many which must have some other cause. Either the objects must have life and active power, as we have, or they must be moved or changed by something that has life and active power, as external objects are moved by us.

"Our first thoughts seem to be, that the objects in which we perceive such motion have understanding and active power as we have. 'Savages,' says the Abbé Raynal, 'wherever they see motion which they can not account for, there they suppose a soul.' All men may be considered as savages in this respect, until they are capable of instruction, and of using their faculties in a more perfect manner than savages do.

"The Abbé Raynal's observation is sufficiently confirmed, both from fact, and from the structure of all languages.

"Rude nations do really believe sun, moon, and stars, earth, sea, and air, fountains, and lakes, to have understanding and active power. To pay homage to them, and implore their favor, is a kind of idolatry natural to savages.

"All languages carry in their structure the marks of their being formed when this belief prevailed. The distinction of verbs and participles into active and passive, which is found in all languages, must have been originally intended to distinguish what is really active from what is merely passive; and in all languages, we find active verbs applied to those objects, in which, according to the Abbé Raynal's observation, savages suppose a soul.

"Thus we say the sun rises and sets, and comes to the meridian, the moon changes, the sea ebbs and flows, the winds blow. Languages were formed by men who believed these objects to have life and active power in themselves. It was therefore proper and natural to express their motions and changes by active verbs.

"There is no surer way of tracing the sentiments of nations before they have records, than by the structure of their language, which, notwithstanding the changes produced in it by time, will always retain some signatures of the thoughts of those by whom it was invented. When we find the same sentiments indicated in the structure of all languages, those sentiments must have been common to the human species when languages were invented.

"When a few, of superior intellectual abilities, find leisure for speculation, they begin to philosophize, and soon discover, that many of those objects which at first they believed to be intelligent and active are really lifeless and passive. This is a very important discovery. It elevates the mind, emancipates from many vulgar superstitions, and invites to further discoveries of the same kind.

"As philosophy advances, life and activity in natural objects retires, and leaves them dead and inactive. Instead of moving voluntarily, we find them to be moved necessarily; instead of acting, we find them to be acted upon; and Nature appears as one great machine, where one wheel is turned by another, that by a third; and how far this necessary succession may reach, the philosopher does not know."*

There is, then, a spontaneous tendency of the intellect to account to itself for all cases of causation by assimilating them to the intentional acts of voluntary agents like itself. This is the instinctive philosophy of the human mind in its earliest stage, before it has become familiar with any other invariable sequences than those between its own volitions or those of other human beings and their voluntary acts. As the notion of fixed laws of succession among external phenomena gradually establishes itself, the propensity to refer all phenomena to voluntary agency slowly gives way before it. The suggestions, however, of daily life continuing to be more powerful than those of scientific thought, the original instinctive philosophy maintains its ground in the mind, underneath the growths obtained by cultivation, and keeps up a constant resistance to their throwing their roots deep into the soil. The theory against which I am contending derives its nourishment from that substratum. Its strength does not lie in argument, but in its affinity to an obstinate tendency of the infancy of the human mind.

That this tendency, however, is not the result of an inherent mental law, is proved by superabundant evidence. The history of science, from its earliest dawn, shows that mankind have not been unanimous in thinking either that the action of matter upon matter was not conceivable, or that the action of mind upon matter was. To some thinkers, and some schools of thinkers, both in ancient and in modern times, this last has appeared much more inconceivable than the former. Sequences entirely physical and material, as soon as they had become sufficiently familiar to the human mind, came to be thought perfectly natural, and were regarded not only as needing no explanation themselves, but as being capable of affording it to others, and even of serving as the ultimate explanation of things in general.

One of the ablest recent supporters of the Volitional theory has furnished an explanation, at once historically true and philosophically acute, of the failure of the Greek philosophers in physical inquiry, in which, as I conceive, he unconsciously depicts his own state of mind. "Their stumbling-block was one as to the nature of the evidence they had to expect for their conviction..... They had not seized the idea that they must not expect to understand the processes of outward causes, but only their results; and consequently, the whole physical philosophy of the Greeks was an attempt to identify mentally the effect with its cause, to feel after some not only necessary but natural connection, where they meant by natural that which would *per se* carry some presumption to their own mind. .... They wanted to see some *reason* why the physical antecedent should produce this particular consequent, and their only attempts were in directions where they could find such reasons."† In other words, they were not content merely to know that one phenomenon was always followed by another; they thought that they had not attained the true aim of science, unless they could perceive something in the nature of the one phenomenon

* Reid's *Essays on the Active Powers*, Essay iv., chap. 3.

† *Prospective Review* for February, 1850.

from which it might have been known or presumed *previous to trial* that it would be followed by the other: just what the writer, who has so clearly pointed out their error, thinks that he perceives in the nature of the phenomenon Volition. And to complete the statement of the case, he should have added that these early speculators not only made this their aim, but were quite satisfied with their success in it; not only sought for causes which should carry in their mere statement evidence of their efficiency, but fully believed that they had found such causes. The reviewer can see plainly that this was an error, because *he* does not believe that there exist any relations between material phenomena which can account for their producing one another; but the very fact of the persistency of the Greeks in this error, shows that their minds were in a very different state: they were able to derive from the assimilation of physical facts to other physical facts, the kind of mental satisfaction which we connect with the word explanation, and which the reviewer would have us think can only be found in referring phenomena to a will. When Thales and Hippo held that moisture was the universal cause, and external element, of which all other things were but the infinitely various sensible manifestations; when Anaximenes predicated the same thing of air, Pythagoras of numbers, and the like, they all thought that they had found a real explanation; and were content to rest in this explanation as ultimate. The ordinary sequences of the external universe appeared to them, no less than to their critic, to be inconceivable without the supposition of some universal agency to connect the antecedents with the consequents; but they did not think that Volition, exerted by minds, was the only agency which fulfilled this requirement. Moisture, or air, or numbers, carried to their minds a precisely similar impression of making intelligible what was otherwise inconceivable, and gave the same full satisfaction to the demands of their conceptive faculty.

It was not the Greeks alone, who "wanted to see some reason why the physical antecedent should produce this particular consequent," some connection "which would *per se* carry some presumption to their own mind." Among modern philosophers, Leibnitz laid it down as a self-evident principle that all physical causes without exception must contain in their own nature something which makes it intelligible that they should be able to produce the effects which they do produce. Far from admitting Volition as the only kind of cause which carried internal evidence of its own power, and as the real bond of connection between physical antecedents and their consequents, he demanded some naturally and *per se* efficient physical antecedent as the bond of connection between Volition itself and its effects. He distinctly refused to admit the will of God as a sufficient explanation of any thing except miracles; and insisted upon finding something that would account *better* for the phenomena of nature than a mere reference to divine volition.*

Again, and conversely, the action of mind upon matter (which, we are now told, not only needs no explanation itself, but is the explanation of all other effects), has appeared to some thinkers to be itself the grand inconceivability. It was to get over this very difficulty that the Cartesians invented the system of Occasional Causes. They could not conceive that thoughts in a mind could produce movements in a body, or that bodily movements could produce thoughts. They could see no necessary connec-

* Vide supra, p. 178, note.

tion, no relation *a priori*, between a motion and a thought. And as the Cartesians, more than any other school of philosophical speculation before or since, made their own minds the measure of all things, and refused, on principle, to believe that Nature had done what they were unable to see any reason why she must do, they affirmed it to be impossible that a material and a mental fact could be causes one of another. They regarded them as mere Occasions on which the real agent, God, thought fit to exert his power as a Cause. When a man wills to move his foot, it is not his will that moves it, but God (they said) moves it on the occasion of his will. God, according to this system, is the only efficient cause, not *quâ* mind, or *quâ* endowed with volition, but *quâ* omnipotent. This hypothesis was, as I said, originally suggested by the supposed inconceivability of any real mutual action between Mind and Matter; but it was afterward extended to the action of Matter upon Matter, for on a nicer examination they found this inconceivable too, and therefore, according to their logic, impossible. The *deus ex machinâ* was ultimately called in to produce a spark on the occasion of a flint and steel coming together, or to break an egg on the occasion of its falling on the ground.

All this, undoubtedly, shows that it is the disposition of mankind in general, not to be satisfied with knowing that one fact is invariably antecedent and another consequent, but to look out for something which may seem to explain their being so. But we also see that this demand may be completely satisfied by an agency purely physical, provided it be much more familiar than that which it is invoked to explain. To Thales and Anaximenes, it appeared inconceivable that the antecedents which we see in nature should produce the consequents; but perfectly natural that water, or air, should produce them. The writers whom I oppose declare this inconceivable, but can conceive that mind, or volition, is *per se* an efficient cause: while the Cartesians could not conceive even that, but peremptorily declared that no mode of production of any fact whatever was conceivable, except the direct agency of an omnipotent being; thus giving additional proof of what finds new confirmation in every stage of the history of science: that both what persons can, and what they can not, conceive, is very much an affair of accident, and depends altogether on their experience, and their habits of thought; that by cultivating the requisite associations of ideas, people may make themselves unable to conceive any given thing; and may make themselves able to conceive most things, however inconceivable these may at first appear; and the same facts in each person's mental history which determine what is or is not conceivable to him, determine also which among the various sequences in nature will appear to him so natural and plausible, as to need no other proof of their existence; to be evident by their own light, independent equally of experience and of explanation.

By what rule is any one to decide between one theory of this description and another? The theorists do not direct us to any external evidence; they appeal each to his own subjective feelings. One says, the succession C B appears to me more natural, conceivable, and credible *per se*, than the succession A B; you are therefore mistaken in thinking that B depends upon A; I am certain, though I can give no other evidence of it, that C comes in between A and B, and is the real and only cause of B. The other answers, the successions C B and A B appear to me equally natural and conceivable, or the latter more so than the former: A is quite capable of producing B without any other intervention. A third agrees with the first in being unable to conceive that A can produce B, but finds the sequence D B still more natural than C B, or of nearer kin to the subject-matter, and prefers his D theory to the C theory. It is plain that there is no universal law operating here, except the law that each person's conceptions are governed and limited by his individual experiences and habits of thought. We are warranted in saying of all three, what each of them already believes of the other two, namely, that they exalt into an original law of the human intellect and of outward nature one particular sequence of phenomena, which appears to them more natural and more conceivable than other sequences, only because it is more familiar. And from this judgment I am unable to except the theory, that Volition is an Efficient Cause.

I am unwilling to leave the subject without adverting to the additional fallacy contained in the corollary from this theory; in the inference that because Volition is an efficient cause, therefore it is the only cause, and the direct agent in producing even what is apparently produced by something else. Volitions are not known to produce any thing directly except nervous action, for the will influences even the muscles only through the nerves. Though it were granted, then, that every phenomenon has an efficient, and not merely a phenomenal cause, and that volition, in the case of the peculiar phenomena which are known to be produced by it, is that efficient cause; are we therefore to say, with these writers, that since we know of no other efficient cause, and ought not to assume one without evidence, there *is* no other, and volition is the direct cause of all phenomena? A more outrageous stretch of inference could hardly be made. Because among the infinite variety of the phenomena of nature there is one, namely, a particular mode of action of certain nerves, which has for its cause, and as we are now supposing for its efficient cause, a state of our mind; and because this is the only efficient cause of which we are conscious, being the only one of which in the nature of the case we *can* be conscious, since it is the only one which exists within ourselves; does this justify us in concluding that all other phenomena must have the same kind of efficient cause with that one eminently special, narrow, and peculiarly human or animal, phenomenon? The nearest parallel to this specimen of generalization is suggested by the recently revived controversy on the old subject of Plurality of Worlds, in which the contending parties have been so conspicuously successful in overthrowing one another. Here also we have experience only of a single case, that of the world in which we live, but that this is inhabited we know absolutely, and without possibility of doubt. Now if on this evidence any one were to infer that every heavenly body without exception, sun, planet, satellite, comet, fixed star or nebula, is inhabited, and must be so from the inherent constitution of things, his inference would exactly resemble that of the writers who conclude that because volition is the efficient cause of our own bodily motions, it must be the efficient cause of every thing else in the universe. It is true there are cases in which, with acknowledged propriety, we generalize from a single instance to a multitude of instances. But they must be instances which resemble the one known instance, and not such as have no circumstance in common with it except that of being instances. I have, for example, no direct evidence that any creature is alive except myself, yet I attribute, with full assurance, life and sensation to other human beings and animals. But I do not conclude that all other things are alive merely because I am. I ascribe to certain other creatures a life like my own, because they manifest it by the same sort of indications by which mine is manifested. I find that their phenomena and mine conform to the same laws, and it is for this reason that I believe both to arise from a similar cause. Accordingly I do not extend the conclusion beyond the grounds for it. Earth, fire, mountains, trees, are remarkable agencies, but their phenomena do not conform to the same laws as my actions do, and I therefore do not believe earth or fire, mountains or trees, to possess animal life. But the supporters of the Volition Theory ask us to infer that volition causes every thing, for no reason except that it causes one particular thing; although that one phenomenon, far from being a type of all natural phenomena, is eminently peculiar; its laws bearing scarcely any resemblance to those of any other phenomenon, whether of inorganic or of organic nature.

NOTE SUPPLEMENTARY TO THE PRECEDING CHAPTER.

The author of the Second Burnett Prize Essay (Dr. Tulloch), who has employed a considerable number of pages in controverting the doctrines of the preceding chapter, has somewhat surprised me by denying a fact, which I imagined too well known to require proof—that there have been philosophers who found in physical explanations of phenomena the same complete mental satisfaction which we are told is only given by volitional explanation, and others who denied the Volitional Theory on the same ground of inconceivability on which it is defended. The assertion of the Essayist is countersigned still more positively by an able reviewer of the Essay:* "Two illustrations," says the reviewer, "are advanced by Mr. Mill: the case of Thales and Anaximenes, stated by him to have maintained, the one Moisture and the other Air to be the origin of all things; and that of Descartes and Leibnitz, whom he asserts to have found the action of Mind upon Matter the grand inconceivability. In counter-statement as to the first of these cases the author shows—what we believe now hardly admits of doubt—that the Greek philosophers distinctly recognized as beyond and above their primal material source, the *νοῦς*, or Divine Intelligence, as the efficient and originating Source of all; and as to the second, by proof that it was the *mode*, not the *fact*, of that action on matter, which was represented as inconceivable."

A greater quantity of historical error has seldom been comprised in a single sentence. With regard to Thales, the assertion that he considered water as a mere material in the hands of *νοῦς* rests on a passage of Cicero *de Naturâ Deorum;* and whoever will refer to any of the accurate historians of philosophy, will find that they treat this as a mere fancy of Cicero, resting on no authority, opposed to all the evidence; and make surmises as to the manner in which Cicero may have been led into the error. (See Ritter, vol. i., p. 211, 2d ed.; Brandis, vol. i., pp. 118–9, 1st ed.; Preller, *Historia Philosophiæ Græco-Romanæ*, p. 10. "Schiefe Ansicht, durchaus zu verwerfen;" "augenscheinlich folgernd statt zu berichten;" "quibus vera sententia Thaletis plane detorquetur," are the expressions of these writers.) As for Anaximenes, he even according to Cicero, maintained, not that air was the material out of which God made the world, but that the air was a god: "Anaximenes aëra deum statuit;" or, according to St. Augustine, that it was the material out of which the gods were made; "non tamen ab ipsis [Diis] aërem factum, sed ipsos ex aëre ortos credidit." Those who are not familiar with the metaphysical terminology of antiquity, must not be misled by finding it stated that Anaximenes attributed *ψυχή* (translated *soul*, or *life*) to his universal element, the air. The Greek philosophers acknowledged several kinds of *ψυχή*, the nutritive, the sensitive, and the intellective.† Even the moderns, with admitted correctness, attribute life to plants. As far as we can make out the meaning of Anaximenes, he made choice of Air as the universal agent, on the ground that it is perpetually in motion, without any apparent cause external to itself: so that he conceived it as exercising spontaneous force, and as the principle of life and activity in all things, men and gods inclusive. If this be not representing it as the Efficient Cause the dispute altogether has no meaning.

If either Anaximenes, or Thales, or any of their contemporaries, had held the doctrine that *νοῦς* was the Efficient Cause, that doctrine could not have been reputed, as it was throughout antiquity, to have originated with Anaxagoras. The testimony of Aristotle, in the first book of his Metaphysics, is perfectly decisive with respect to these early speculations. After enumerating four kinds of causes, or rather four different meanings of the word Cause, viz., the Essence of a thing, the Matter of it, the Origin of Motion (Efficient Cause), and the End or

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* *Westminster Review* for October, 1855.

† See the whole doctrine in Aristotle *de Animâ*, where the *θρεπτική ψυχή* is treated as exactly equivalent to *θρεπτική δύναμις*.

Final Cause, he proceeds to say, that most of the early philosophers recognized only the second kind of Cause, the Matter of a thing, *τὰς ἐν ὕλης εἴδει μόνας ᾠήθησαν ἀρχὰς εἶναι πάντων*. As his first example he specifies Thales, whom he describes as taking the lead in this view of the subject, *ὁ τῆς τοιαύτης ἀρχηγὸς φιλοσοφίας*. and goes on to Hippon, Anaximenes, Diogenes (of Apollonia), Hippasus of Metapontum, Heraclitus, and Empedocles. Anaxagoras, however (he proceeds to say), taught a different doctrine, as we *know*, and it is *alleged* that Hermotimus of Clazomenæ taught it before him. Anaxagoras represented, that even if these various theories of the universal material were true, there would be need of some other cause to account for the transformations of the materials, since the material can not originate its own changes: *οὐ γὰρ δὴ τό γε ὑποκείμενον αὐτὸ ποιεῖ μεταβάλλειν ἑαυτό· λέγω δ' οἷον οὔτε τὸ ξύλον οὔτε ὁ χαλκὸς αἴτιος τοῦ μεταβάλλειν ἑκάτερον αὐτῶν, οὐδὲ ποιεῖ τὸ μὲν ξύλον κλίνην ὁ δὲ χαλκὸς ἀνδριάντα, ἀλλ' ἕτερόν τι τῆς μεταβολῆς αἴτιον*, viz., the other kind of cause, *ὅθεν ἡ ἀρχὴ τῆς κινήσεως*—an Efficient Cause. Aristotle expresses great approbation of this doctrine (which he says made its author appear the only sober man among persons raving, *οἷον νήφων ἐφάνη παρ' εἰκῇ λέγοντας τοὺς πρότερον*); but while describing the influence which it exercised over subsequent speculation, he remarks that the philosophers against whom this, as he thinks, insuperable difficulty was urged, had not felt it to be any difficulty: *οὐδὲν ἐδυσχέραναν ἐν ἑαυτοῖς*. It is surely unnecessary to say more in proof of the matter of fact which Dr. Tulloch and his reviewer disbelieve.

Having pointed out what he thinks the error of these early speculators in not recognizing the need of an efficient cause, Aristotle goes on to mention two other efficient causes to which they might have had recourse, instead of intelligence: *τύχη*, chance, and *τὸ αὐτόματον*, spontaneity. He indeed puts these aside as not sufficiently worthy causes for the order in the universe, *οὐδ' αὖ τῷ αὐτομάτῳ καὶ τῇ τύχῃ τοσοῦτον ἐπιτρέψαι πρᾶγμα καλῶς εἶχεν*; but he does not reject them as incapable of producing *any* effect, but only as incapable of producing *that* effect. He himself recognizes *τύχη* and *τὸ αὐτόματον* as co-ordinate agents with Mind in producing the phenomena of the universe; the department allotted to them being composed of all the classes of phenomena which are not supposed to follow any uniform law. By thus including Chance among efficient causes, Aristotle fell into an error which philosophy has now outgrown, but which is by no means so alien to the spirit even of modern speculation as it may at first sight appear. Up to quite a recent period philosophers went on ascribing, and many of them have not yet ceased to ascribe, a real existence to the results of abstraction. Chance could make out as good a title to that dignity as many other of the mind's abstract creations: it had had a name given to it, and why should it not be a reality? As for *τὸ αὐτόματον*, it is recognized even yet as one of the modes of origination of phenomena by all those thinkers who maintain what is called the Freedom of the Will. The same self-determining power which that doctrine attributes to volitions, was supposed by the ancients to be possessed also by some other natural phenomena: a circumstance which throws considerable light on more than one of the supposed invincible necessities of belief. I have introduced it here, because this belief of Aristotle, or rather of the Greek philosophers generally, is as fatal as the doctrines of Thales and the Ionic school to the theory that the human mind is compelled by its constitution to conceive volition as the origin of all force, and the efficient cause of all phenomena.*

* It deserves notice that the parts of nature which Aristotle regards as representing evidence of design, are the Uniformities: the phenomena in so far as reducible to law. *Τύχη* and *τὸ αὐτόματον* satisfy him as explanations of the variable element in phenomena, but their occurring according to a fixed rule can only, to his conceptions, be accounted for by an Intelligent Will. The common, or what may be called the instinctive, religious interpretation of nature, is the reverse of this. The events in which men spontaneously see the hand of a supernatural being, are those which can not, as they think, be reduced to a physical law. What they can distinctly connect with physical causes, and especially what they can predict, though of course ascribed to an Author of Nature, if they already recognize such an author, might be conceived, they think, to arise from a blind fatality, and in any case do not appear to them to bear so obviously the mark of a divine will. And this distinction has been countenanced by eminent writers on Natural Theology, in particular by Dr. Chalmers, who thinks that though design is present everywhere, the irresistible evidence of it is to be found not in the *laws* of nature but in the collocations, *i. e.*, in the part of nature in which it is impossible to trace any law. A few properties of dead matter might, he thinks, conceivably account for the regular and invariable succession of effects and causes; but that the different kinds of matter have been so placed as to promote beneficent ends, is what he regards at the proof of a Divine Providence. Mr. Baden Powell, in his Essay entitled "Philosophy of Creation," has returned to the point of view of Aristotle and the ancients, and vigorously re-asserts the doctrine that the indication of design in the universe is not special adaptations, but Uniformity and Law, these being the evidences of mind, and not what appears to us to be a provision

With regard to the modern philosophers (Leibnitz and the Cartesians) whom I had cited as having maintained that the action of mind upon matter, so far from being the only conceivable origin of material phenomena, is itself inconceivable; the attempt to rebut this argument by asserting that the mode, not the fact, of the action of mind on matter was represented as inconceivable, is an abuse of the privilege of writing confidently about authors without reading them; for any knowledge whatever of Leibnitz would have taught those who thus speak of him, that the inconceivability of the mode, and the impossibility of the thing, were in his mind convertible expressions. What was his famous Principle of the Sufficient Reason, the very corner-stone of his Philosophy, from which the Pre-established Harmony, the doctrine of Monads, and all the opinions most characteristic of Leibnitz, were corollaries? It was, that nothing exists, the existence of which is not capable of being proved and explained *a priori;* the proof and explanation in the case of contingent facts being derived from the nature of their causes; which could not be the causes unless there was something in their nature showing them to be capable of producing those particular effects. And this "something" which accounts for the production of physical effects, he was able to find in many physical causes, but could not find it in any finite minds, which therefore he unhesitatingly asserted to be incapable of producing any physical effects whatever. "On ne saurait concevoir," he says, "une action réciproque de la matière et de l'intelligence l'une sur l'autre," and there is therefore (he contends) no choice but between the Occasional Causes of the Cartesians and his own Pre-established Harmony, according to which there is no more connection between our volitions and our muscular actions than there is between two clocks which are wound up to strike at the same instant. But he felt no similar difficulty as to physical causes; and throughout his speculations, as in the passage I have already cited respecting gravitation, he distinctly refuses to consider as part of the order of nature any fact which is not explicable from the nature of its physical cause.

With regard to the Cartesians (not Descartes; I did not make that mistake, though the reviewer of Dr. Tulloch's Essay attributes it to me) I take a passage almost at random from Malebranche, who is the best known of the Cartesians, and, though not the inventor of the system of Occasional Causes, is its principal expositor. In Part II., chap. iii., of his Sixth Book, having first said that matter can not have the power of moving itself, he proceeds to argue that neither can mind have the power of moving it. "Quand on examine l'idée que l'on a de tous les esprits finis, on ne voit point de liaison nécessaire entre leur volonté et le mouvement de quelque corps que ce soit, on voit au contraire qu'il n'y en a point, et qu'il n'y en peut avoir" (there is nothing in the idea of finite mind which can account for its causing the motion of a body); "on doit aussi conclure, si on veut raisonner selon ses lumières, qu'il n'y a aucun esprit créé qui puisse remuer quelque corps que ce soit comme cause véritable ou principale, de même que l'on a dit qu'aucun corps ne se pouvait remuer soi-même:" thus the idea of Mind is according to him as incompatible as the idea of Matter with the exercise of active force. But when, he continues, we consider not a created but a Divine Mind, the case is altered; for the idea of a Divine Mind includes omnipotence; and the idea of omnipotence does contain the idea of being able to move bodies. Thus it is the nature of omnipotence which renders the motion of bodies even by the Divine Mind credible or conceivable, while, so far as depended on the mere nature of mind, it would have been inconceivable and incredible. If Malebranche had not believed in an omnipotent Being, he would have held all action of mind on body to be a demonstrated impossibility.*

A doctrine more precisely the reverse of the Volitional theory of causation can not well be imagined. The Volitional theory is, that we know by intuition or by direct experience the action of our own mental volitions on matter; that we may hence infer all other action upon matter to be that of volition, and might thus know, without any other evidence, that matter is under the government of a Divine Mind. Leibnitz and the Cartesians, on the contrary, maintain that our volitions do not and can not act upon matter, and that it is only the existence of an all-governing Being, and that Being omnipotent, which can account for the sequence between our volitions and our bodily actions. When we consider that each of these two theories, which, as theories of causation, stand at the opposite extremes of possible divergence

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for our uses. While I decline to express any opinion here on this *vexata quæstio,* I ought not to mention Mr. Powell's volume without the acknowledgment due to the philosophic spirit which pervades generally the three Essays composing it, forming in the case of one of them (the "Unity of Worlds") an honorable contrast with the other dissertations, so far as they have come under my notice, which have appeared on either side of that controversy.

* In the words of Fontenelle, another celebrated Cartesian, "les philosophes aussi bien que le peuple avaient cru que l'âme et le corps agissaient réellement et physiquement l'un sur l'autre. Descartes vint, qui prouva que leur nature ne permettait point cette sorte de communication véritable, et qu'ils n'en pouvaient avoir qu'une apparente, dont Dieu était le Médiateur."—(*Œuvres de Fontenelle*, ed. 1767, tom. v., p. 534.)

from one another, invokes not only as its evidence, but as its sole evidence, the absolute inconceivability of any theory but itself, we are enabled to measure the worth of this kind of evidence: and when we find the Volitional theory entirely built upon the assertion that by our mental constitution we are compelled to recognize our volitions as efficient causes, and then find other thinkers maintaining that we know that they are not and can not be such causes, and can not conceive them to be so, I think we have a right to say that this supposed law of our mental constitution does not exist.

Dr. Tulloch (pp. 45-47) thinks it a sufficient answer to this, that Leibnitz and the Cartesians were Theists, and believed the will of God to be an efficient cause. Doubtless they did, and the Cartesians even believed (though Leibnitz did not) that it is the only such cause. Dr. Tulloch mistakes the nature of the question. I was not writing on Theism, as Dr. Tulloch is, but against a particular theory of causation, which, if it be unfounded, can give no effective support to Theism or to any thing else. I found it asserted that volition is the only efficient cause, on the ground that no other efficient cause is conceivable. To this assertion I oppose the instances of Leibnitz and of the Cartesians, who affirmed with equal positiveness that volition as an efficient cause is itself not conceivable, and that omnipotence, which renders all things conceivable, can alone take away the impossibility. This I thought, and think, a conclusive answer to the argument on which this theory of causation avowedly depends. But I certainly did not imagine that Theism was bound up with that theory; nor expected to be charged with denying Leibnitz and the Cartesians to be Theists because I denied that they held the theory.

CHAPTER VI.

ON THE COMPOSITION OF CAUSES.

§ 1. To complete the general notion of causation on which the rules of experimental inquiry into the laws of nature must be founded, one distinction still remains to be pointed out: a distinction so radical, and of so much importance, as to require a chapter to itself.

The preceding discussions have rendered us familiar with the case in which several agents, or causes, concur as conditions to the production of an effect; a case, in truth, almost universal, there being very few effects to the production of which no more than one agent contributes. Suppose, then, that two different agents, operating jointly, are followed, under a certain set of collateral conditions, by a given effect. If either of these agents, instead of being joined with the other, had operated alone, under the same set of conditions in all other respects, some effect would probably have followed, which would have been different from the joint effect of the two, and more or less dissimilar to it. Now, if we happen to know what would be the effect of each cause when acting separately from the other, we are often able to arrive deductively, or *a priori*, at a correct prediction of what will arise from their conjunct agency. To render this possible, it is only necessary that the same law which expresses the effect of each cause acting by itself, shall also correctly express the part due to that cause of the effect which follows from the two together. This condition is realized in the extensive and important class of phenomena commonly called mechanical, namely the phenomena of the communication of motion (or of pressure, which is tendency to motion) from one body to another. In this important class of cases of causation, one cause never, properly speaking, defeats or frustrates another; both have their full effect. If a body is propelled in two directions by two forces, one tending to drive it to the north and the other to the east, it is caused to move in a given time exactly as far in both directions as the two forces would separately have carried it; and is left precisely where it would have arrived if it had been acted upon first by one of the two forces, and afterward by the other. This law of nature is called, in dynamics, the principle of the Composition of Forces; and in imitation of that well-chosen expression, I shall give the name of the Composition of Causes to the principle which is exemplified in all cases in which the joint effect of several causes is identical with the sum of their separate effects.

This principle, however, by no means prevails in all departments of the field of nature. The chemical combination of two substances produces, as is well known, a third substance, with properties different from those of either of the two substances separately, or of both of them taken together. Not a trace of the properties of hydrogen or of oxygen is observable in those of their compound, water. The taste of sugar of lead is not the sum of the tastes of its component elements, acetic acid and lead or its oxide; nor is the color of blue vitriol a mixture of the colors of sulphuric acid and copper. This explains why mechanics is a deductive or demonstrative science, and chemistry not. In the one, we can compute the effects of combinations of causes, whether real or hypothetical, from the laws which we know to govern those causes when acting separately, because they continue to observe the same laws when in combination which they observe when separate: whatever would have happened in consequence of each cause taken by itself, happens when they are together, and we have only to cast up the results. Not so in the phenomena which are the peculiar subject of the science of chemistry. There most of the uniformities to which the causes conform when separate, cease altogether when they are conjoined; and we are not, at least in the present state of our knowledge, able to foresee what result will follow from any new combination until we have tried the specific experiment.

If this be true of chemical combinations, it is still more true of those far more complex combinations of elements which constitute organized bodies; and in which those extraordinary new uniformities arise which are called the laws of life. All organized bodies are composed of parts similar to those composing inorganic nature, and which have even themselves existed in an inorganic state; but the phenomena of life, which result from the juxtaposition of those parts in a certain manner, bear no analogy to any of the effects which would be produced by the action of the component substances considered as mere physical agents. To whatever degree we might imagine our knowledge of the properties of the several ingredients of a living body to be extended and perfected, it is certain that no mere summing up of the separate actions of those elements will ever amount to the action of the living body itself. The tongue, for instance, is, like all other parts of the animal frame, composed of gelatine, fibrine, and other products of the chemistry of digestion; but from no knowledge of the properties of those substances could we ever predict that it could taste, unless gelatine or fibrine could themselves taste; for no elementary fact can be in the conclusion which was not in the premises.

There are thus two different modes of the conjunct action of causes; from which arise two modes of conflict, or mutual interference, between laws of nature. Suppose, at a given point of time and space, two or more causes, which, if they acted separately, would produce effects contrary, or at least conflicting with each other; one of them tending to undo, wholly or partially, what the other tends to do. Thus the expansive force of the gases generated by the ignition of gunpowder tends to project a bullet toward the sky, while its gravity tends to make it fall to the ground. A stream running into a reservoir at one end tends to fill it higher and higher, while a drain at the other extremity tends to empty it. Now, in such cases as these, even if the two causes which are in joint action exactly annul one another, still the laws of both are fulfilled; the effect is the same as if the drain had been open for half an hour first,* and the stream had flowed in for as long afterward. Each agent produces the same amount of effect as if it had acted separately, though the contrary effect which was taking place during the same time obliterated it as fast as it was produced. Here, then, are two causes, producing by their joint operations an effect which at first seems quite dissimilar to those which they produce separately, but which on examination proves to be really the sum of those separate effects. It will be noticed that we here enlarge the idea of the sum of two effects, so as to include what is commonly called their difference, but which is in reality the result of the addition of opposites; a conception to which mankind are indebted for that admirable extension of the algebraical calculus, which has so vastly increased its powers as an instrument of discovery, by introducing into its reasonings (with the sign of subtraction prefixed, and under the name of Negative Quantities) every description whatever of positive phenomena, provided they are of such a quality in reference to those previously introduced, that to add the one is equivalent to subtracting an equal quantity of the other.

There is, then, one mode of the mutual interference of laws of nature, in which, even when the concurrent causes annihilate each other's effects, each exerts its full efficacy according to its own law—its law as a separate agent. But in the other description of cases, the agencies which are brought together cease entirely, and a totally different set of phenomena arise: as in the experiment of two liquids which, when mixed in certain proportions, instantly become, not a larger amount of liquid, but a solid mass.

§ 2. This difference between the case in which the joint effect of causes is the sum of their separate effects, and the case in which it is heterogeneous to them—between laws which work together without alteration, and laws which, when called upon to work together, cease and give place to others—is one of the fundamental distinctions in nature. The former case, that of the Composition of Causes, is the general one; the other is always special and exceptional. There are no objects which do not, as to some of their phenomena, obey the principle of the Composition of Causes; none that have not some laws which are rigidly fulfilled in every combination into which the objects enter. The weight of a body, for instance, is a property which it retains in all the combinations in which it is placed. The weight of a chemical compound, or of an organized body, is equal to the sum of the weights of the elements which compose it. The weight either of the elements or of the compound will vary, if they be carried farther from their centre of attraction, or brought nearer to it; but whatever effects the one effects the other. They always remain precisely equal. So, again, the component parts of a vegetable or animal substance do not lose their mechanical and chemical properties as separate agents, when, by a peculiar mode of juxtaposition, they, as an aggregate whole, acquire physiological or vital properties in addition. Those bodies continue, as before,

* I omit, for simplicity, to take into account the effect, in this latter case, of the diminution of pressure, in diminishing the flow of water through the drain; which evidently in no way affects the truth or applicability of the principle, since when the two causes act simultaneously the conditions of that diminution of pressure do not arise.

to obey mechanical and chemical laws, in so far as the operation of those laws is not counteracted by the new laws which govern them as organized beings; when, in short, a concurrence of causes takes place which calls into action new laws bearing no analogy to any that we can trace in the separate operation of the causes, the new laws, while they supersede one portion of the previous laws, may co-exist with another portion, and may even compound the effect of those previous laws with their own.

Again, laws which were themselves generated in the second mode, may generate others in the first. Though there are laws which, like those of chemistry and physiology, owe their existence to a breach of the principle of Composition of Causes, it does not follow that these peculiar, or, as they might be termed, *heteropathic* laws, are not capable of composition with one another. The causes which by one combination have had their laws altered, may carry their new laws with them unaltered into their ulterior combinations. And hence there is no reason to despair of ultimately raising chemistry and physiology to the condition of deductive sciences; for though it is impossible to deduce all chemical and physiological truths from the laws or properties of simple substances or elementary agents, they may possibly be deducible from laws which commence when these elementary agents are brought together into some moderate number of not very complex combinations. The Laws of Life will never be deducible from the mere laws of the ingredients, but the prodigiously complex Facts of Life may all be deducible from comparatively simple laws of life; which laws (depending indeed on combinations, but on comparatively simple combinations, of antecedents) may, in more complex circumstances, be strictly compounded with one another, and with the physical and chemical laws of the ingredients. The details of the vital phenomena, even now, afford innumerable exemplifications of the Composition of Causes; and in proportion as these phenomena are more accurately studied, there appears more reason to believe that the same laws which operate in the simpler combinations of circumstances do, in fact, continue to be observed in the more complex. This will be found equally true in the phenomena of mind; and even in social and political phenomena, the results of the laws of mind. It is in the case of chemical phenomena that the least progress has yet been made in bringing the special laws under general ones from which they may be deduced; but there are even in chemistry many circumstances to encourage the hope that such general laws will hereafter be discovered. The different actions of a chemical compound will never, undoubtedly, be found to be the sums of the actions of its separate elements; but there may exist, between the properties of the compound and those of its elements, some constant relation, which, if discoverable by a sufficient induction, would enable us to foresee the sort of compound which will result from a new combination before we have actually tried it, and to judge of what sort of elements some new substance is compounded before we have analyzed it. The law of definite proportions, first discovered in its full generality by Dalton, is a complete solution of this problem in one, though but a secondary aspect, that of quantity; and in respect to quality, we have already some partial generalizations, sufficient to indicate the possibility of ultimately proceeding farther. We can predicate some common properties of the kind of compounds which result from the combination, in each of the small number of possible proportions, of any acid whatever with any base. We have also the curious law, discovered by Berthollet, that two soluble salts mutually decompose one another whenever the new combinations which result produce an insoluble compound, or one less soluble than the two former. Another uniformity is that called the law of isomorphism; the identity of the crystalline forms of substances which possess in common certain peculiarities of chemical composition.* Thus it appears that even heteropathic laws, such laws of combined agency as are not compounded of the laws of the separate agencies, are yet, at least in some cases, derived from them according to a fixed principle. There may, therefore, be laws of the generation of laws from others dissimilar to them; and in chemistry, these undiscovered laws of the dependence of the properties of the compound on the properties of its elements, may, together with the laws of the elements themselves, furnish the premises by which the science is perhaps destined one day to be rendered deductive.

It would seem, therefore, that there is no class of phenomena in which the Composition of Causes does not obtain: that as a general rule, causes in combination produce exactly the same effects as when acting singly: but that this rule, though general, is not universal: that in some instances, at some particular points in the transition from separate to united action, the laws change, and an entirely new set of effects are either added to, or take the place of, those which arise from the separate agency of the same causes: the laws of these new effects being again susceptible of composition, to an indefinite extent, like the laws which they superseded.

§ 3. That effects are proportional to their causes is laid down by some writers as an axiom in the theory of causation; and great use is sometimes made of this principle in reasonings respecting the laws of nature, though it is encumbered with many difficulties and apparent exceptions, which much ingenuity has been expended in showing not to be real ones. This proposition, in so far as it is true, enters as a particular case into the general principle of the Composition of Causes; the causes compounded being, in this instance, homogeneous; in which case, if in any, their joint effect might be expected to be identical with the sum of their separate effects. If a force equal to one hundred weight will raise a certain body along an inclined plane, a force equal to two hundred weight will raise two bodies exactly similar, and thus the effect is proportional to the cause. But does not a force equal to two hundred weight actually contain in itself two forces each equal to one hundred weight, which, if employed apart, would separately raise the two bodies in question? The fact, therefore, that when exerted jointly they raise both bodies at once, results from the Composition of Causes, and is a mere instance of the general fact that mechanical forces are subject to the law of Composition. And so in every other case which can be supposed. For the doctrine of the proportionality of effects to their causes can not of course be applicable to cases in which the augmentation of the cause alters the *kind* of effect; that is, in which the surplus quantity superadded to the cause does not become compounded with it, but the two together generate an altogether new phenomenon. Suppose that the application of a certain quantity of heat to a body merely increases its bulk, that a double quantity melts it, and a triple quantity decomposes it: these three effects being heterogeneous, no ratio, whether corresponding

* Professor Bain adds several other well-established chemical generalizations: "The laws that simple substances exhibit the strongest affinities; that compounds are more fusible than their elements; that combination tends to a lower state of matter from gas down to solid;" and some general propositions concerning the circumstances which facilitate or resist chemical combination. (Logic, ii., 254.)

or not to that of the quantities of heat applied, can be established between them. Thus the supposed axiom of the proportionality of effects to their causes fails at the precise point where the principle of the Composition of Causes also fails; viz., where the concurrence of causes is such as to determine a change in the properties of the body generally, and render it subject to new laws, more or less dissimilar to those to which it conformed in its previous state. The recognition, therefore, of any such law of proportionality is superseded by the more comprehensive principle, in which as much of it as is true is implicitly asserted.*

The general remarks on causation, which seemed necessary as an introduction to the theory of the inductive process, may here terminate. That process is essentially an inquiry into cases of causation. All the uniformities which exist in the succession of phenomena, and most of the uniformities in their co-existence, are either, as we have seen, themselves laws of causation, or consequences resulting from, and corollaries capable of being deduced from, such laws. If we could determine what causes are correctly assigned to what effects, and what effects to what causes, we should be virtually acquainted with the whole course of nature. All those uniformities which are mere results of causation might then be explained and accounted for; and every individual fact or event might be predicted, provided we had the requisite data, that is, the requisite knowledge of the circumstances which, in the particular instance, preceded it.

To ascertain, therefore, what are the laws of causation which exist in nature; to determine the effect of every cause, and the causes of all effects, is the main business of Induction; and to point out how this is done is the chief object of Inductive Logic.

* Professor Bain (Logic, ii., 39) points out a class of cases, other than that spoken of in the text, which he thinks must be regarded as an exception to the Composition of Causes. "Causes that merely make good the collocation for bringing a prime mover into action, or that release a potential force, do not follow any such rule. One man may direct a gun upon a fort as well as three: two sparks are not more effectual than one in exploding a barrel of gunpowder. In medicine there is a certain dose that answers the end; and adding to it does no more good."

I am not sure that these cases are really exceptions. The law of Composition of Causes, I think, is really fulfilled, and the appearance to the contrary is produced by attending to the remote instead of the immediate effect of the causes. In the cases mentioned, the immediate effect of the causes in action is a collocation, and the duplication of the cause does double the quantity of collocation. Two men could raise the gun to the required angle twice as quickly as one, though one is enough. Two sparks put two sets of particles of the gunpowder into the state of intestine motion which makes them explode, though one is sufficient. It is the collocation itself that does not, by being doubled, always double the effect; because in many cases a certain collocation, once obtained, is all that is required for the production of the whole amount of effect which can be produced at all at the given time and place. Doubling the collocation with difference of time and place, as by pointing two guns, or exploding a second barrel after the first, does double the effect. This remark applies still more to Mr. Bain's third example, that of a double dose of medicine; for a double dose of an aperient does purge more violently, and a double dose of laudanum does produce longer and sounder sleep. But a double purging, or a double amount of narcotism, may have remote effects different in kind from the effect of the smaller amount, reducing the case to that of heteropathic laws, discussed in the text.

CHAPTER VII.

OF OBSERVATION AND EXPERIMENT.

§ 1. It results from the preceding exposition, that the process of ascertaining what consequents, in nature, are invariably connected with what antecedents, or in other words what phenomena are related to each other as causes and effects, is in some sort a process of analysis. That every fact which begins to exist has a cause, and that this cause must be found in some fact or concourse of facts which immediately preceded the occurrence, may be taken for certain. The whole of the present facts are the infallible result of all past facts, and more immediately of all the facts which existed at the moment previous. Here, then, is a great sequence, which we know to be uniform. If the whole prior state of the entire universe could again recur, it would again be followed by the present state. The question is, how to resolve this complex uniformity into the simpler uniformities which compose it, and assign to each portion of the vast antecedent the portion of the consequent which is attendant on it.

This operation, which we have called analytical, inasmuch as it is the resolution of a complex whole into the component elements, is more than a merely mental analysis. No mere contemplation of the phenomena, and partition of them by the intellect alone, will of itself accomplish the end we have now in view. Nevertheless, such a mental partition is an indispensable first step. The order of nature, as perceived at a first glance, presents at every instant a chaos followed by another chaos. We must decompose each chaos into single facts. We must learn to see in the chaotic antecedent a multitude of distinct antecedents, in the chaotic consequent a multitude of distinct consequents. This, supposing it done, will not of itself tell us on which of the antecedents each consequent is invariably attendant. To determine that point, we must endeavor to effect a separation of the facts from one another, not in our minds only, but in nature. The mental analysis, however, must take place first. And every one knows that in the mode of performing it, one intellect differs immensely from another. It is the essence of the act of observing; for the observer is not he who merely sees the thing which is before his eyes, but he who sees what parts that thing is composed of. To do this well is a rare talent. One person, from inattention, or attending only in the wrong place, overlooks half of what he sees; another sets down much more than he sees, confounding it with what he imagines, or with what he infers; another takes note of the *kind* of all the circumstances, but being inexpert in estimating their degree, leaves the quantity of each vague and uncertain; another sees indeed the whole, but makes such an awkward division of it into parts, throwing things into one mass which require to be separated, and separating others which might more conveniently be considered as one, that the result is much the same, sometimes even worse, than if no analysis had been attempted at all. It would be possible to point out what qualities of mind, and modes of mental culture, fit a person for being a good observer: that, however, is a question not of Logic, but of the Theory of Education, in the most enlarged sense of the term. There is not properly an Art of Observing.

There may be rules for observing. But these, like rules for inventing, are properly instructions for the preparation of one's own mind; for putting it into the state in which it will be most fitted to observe, or most likely to invent. They are, therefore, essentially rules of self-education, which is a different thing from Logic. They do not teach how to do the thing, but how to make ourselves capable of doing it. They are an art of strengthening the limbs, not an art of using them.

The extent and minuteness of observation which may be requisite, and the degree of decomposition to which it may be necessary to carry the mental analysis, depend on the particular purpose in view. To ascertain the state of the whole universe at any particular moment is impossible, but would also be useless. In making chemical experiments, we do not think it necessary to note the position of the planets; because experience has shown, as a very superficial experience is sufficient to show, that in such cases that circumstance is not material to the result: and accordingly, in the ages when men believed in the occult influences of the heavenly bodies, it might have been unphilosophical to omit ascertaining the precise condition of those bodies at the moment of the experiment. As to the degree of minuteness of the mental subdivision, if we were obliged to break down what we observe into its very simplest elements, that is, literally into single facts, it would be difficult to say where we should find them; we can hardly ever affirm that our divisions of any kind have reached the ultimate unit. But this, too, is fortunately unnecessary. The only object of the mental separation is to suggest the requisite physical separation, so that we may either accomplish it ourselves, or seek for it in nature; and we have done enough when we have carried the subdivision as far as the point at which we are able to see what observations or experiments we require. It is only essential, at whatever point our mental decomposition of facts may for the present have stopped, that we should hold ourselves ready and able to carry it further as occasion requires, and should not allow the freedom of our discriminating faculty to be imprisoned by the swathes and bands of ordinary classification; as was the case with all early speculative inquirers, not excepting the Greeks, to whom it seldom occurred that what was called by one abstract name might, in reality, be several phenomena, or that there was a possibility of decomposing the facts of the universe into any elements but those which ordinary language already recognized.

§ 2. The different antecedents and consequents being, then, supposed to be, so far as the case requires, ascertained and discriminated from one another, we are to inquire which is connected with which. In every instance which comes under our observation, there are many antecedents and many consequents. If those antecedents could not be severed from one another except in thought, or if those consequents never were found apart, it would be impossible for us to distinguish (*a posteriori* at least) the real laws, or to assign to any cause its effect, or to any effect its cause. To do so, we must be able to meet with some of the antecedents apart from the rest, and observe what follows from them; or some of the consequents, and observe by what they are preceded. We must, in short, follow the Baconian rule of *varying the circumstances*. This is, indeed, only the first rule of physical inquiry, and not, as some have thought, the sole rule; but it is the foundation of all the rest.

For the purpose of varying the circumstances, we may have recourse (according to a distinction commonly made) either to observation or to experiment; we may either *find* an instance in nature suited to our purposes, or, by an artificial arrangement of circumstances, *make* one. The value of the instance depends on what it is in itself, not on the mode in which it is obtained: its employment for the purposes of induction depends on the same principles in the one case and in the other; as the uses of money are the same whether it is inherited or acquired. There is, in short, no difference in kind, no real logical distinction, between the two processes of investigation. There are, however, practical distinctions to which it is of considerable importance to advert.

§ 3. The first and most obvious distinction between Observation and Experiment is, that the latter is an immense extension of the former. It not only enables us to produce a much greater number of variations in the circumstances than nature spontaneously offers, but also, in thousands of cases, to produce the precise *sort* of variation which we are in want of for discovering the law of the phenomenon; a service which nature, being constructed on a quite different scheme from that of facilitating our studies, is seldom so friendly as to bestow upon us. For example, in order to ascertain what principle in the atmosphere enables it to sustain life, the variation we require is that a living animal should be immersed in each component element of the atmosphere separately. But nature does not supply either oxygen or azote in a separate state. We are indebted to artificial experiment for our knowledge that it is the former, and not the latter, which supports respiration; and for our knowledge of the very existence of the two ingredients.

Thus far the advantage of experimentation over simple observation is universally recognized: all are aware that it enables us to obtain innumerable combinations of circumstances which are not to be found in nature, and so add to nature's experiments a multitude of experiments of our own. But there is another superiority (or, as Bacon would have expressed it, another prerogative) of instances artificially obtained over spontaneous instances—of our own experiments over even the same experiments when made by nature—which is not of less importance, and which is far from being felt and acknowledged in the same degree.

When we can produce a phenomenon artificially, we can take it, as it were, home with us, and observe it in the midst of circumstances with which in all other respects we are accurately acquainted. If we desire to know what are the effects of the cause A, and are able to produce A by means at our disposal, we can generally determine at our own discretion, so far as is compatible with the nature of the phenomenon A, the whole of the circumstances which shall be present along with it: and thus, knowing exactly the simultaneous state of every thing else which is within the reach of A's influence, we have only to observe what alteration is made in that state by the presence of A.

For example, by the electric machine we can produce, in the midst of known circumstances, the phenomena which nature exhibits on a grander scale in the form of lightning and thunder. Now let any one consider what amount of knowledge of the effects and laws of electric agency mankind could have obtained from the mere observation of thunder-storms, and compare it with that which they have gained, and may expect to gain, from electrical and galvanic experiments. This example is the more striking, now that we have reason to believe that electric action is of all natural phenomena (except heat) the most pervading and universal, which, therefore, it might antecedently have been supposed could stand least in need of artificial means of production to enable it to be studied; while the fact is so much the contrary, that without the electric machine, the Leyden jar, and the voltaic battery, we probably should never have suspected the existence of electricity as one of the great agents in nature; the few electric phenomena we should have known of would have continued to be regarded either as supernatural, or as a sort of anomalies and eccentricities in the order of the universe.

When we have succeeded in insulating the phenomenon which is the subject of inquiry, by placing it among known circumstances, we may produce further variations of circumstances to any extent, and of such kinds as we think best calculated to bring the laws of the phenomenon into a clear light. By introducing one well-defined circumstance after another into the experiment, we obtain assurance of the manner in which the phenomenon behaves under an indefinite variety of possible circumstances. Thus, chemists, after having obtained some newly-discovered substance in a pure state (that is, having made sure that there is nothing present which can interfere with and modify its agency), introduce various other substances, one by one, to ascertain whether it will combine with them, or decompose them, and with what result; and also apply heat, or electricity, or pressure, to discover what will happen to the substance under each of these circumstances.

But if, on the other hand, it is out of our power to produce the phenomenon, and we have to seek for instances in which nature produces it, the task before us is very different.

Instead of being able to choose what the concomitant circumstances shall be, we now have to discover what they are; which, when we go beyond the simplest and most accessible cases, it is next to impossible to do with any precision and completeness. Let us take, as an exemplification of a phenomenon which we have no means of fabricating artificially, a human mind. Nature produces many; but the consequence of our not being able to produce them by art is, that in every instance in which we see a human mind developing itself, or acting upon other things, we see it surrounded and obscured by an indefinite multitude of unascertainable circumstances, rendering the use of the common experimental methods almost delusive. We may conceive to what extent this is true, if we consider, among other things, that whenever Nature produces a human mind, she produces, in close connection with it, a body; that is, a vast complication of physical facts, in no two cases perhaps exactly similar, and most of which (except the mere structure, which we can examine in a sort of coarse way after it has ceased to act), are radically out of the reach of our means of exploration. If, instead of a human mind, we suppose the subject of investigation to be a human society or State, all the same difficulties recur in a greatly augmented degree.

We have thus already come within sight of a conclusion, which the progress of the inquiry will, I think, bring before us with the clearest evidence: namely, that in the sciences which deal with phenomena in which artificial experiments are impossible (as in the case of astronomy), or in which they have a very limited range (as in mental philosophy, social science, and even physiology), induction from direct experience is practiced at a disadvantage in most cases equivalent to impracticability; from which it follows that the methods of those sciences, in order to accomplish any thing worthy of attainment, must be to a great extent, if not principally, deductive. This is already known to be the case with the first of the sciences we have mentioned, astronomy; that it is not generally recognized as true of the others, is probably one of the reasons why they are not in a more advanced state.

§ 4. If what is called pure observation is at so great a disadvantage, compared with artificial experimentation, in one department of the direct exploration of phenomena, there is another branch in which the advantage is all on the side of the former.

Inductive inquiry having for its object to ascertain what causes are connected with what effects, we may begin this search at either end of the road which leads from the one point to the other: we may either inquire into the effects of a given cause or into the causes of a given effect. The fact that light blackens chloride of silver might have been discovered either by experiments on light, trying what effect it would produce on various substances, or by observing that portions of the chloride had repeatedly become black, and inquiring into the circumstances. The effect of the urali poison might have become known either by administering it to animals, or by examining how it happened that the wounds which the Indians of Guiana inflict with their arrows prove so uniformly mortal. Now it is manifest from the mere statement of the examples, without any theoretical discussion, that artificial experimentation is applicable only to the former of these modes of investigation. We can take a cause, and try what it will produce; but we can not take an effect, and try what it will be produced by. We can only watch till we see it produced, or are enabled to produce it by accident.

This would be of little importance, if it always depended on our choice from which of the two ends of the sequence we would undertake our inquiries. But we have seldom any option. As we can only travel from the known to the unknown, we are obliged to commence at whichever end we are best acquainted with. If the agent is more familiar to us than its effects, we watch for, or contrive, instances of the agent, under such varieties of circumstances as are open to us, and observe the result. If, on the contrary, the conditions on which a phenomenon depends are obscure, but the phenomenon itself familiar, we must commence our inquiry from the effect. If we are struck with the fact that chloride of silver has been blackened, and have no suspicion of the cause, we have no resource but to compare instances in which the fact has chanced to occur, until by that comparison we discover that in all those instances the substances had been exposed to light. If we knew nothing of the Indian arrows but their fatal effect, accident alone could turn our attention to experiments on the urali; in the regular course of investigation, we could only inquire, or try to observe, what had been done to the arrows in particular instances.

Wherever, having nothing to guide us to the cause, we are obliged to set out from the effect, and to apply the rule of varying the circumstances to the consequents, not the antecedents, we are necessarily destitute of the resource of artificial experimentation. We can not, at our choice, obtain consequents, as we can antecedents, under any set of circumstances compatible with their nature. There are no means of producing effects but through their causes, and by the supposition the causes of the effect in question are not known to us. We have, therefore, no expedient but to study it where it offers itself spontaneously. If nature happens to present us with instances sufficiently varied in their circumstances, and if we are able to discover, either among the proximate antecedents or among some other order of antecedents, something which is always found when the effect is found, however various the circumstances, and never found when it is not, we may discover, by mere observation without experiment, a real uniformity in nature.

But though this is certainly the most favorable case for sciences of pure observation, as contrasted with those in which artificial experiments are possible, there is in reality no case which more strikingly illustrates the inherent imperfection of direct induction when not founded on experimentation. Suppose that, by a comparison of cases of the effect, we have found an antecedent which appears to be, and perhaps is, invariably connected with it: we have not yet proved that antecedent to be the cause until we have reversed the process, and produced the effect by means of that antecedent. If we can produce the antecedent artificially, and if, when we do so, the effect follows, the induction is complete; that antecedent is the cause of that consequent.* But we have then added the evidence of experiment to that of simple observation. Until we had done so, we had only proved *invariable* antecedence within the limits of experience, but not *unconditional* antecedence, or causation. Until it had been shown by the actual production of the antecedent under known circumstances, and the occurrence thereupon of the consequent, that the antecedent was really the condition on which it depended; the uniformity of succession which was proved to exist between them might, for aught we knew, be (like the succession of day and night) not a case of causation at all; both antecedent and consequent might be successive stages of the effect of an ulterior cause. Observation, in short, without experiment (supposing no aid from deduction) can ascertain sequences and co-existences, but can not prove causation.

In order to see these remarks verified by the actual state of the sciences, we have only to think of the condition of natural history. In zoology, for example, there is an immense number of uniformities ascertained, some of co-existence, others of succession, to many of which, notwithstanding considerable variations of the attendant circumstances, we know not any exception: but the antecedents, for the most part, are such as we can not artificially produce; or if we can, it is only by setting in motion the exact process by which nature produces them; and this being to us a mysterious process, of which the main circumstances are not only unknown but unobservable, we do not succeed in obtaining the antecedents under known circumstances. What is the result? That on this vast subject, which affords so 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 prep-

* Unless, indeed, the consequent was generated, not by the antecedent, but by the means employed to produce the antecedent. As, however, these means are under our power, there is so far a probability that they are also sufficiently within our knowledge to enable us to judge whether that could be the case or not.

aration 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, 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 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.

§ 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 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, 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 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 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 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 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 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 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 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 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,* "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 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.

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

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 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 suboxide, 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 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* 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 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 of a body oppositely electrified.

* 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 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; 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.*

§ 3. Our third example shall be extracted from Sir John Herschel's *Dis-*

* 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.

course on the Study of Natural Philosophy, a work replete with happily-selected exemplifications of 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.*

"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.†

"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 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

* Pp. 110, 111.

† Infra, book iv., chap. ii., On Abstraction.

it *is* very copiously on glass, both exposed with their 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 *cœteris 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 *cœteris 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 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 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 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.*

The accumulated proof of which the Theory of Dew has 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, and putrefaction sets in later, and proceeds more slowly.

* 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.

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 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. 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 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 Encke 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.*

"M. Arago, having suspended a magnetic needle by a silk thread, and set

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

it in vibration, observed, that it came much 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."*

"Almost all the greatest discoveries in Astronomy," says the same author,† "have resulted from the consideration of residual phenomena of a quantitative or numerical kind..... It was thus that the grand discovery

* Discourse, pp. 156–8, and 171. † Outlines of Astronomy, § 856.

of the precession of the equinoxes resulted 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 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:*

* *Philosophy of Discovery*, pp. 263, 264.

"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 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.

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,* fall under the Method of Agreement. The law of "falling bodies," namely, that they describe spaces proportional to the squares of the times, was his-

* See, on this point, the second chapter of the present book.

torically a 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, 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 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 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 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 *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 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 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 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)* is but a transformation of its cause, or in other words, where the effect and its cause are reciprocally such, and mutually convertible into each other; the problem of finding the cause resolves itself into the far easier one of finding an effect, which is the kind of inquiry that admits of being prosecuted by direct experiment. But there are other cases of heteropathic effects to which this mode of investigation is not applicable. Take, for instance, the heteropathic laws of mind; that portion of the phenomena of our mental nature which are analogous to chemical rather than to dynamical phenomena; as when a complex passion is formed by the coalition of several elementary impulses, or a complex emotion by several simple pleasures or pains, of which it is the result without being the aggregate, or in any respect homogeneous with them. The product, in these cases, is generated by its various factors; but the factors 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 $+a$, $-a$, $\frac{1}{2}b$, $-b$, $2b$, etc.; some of

* Ante, chap. vii., § 1.

which cancel one another, while many others do not appear distinguishably, but merge in one sum; forming altogether a result, between which and the causes whereby it was produced there is often an insurmountable difficulty in tracing by observation any fixed relation whatever.

The general idea of the Composition of Causes has been seen to be, that though two or more laws interfere with one another, and apparently frustrate or modify one another's operation, yet in reality all are fulfilled, the collective effect being the exact sum of the effects of the causes taken separately. A familiar instance is that of a body kept in equilibrium by two equal and contrary forces. One of the forces if acting alone would carry the body in a given time a certain distance to the west, the other if acting alone would carry it exactly as far 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 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* against the first force, and deflects it from its direction. There are not a law and an exception to that law, the law acting in ninety-nine cases, and the exception in one. There are two laws, each possibly acting in the whole hundred cases, and bringing about a common effect by their conjunct operation. If the force which, being the less conspicuous of the two, is called the *disturbing* force, prevails sufficiently over the other force in some one case, to constitute that case what is commonly called an exception, the same disturbing force probably acts as a modifying cause in many other cases which no one will call exceptions.

"Thus if it were stated to be a law of nature that all heavy bodies fall to the ground, it would probably be said that the resistance of the atmosphere, which prevents a balloon from falling, constitutes the balloon an ex-

* 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.

ception to that pretended law of nature. But the real law is, that all heavy bodies *tend* to fall; and to this there is no exception, not even the sun and moon; for even they, as every astronomer knows, tend 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."*

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

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

In order more completely to clear up the nature of each of these three methods, and determine which of them deserves the preference, it will be expedient (conformably to a 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 properties of

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

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 the subject.*

* 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

§ 8. The inapplicability of the method of simple observation to 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 of those rules of experimentation which it is found necessary to observe in other cases. When we devise an experiment to ascertain the effect of a given agent, there are certain precautions which we never, if we can help it, omit. In the first place, we introduce the agent into the midst of a set of circumstances which we have exactly ascertained. It needs hardly be remarked how far this condition is from being realized in any case connected with the phenomena of life; how far we are from knowing what are all the circumstances which pre-exist in any instance in which mercury is administered to a living being. This difficulty, however, though insuperable in

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 production of the effect.

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,"* and a few others. Even these are

* Bain's *Logic*, ii., 360.

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.*

If so little can be done by the experimental method to determine the conditions of an effect of many combined causes, in the case of medical science; still less is this method applicable to a class of phenomena more complicated than even those of physiology, the phenomena of politics and history. There, Plurality of Causes exists in almost boundless excess, and effects are, for the most part, inextricably interwoven with one another. To add to the embarrassment, most of the inquiries in political science relate to the production of effects of a most comprehensive description, such as the public wealth, public security, public morality, and the like: results liable to be affected directly or indirectly either in *plus* or in *minus* by nearly every fact which exists, or event which occurs, in human society. The vulgar notion, that the safe methods on political subjects are those of Baconian induction—that the true guide is not general reasoning, but specific experience—will one day be quoted as among the most unequivocal marks of a low state of the speculative faculties in any age in which it is accredited. Nothing can be more ludicrous than the sort of parodies on experimental reasoning which one is accustomed to meet with, not in popular discussion only, but in grave treatises, when the affairs of nations are the theme. "How," it is asked, "can an institution be bad, when the country has prospered under it?" "How can such or such causes have contributed to the prosperity of one country, when another has prospered without them?" Whoever makes use of an argument of this kind, not intending to deceive, should be sent back to learn the elements of some one of the more easy physical sciences. Such reasoners ignore the fact of Plurality of Causes in the very case which affords the most signal example of it. So little could be concluded, in such a case, from any possible collation of individual instances, that even the impossibility, in social phenomena, of making artificial experiments, a circumstance otherwise so prejudicial to directly inductive inquiry, hardly affords, in this case, additional reason of regret. For even if we could try experiments upon a nation or upon the human race, with as little scruple as M. Magendie tried them on dogs and rabbits, we should never succeed in making two instances identical in every respect except the presence or absence of some one definite circumstance. The nearest approach to an experiment in the philosophical sense, which takes place in politics, is the introduction of a new operative element into national affairs by some special and assignable measure of government, such as the enactment or repeal of a particular law. But where there are so many influences at work, it requires some time for the influence of any new cause upon national phenomena to become apparent; and as the causes operating in so extensive a sphere are not only infinitely numerous, but in a state of perpetual alteration, it is always certain that before the effect of

* 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.

the new cause becomes conspicuous enough to be a subject of induction, so many of the other influencing circumstances will have changed as to vitiate the experiment.*

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

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 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

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

influence of which 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 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 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