CHAPTER XIV.

OF THE LIMITS TO THE EXPLANATION OF LAWS OF NATURE; AND OF HYPOTHESES.

Sec. 1. The preceding considerations have led us to recognise a distinction between two kinds of laws, or observed uniformities in nature: ultimate laws, and what may be termed derivative laws. Derivative laws are such as are deducible from, and may, in any of the modes which we have pointed out, be resolved into, other and more general ones. Ultimate laws are those which cannot. We are not sure that any of the uniformities with which we are yet acquainted are ultimate laws; but we know that there must be ultimate laws; and that every resolution of a derivative law into more general laws, brings us nearer to them.

Since we are continually discovering that uniformities, not previously known to be other than ultimate, are derivative, and resolvable into more general laws; since (in other words) we are continually discovering the explanation of some sequence which was previously known only as a fact; it becomes an interesting question whether there are any necessary limits to this philosophical operation, or whether it may proceed until all the uniform sequences in nature are resolved into some one universal law. For this seems, at first sight, to be the ultimatum towards which the progress of induction, by the Deductive Method resting on a basis of observation and experiment, is tending. Projects of this kind were universal in the infancy of philosophy; any speculations which held out a less brilliant prospect, being in those early times deemed not worth pursuing. And the idea receives so much apparent countenance from the nature of the most remarkable achievements of modern science, that speculators are even now frequently appearing, who profess either to have solved the problem, or to suggest modes in which it may one day be solved. Even where pretensions of this magnitude are not made, the character of the solutions which are given or sought of particular classes of phenomena, often involves such conceptions of what constitutes explanation, as would render the notion of explaining all phenomena whatever by means of some one cause or law, perfectly admissible.

Sec. 2. It is therefore useful to remark, that the ultimate Laws of Nature cannot possibly be less numerous than the distinguishable sensations or other feelings of our nature;--those, I mean, which are distinguishable from one another in quality, and not merely in quantity or degree. For example; since there is a phenomenon *sui generis*, called colour, which our consciousness testifies to be not a particular degree of some other phenomenon, as heat or odour or motion, but intrinsically unlike all others, it follows that there are ultimate laws of colour; that though the facts of colour may admit of explanation, they never can be explained from laws of heat or odour alone, or of motion alone, but that however far the explanation may be carried, there will always remain in it a law of colour. I do not mean that it might not possibly be shown that some other phenomenon, some chemical or mechanical action for example, invariably precedes, and is the cause of, every phenomenon of colour. But though this, if proved, would be an important extension of our knowledge of nature, it would not explain how or why a motion, or a chemical action, can produce a sensation of colour; and however diligent might be our scrutiny of the phenomena, whatever number of hidden links we might detect in the chain of causation terminating in the colour, the last link would still be a law of colour, not a law of motion, nor of any other phenomenon whatever. Nor does this observation apply only to colour, as compared with any other of the great classes of sensations; it applies to every particular colour, as compared with others. White colour can in no manner be explained exclusively by the laws of the production of red colour. In any attempt to explain it, we cannot but introduce, as one element of the explanation, the proposition that some antecedent or other produces the sensation of white.

The ideal limit, therefore, of the explanation of natural phenomena (towards which as towards other ideal limits we are constantly tending, without the prospect of ever completely attaining it) would be to show that each distinguishable variety of our sensations, or other states of consciousness, has only one sort of cause; that, for example, whenever we perceive a white colour, there is some one condition or set of conditions which is always present, and the presence of which always produces in us that sensation. As long as there are several known modes of production of a phenomenon, (several different substances, for instance, which have the property of whiteness, and between which we cannot trace any other resemblance,) so long it is not impossible that one of these modes of production may be resolved into another, or that all of them may be resolved into some more general mode of production not hitherto recognised. But when the modes of production are reduced to one, we cannot, in point of simplification, go any further. This one may not, after all, be the ultimate mode; there may be other links to be discovered between the supposed cause and the effect; but we can only further resolve the known law, by introducing some other law hitherto unknown; which will not diminish the number of ultimate laws.

In what cases, accordingly, has science been most successful in explaining phenomena, by resolving their complex laws into laws of greater simplicity and generality? Hitherto chiefly in cases of the propagation of various phenomena through space: and, first and principally, the most extensive and important of all facts of that description, the fact of motion. Now this is exactly what might be expected from the principles here laid down. Not only is motion one of the most universal of all phenomena, it is also (as might be expected from that circumstance) one of those which, apparently at least, are produced in the greatest number of ways; but the phenomenon itself is always, to our sensations, the same in every respect but degree. Differences of duration, or of velocity, are evidently differences in degree only; and differences of direction in space, which alone has any semblance of being a distinction in kind, entirely disappear (so far as our sensations are concerned) by a change in our own position; indeed the very same motion appears to us, according to our position, to take place in every variety of direction, and motions in every different direction to take place in the same. And again, motion in a straight line and in a curve are no otherwise distinct than that the one is motion continuing in the same direction, the other is motion which at each instant changes its direction. There is, therefore, according to the principles I have stated, no absurdity in supposing that all motion may be produced in one and the same way; by the same kind of cause. Accordingly, the greatest achievements in physical science have consisted in resolving one observed law of the production of motion into the laws of other known modes of production, or the laws of several such modes into one more general mode; as when the fall of bodies to the earth, and the motions of the planets, were brought under the one law of the mutual attraction of all particles of matter; when the motions said to be produced by magnetism were shown to be produced by electricity; when the motions of fluids in a lateral direction, or even contrary to the direction of gravity, were shown to be produced by gravity; and the like. There is an abundance of distinct causes of motion still unresolved into one another; gravitation, heat, electricity, chemical action, nervous action, and so forth; but whether the efforts of the present generation of savans to resolve all these different modes of production into one, are ultimately successful or not, the attempt so to resolve them is perfectly legitimate. For though these various causes produce, in other respects, sensations intrinsically different, and are not, therefore, capable of being resolved into one another, yet in so far as they all produce motion, it is quite possible that the immediate antecedent of the motion may in all these different cases be the same; nor is it impossible that these various agencies themselves may, as the new doctrines assert, all of them have for their own immediate antecedent, modes of molecular motion.

We need not extend our illustration to other cases, as for instance to the propagation of light, sound, heat, electricity, &c. through space, or any of the other phenomena which have been found susceptible of explanation by the resolution of their observed laws into more general laws. Enough has been said to display the difference between the kind of explanation and resolution of laws which is chimerical, and that of which the accomplishment is the great aim of science; and to show into what sort of elements the resolution must be effected, if at all.

Sec. 3. As, however, there is scarcely any one of the principles of a true method of philosophizing which does not require to be guarded against errors on both sides, I must enter a caveat against another misapprehension, of a kind directly contrary to the preceding. M. Comte, among other occasions on which he has condemned, with some asperity, any attempt to explain phenomena which are "evidently primordial," (meaning, apparently, no more than that every peculiar phenomenon must have at least one peculiar and therefore inexplicable law,) has spoken of the attempt to furnish any explanation of the colour belonging to each substance, "la couleur elementaire propre a chaque substance," as essentially illusory. "No one," says he, "in our time attempts to explain the particular specific gravity of each substance or of each structure. Why should it be otherwise as to the specific colour, the notion of which is undoubtedly no less primordial?"[1]

Now although, as he elsewhere observes, a colour must always remain a different thing from a weight or a sound, varieties of colour might nevertheless follow, or correspond to, given varieties of weight, or sound, or some other phenomenon as different as these are from colour itself. It is one question what a thing is, and another what it depends on; and though to ascertain the conditions of an elementary phenomenon is not to obtain any new insight into the nature of the phenomenon itself, that is no reason against attempting to discover the conditions. The interdict against endeavouring to reduce distinctions of colour to any common principle, would have held equally good against a like attempt on the subject of distinctions of sound; which nevertheless have been found to be immediately preceded and caused by distinguishable varieties in the vibrations of elastic bodies: though a sound, no doubt, is quite as different as a colour is from any motion of particles, vibratory or otherwise. We might add, that, in the case of colours, there are strong positive indications that they are not ultimate properties of the different kinds of substances, but depend on conditions capable of being superinduced upon all substances; since there is no substance which cannot, according to the kind of light thrown upon it, be made to assume almost any colour; and since almost every change in the mode of aggregation of the particles of the same substance, is attended with alterations in its colour, and in its optical properties generally.

The real defect in the attempts which have been made to account for colours by the vibrations of a fluid, is not that the attempt itself is unphilosophical, but that the existence of the fluid, and the fact of its vibratory motion, are not proved; but are assumed, on no other ground than the facility they are supposed to afford of explaining the phenomena. And this consideration leads to the important question of the proper use of scientific hypotheses; the connexion of which with the subject of the explanation of the phenomena of nature, and of the necessary limits to that explanation, needs not be pointed out.

Sec. 4. An hypothesis is any supposition which we make (either without actual evidence, or on evidence avowedly insufficient) in order to endeavour to deduce from it conclusions in accordance with facts which are known to be real; under the idea that if the conclusions to which the hypothesis leads are known truths, the hypothesis itself either must be, or at least is likely to be, true. If the hypothesis relates to the cause, or mode of production of a phenomenon, it will serve, if admitted, to explain such facts as are found capable of being deduced from it. And this explanation is the purpose of many, if not most, hypotheses. Since explaining, in the scientific sense, means resolving an uniformity which is not a law of causation, into the laws of causation from which it results, or a complex law of causation into simpler and more general ones from which it is capable of being deductively inferred; if there do not exist any known laws which fulfil this requirement, we may feign or imagine some which would fulfil it; and this is making an hypothesis.

An hypothesis being a mere supposition, there are no other limits to hypotheses than those of the human imagination; we may, if we please, imagine, by way of accounting for an effect, some cause of a kind utterly unknown, and acting according to a law altogether fictitious. But as hypotheses of this sort would not have any of the plausibility belonging to those which ally themselves by analogy with known laws of nature, and besides would not supply the want which arbitrary hypotheses are generally invented to satisfy, by enabling the imagination to represent to itself an obscure phenomenon in a familiar light; there is probably no hypothesis in the history of science in which both the agent itself and the law of its operation were fictitious. Either the phenomenon assigned as the cause is real, but the law according to which it acts, merely supposed; or the cause is fictitious, but is supposed to produce its effects according to laws similar to those of some known class of phenomena. An instance of the first kind is afforded by the different suppositions made respecting the law of the planetary central force, anterior to the discovery of the true law, that the force varies as the inverse square of the distance; which also suggested itself to Newton, in the first instance, as an hypothesis, and was verified by proving that it led deductively to Kepler's laws. Hypotheses of the second kind are such as the vortices of Descartes, which were fictitious, but were supposed to obey the known laws of rotatory motion; or the two rival hypotheses respecting the nature of light, the one ascribing the phenomena to a fluid emitted from all luminous bodies, the other (now generally received) attributing them to vibratory motions among the particles of an ether pervading all space. Of the existence of either fluid there is no evidence, save the explanation they are calculated to afford of some of the phenomena; but they are supposed to produce their effects according to known laws; the ordinary laws of continued locomotion in the one case, and in the other, those of the propagation of undulatory movements among the particles of an elastic fluid.

According to the foregoing remarks, hypotheses are invented to enable the Deductive Method to be earlier applied to phenomena. But[2] in order to discover the cause of any phenomenon by the Deductive Method, the process must consist of three parts; induction, ratiocination, and verification. Induction, (the place of which, however, may be supplied by a prior deduction,) to ascertain the laws of the causes; ratiocination, to compute from those laws, how the causes will operate in the particular combination known to exist in the case in hand; verification, by comparing this calculated effect with the actual phenomenon. No one of these three parts of the process can be dispensed with. In the deduction which proves the identity of gravity with the central force of the solar system, all the three are found. First, it is proved from the moon's motions, that the earth attracts her with a force varying as the inverse square of the distance. This (though partly dependent on prior deductions) corresponds to the first, or purely inductive, step, the ascertainment of the law of the cause. Secondly, from this law, and from the knowledge previously obtained of the moon's mean distance from the earth, and of the actual amount of her deflexion from the tangent, it is ascertained with what rapidity the earth's attraction would cause the moon to fall, if she were no further off, and no more acted upon by extraneous forces, than terrestrial bodies are: that is the second step, the ratiocination. Finally, this calculated velocity being compared with the observed velocity with which all heavy bodies fall, by mere gravity, towards the surface of the earth, (sixteen feet in the first second, forty-eight in the second, and so forth, in the ratio of the odd numbers, 1, 3, 5, &c.,) the two quantities are found to agree. The order in which the steps are here presented, was not that of their discovery; but it is their correct logical order, as portions of the proof that the same attraction of the earth which causes the moon's motion, causes also the fall of heavy bodies to the earth: a proof which is thus complete in all its parts.

Now, the Hypothetical Method suppresses the first of the three steps, the induction to ascertain the law; and contents itself with the other two operations, ratiocination and verification; the law which is reasoned from, being assumed, instead of proved.

This process may evidently be legitimate on one supposition, namely, if the nature of the case be such that the final step, the verification, shall amount to, and fulfil the conditions of, a complete induction. We want to be assured that the law we have hypothetically assumed is a true one; and its leading deductively to true results will afford this assurance, provided the case be such that a false law cannot lead to a true result; provided no law, except the very one which we have assumed, can lead deductively to the same conclusions which that leads to. And this proviso is often realized. For example, in the very complete specimen of deduction which we just cited, the original major premise of the ratiocination, the law of the attractive force, was ascertained in this mode; by this legitimate employment of the Hypothetical Method. Newton began by an assumption, that the force which at each instant deflects a planet from its rectilineal course, and makes it describe a curve round the sun, is a force tending directly towards the sun. He then proved that if this be so, the planet will describe, as we know by Kepler's first law that it does describe, equal areas in equal times; and, lastly, he proved that if the force acted in any other direction whatever, the planet would not describe equal areas in equal times. It being thus shown that no other hypothesis would accord with the facts, the assumption was proved; the hypothesis became an inductive truth. Not only did Newton ascertain by this hypothetical process the direction of the deflecting force; he proceeded in exactly the same manner to ascertain the law of variation of the quantity of that force. He assumed that the force varied inversely as the square of the distance; showed that from this assumption the remaining two of Kepler's laws might be deduced; and finally, that any other law of variation would give results inconsistent with those laws, and inconsistent, therefore, with the real motions of the planets, of which Kepler's laws were known to be a correct expression.

I have said that in this case the verification fulfils the conditions of an induction: but an induction of what sort? On examination we find that it conforms to the canon of the Method of Difference. It affords the two instances, A B C, *a b c*, and B C, *b c*. A represents central force; A B C, the planets *plus* a central force; B C, the planets apart from a central force. The planets with a central force give *a*, areas proportional to the times; the planets without a central force give *b c* (a set of motions) without *a*, or with something else instead of *a*. This is the Method of Difference in all its strictness. It is true, the two instances which the method requires are obtained in this case, not by experiment, but by a prior deduction. But that is of no consequence. It is immaterial what is the nature of the evidence from which we derive the assurance that A B C will produce *a b c*, and B C only *b c*; it is enough that we have that assurance. In the present case, a process of reasoning furnished Newton with the very instances, which, if the nature of the case had admitted of it, he would have sought by experiment.

It is thus perfectly possible, and indeed is a very common occurrence, that what was an hypothesis at the beginning of the inquiry, becomes a proved law of nature before its close. But in order that this should happen, we must be able, either by deduction or experiment, to obtain *both* the instances which the Method of Difference requires. That we are able from the hypothesis to deduce the known facts, gives only the affirmative instance, A B C, *a b c*. It is equally necessary that we should be able to obtain, as Newton did, the negative instance B C, *b c*; by showing that no antecedent, except the one assumed in the hypothesis, would in conjunction with B C produce *a*.

Now it appears to me that this assurance cannot be obtained, when the cause assumed in the hypothesis is an unknown cause, imagined solely to account for *a*. When we are only seeking to determine the precise law of a cause already ascertained, or to distinguish the particular agent which is in fact the cause, among several agents of the same kind, one or other of which it is already known to be, we may then obtain the negative instance. An inquiry, which of the bodies of the solar system causes by its attraction some particular irregularity in the orbit or periodic time of some satellite or comet, would be a case of the second description. Newton's was a case of the first. If it had not been previously known that the planets were hindered from moving in straight lines by some force tending towards the interior of their orbit, though the exact direction was doubtful; or if it had not been known that the force increased in some proportion or other as the distance diminished, and diminished as it increased; Newton's argument would not have proved his conclusion. These facts, however, being already certain, the range of admissible suppositions was limited to the various possible directions of a line, and the various possible numerical relations between the variations of the distance, and the variations of the attractive force: now among these it was easily shown that different suppositions could not lead to identical consequences.

Accordingly, Newton could not have performed his second great scientific operation, that of identifying terrestrial gravity with the central force of the solar system, by the same hypothetical method. When the law of the moon's attraction had been proved from the data of the moon itself, then on finding the same law to accord with the phenomena of terrestrial gravity, he was warranted in adopting it as the law of those phenomena likewise; but it would not have been allowable for him, without any lunar data, to assume that the moon was attracted towards the earth with a force as the inverse square of the distance, merely because that ratio would enable him to account for terrestrial gravity: for it would have been impossible for him to prove that the observed law of the fall of heavy bodies to the earth could not result from any force, save one extending to the moon, and proportional to the inverse square.

It appears, then, to be a condition of a genuinely scientific hypothesis, that it be not destined always to remain an hypothesis, but be of such a nature as to be either proved or disproved by comparison with observed facts. This condition is fulfilled when the effect is already known to depend on the very cause supposed, and the hypothesis relates only to the precise mode of dependence; the law of the variation of the effect according to the variations in the quantity or in the relations of the cause. With these may be classed the hypotheses which do not make any supposition with regard to causation, but only with regard to the law of correspondence between facts which accompany each other in their variations, though there may be no relation of cause and effect between them. Such were the different false hypotheses which Kepler made respecting the law of the refraction of light. It was known that the direction of the line of refraction varied with every variation in the direction of the line of incidence, but it was not known how; that is, what changes of the one corresponded to the different changes of the other. In this case any law, different from the true one, must have led to false results. And, lastly, we must add to these, all hypothetical modes of merely representing, or *describing*, phenomena; such as the hypothesis of the ancient astronomers that the heavenly bodies moved in circles; the various hypotheses of excentrics, deferents, and epicycles, which were added to that original hypothesis; the nineteen false hypotheses which Kepler made and abandoned respecting the form of the planetary orbits; and even the doctrine in which he finally rested, that those orbits are ellipses, which was but an hypothesis like the rest until verified by facts.

In all these cases, verification is proof; if the supposition accords with the phenomena there needs no other evidence of it. But in order that this may be the case, I conceive it to be necessary, when the hypothesis relates to causation, that the supposed cause should not only be a real phenomenon, something actually existing in nature, but should be already known to exercise, or at least to be capable of exercising, an influence of some sort over the effect. In any other case, it is no evidence of the truth of the hypothesis that we are able to deduce the real phenomena from it.

Is it, then, never allowable, in a scientific hypothesis, to assume a cause; but only to ascribe an assumed law to a known cause? I do not assert this. I only say, that in the latter case alone can the hypothesis be received as true merely because it explains the phenomena: in the former case it is only useful by suggesting a line of investigation which may possibly terminate in obtaining real proof. For this purpose, as is justly remarked by M. Comte, it is indispensable that the cause suggested by the hypothesis should be in its own nature susceptible of being proved by other evidence. This seems to be the philosophical import of Newton's maxim, (so often cited with approbation by subsequent writers,) that the cause assigned for any phenomenon must not only be such as if admitted would explain the phenomenon, but must also be a *vera causa*. What he meant by a *vera causa* Newton did not indeed very explicitly define; and Dr. Whewell, who dissents from the propriety of any such restriction upon the latitude of framing hypotheses, has had little difficulty in showing[3] that his conception of it was neither precise nor consistent with itself: accordingly his optical theory was a signal instance of the violation of his own rule. It is certainly not necessary that the cause assigned should be a cause already known; else how could we ever become acquainted with any new cause? But what is true in the maxim is, that the cause, though not known previously, should be capable of being known thereafter; that its existence should be capable of being detected, and its connexion with the effect ascribed to it should be susceptible of being proved, by independent evidence. The hypothesis, by suggesting observations and experiments, puts us on the road to that independent evidence if it be really attainable; and till it be attained, the hypothesis ought not to count for more than a conjecture.

Sec. 5. This function, however, of hypotheses, is one which must be reckoned absolutely indispensable in science. When Newton said, "Hypotheses non fingo," he did not mean that he deprived himself of the facilities of investigation afforded by assuming in the first instance what he hoped ultimately to be able to prove. Without such assumptions, science could never have attained its present state: they are necessary steps in the progress to something more certain; and nearly everything which is now theory was once hypothesis. Even in purely experimental science, some inducement is necessary for trying one experiment rather than another; and though it is abstractedly possible that all the experiments which have been tried, might have been produced by the mere desire to ascertain what would happen in certain circumstances, without any previous conjecture as to the result; yet, in point of fact, those unobvious, delicate, and often cumbrous and tedious processes of experiment, which have thrown most light upon the general constitution of nature, would hardly ever have been undertaken by the persons or at the time they were, unless it had seemed to depend on them whether some general doctrine or theory which had been suggested, but not yet proved, should be admitted or not. If this be true even of merely experimental inquiry, the conversion of experimental into deductive truths could still less have been effected without large temporary assistance from hypotheses. The process of tracing regularity in any complicated, and at first sight confused set of appearances, is necessarily tentative: we begin by making any supposition, even a false one, to see what consequences will follow from it; and by observing how these differ from the real phenomena, we learn what corrections to make in our assumption. The simplest supposition which accords with the more obvious facts, is the best to begin with; because its consequences are the most easily traced. This rude hypothesis is then rudely corrected, and the operation repeated; and the comparison of the consequences deducible from the corrected hypothesis, with the observed facts, suggests still further correction, until the deductive results are at last made to tally with the phenomena. "Some fact is as yet little understood, or some law is unknown: we frame on the subject an hypothesis as accordant as possible with the whole of the data already possessed; and the science, being thus enabled to move forward freely, always ends by leading to new consequences capable of observation, which either confirm or refute, unequivocally, the first supposition." Neither induction nor deduction would enable us to understand even the simplest phenomena, "if we did not often commence by anticipating on the results; by making a provisional supposition, at first essentially conjectural, as to some of the very notions which constitute the final object of the inquiry."[4] Let any one watch the manner in which he himself unravels a complicated mass of evidence; let him observe how, for instance, he elicits the true history of any occurrence from the involved statements of one or of many witnesses: he will find that he does not take all the items of evidence into his mind at once, and attempt to weave them together: he extemporises, from a few of the particulars, a first rude theory of the mode in which the facts took place, and then looks at the other statements one by one, to try whether they can be reconciled with that provisional theory, or what alterations or additions it requires to make it square with them. In this way, which has been justly compared to the Methods of Approximation of mathematicians, we arrive, by means of hypotheses, at conclusions not hypothetical.[5]

Sec. 6. It is perfectly consistent with the spirit of the method, to assume in this provisional manner not only an hypothesis respecting the law of what we already know to be the cause, but an hypothesis respecting the cause itself. It is allowable, useful, and often even necessary, to begin by asking ourselves what cause *may* have produced the effect, in order that we may know in what direction to look out for evidence to determine whether it actually *did*. The vortices of Descartes would have been a perfectly legitimate hypothesis, if it had been possible, by any mode of exploration which we could entertain the hope of ever possessing, to bring the reality of the vortices, as a fact in nature, conclusively to the test of observation. The hypothesis was vicious, simply because it could not lead to any course of investigation capable of converting it from an hypothesis into a proved fact. It might chance to be *dis*proved, either by some want of correspondence with the phenomena it purported to explain, or (as actually happened) by some extraneous fact. "The free passage of comets through the spaces in which these vortices should have been, convinced men that these vortices did not exist."[6] But the hypothesis would have been false, though no such direct evidence of its falsity had been procurable. Direct evidence of its truth there could not be.

The prevailing hypothesis of a luminiferous ether, in other respects not without analogy to that of Descartes, is not in its own nature entirely cut off from the possibility of direct evidence in its favour. It is well known that the difference between the calculated and the observed times of the periodical return of Encke's comet, has led to a conjecture that a medium capable of opposing resistance to motion is diffused through space. If this surmise should be confirmed, in the course of ages, by the gradual accumulation of a similar variance in the case of the other bodies of the solar system, the luminiferous ether would have made a considerable advance towards the character of a *vera causa*, since the existence would have been ascertained of a great cosmical agent, possessing some of the attributes which the hypothesis assumes; though there would still remain many difficulties, and the identification of the ether with the resisting medium would even, I imagine, give rise to new ones. At present, however, this supposition cannot be looked upon as more than a conjecture; the existence of the ether still rests on the possibility of deducing from its assumed laws a considerable number of the phenomena of light; and this evidence I cannot regard as conclusive, because we cannot have, in the case of such an hypothesis, the assurance that if the hypothesis be false it must lead to results at variance with the true facts.

Accordingly, most thinkers of any degree of sobriety allow, that an hypothesis of this kind is not to be received as probably true because it accounts for all the known phenomena; since this is a condition sometimes fulfilled tolerably well by two conflicting hypotheses; while there are probably a thousand more which are equally possible, but which, for want of anything analogous in our experience, our minds are unfitted to conceive. But it seems to be thought that an hypothesis of the sort in question is entitled to a more favourable reception, if, besides accounting for all the facts previously known, it has led to the anticipation and prediction of others which experience afterwards verified; as the undulatory theory of light led to the prediction, subsequently realized by experiment, that two luminous rays might meet each other in such a manner as to produce darkness. Such predictions and their fulfilment are, indeed, well calculated to impress the uninformed, whose faith in science rests solely on similar coincidences between its prophecies and what comes to pass. But it is strange that any considerable stress should be laid upon such a coincidence by persons of scientific attainments. If the laws of the propagation of light accord with those of the vibrations of an elastic fluid in as many respects as is necessary to make the hypothesis afford a correct expression of all or most of the phenomena known at the time, it is nothing strange that they should accord with each other in one respect more. Though twenty such coincidences should occur, they would not prove the reality of the undulatory ether; it would not follow that the phenomena of light were results of the laws of elastic fluids, but at most that they are governed by laws partially identical with these; which, we may observe, is already certain, from the fact that the hypothesis in question could be for a moment tenable.[7] Cases may be cited, even in our imperfect acquaintance with nature, where agencies that we have good reason to consider as radically distinct, produce their effects, or some of their effects, according to laws which are identical. The law, for example, of the inverse square of the distance, is the measure of the intensity not only of gravitation, but (it is believed) of illumination, and of heat diffused from a centre. Yet no one looks upon this identity as proving similarity in the mechanism by which the three kinds of phenomena are produced.

According to Dr. Whewell, the coincidence of results predicted from an hypothesis, with facts afterwards observed, amounts to a conclusive proof of the truth of the theory. "If I copy a long series of letters, of which the last half dozen are concealed, and if I guess these aright, as is found to be the case when they are afterwards uncovered, this must be because I have made out the import of the inscription. To say, that because I have copied all that I could see, it is nothing strange that I should guess those which I cannot see, would be absurd, without supposing such a ground for guessing."[8] If any one, from examining the greater part of a long inscription, can interpret the characters so that the inscription gives a rational meaning in a known language, there is a strong presumption that his interpretation is correct; but I do not think the presumption much increased by his being able to guess the few remaining letters without seeing them: for we should naturally expect (when the nature of the case excludes chance) that even an erroneous interpretation which accorded with all the visible parts of the inscription would accord also with the small remainder; as would be the case, for example, if the inscription had been designedly so contrived as to admit of a double sense. I assume that the uncovered characters afford an amount of coincidence too great to be merely casual: otherwise the illustration is not a fair one. No one supposes the agreement with the phenomena of light with the theory of undulations to be merely fortuitous. It must arise from the actual identity of some of the laws of undulations with some of those of light: and if there be that identity, it is reasonable to suppose that its consequences would not end with the phenomena which first suggested the identification, nor be even confined to such phenomena as were known at the time. But it does not follow, because some of the laws agree with those of undulations, that there are any actual undulations; no more than it followed because some (though not so many) of the same laws agreed with those of the projection of particles, that there was actual emission of particles. Even the undulatory hypothesis does not account for all the phenomena of light. The natural colours of objects, the compound nature of the solar ray, the absorption of light, and its chemical and vital action, the hypothesis leaves as mysterious as it found them; and some of these facts are, at least apparently, more reconcileable with the emission theory than with that of Young and Fresnel. Who knows but that some third hypothesis, including all these phenomena, may in time leave the undulatory theory as far behind as that has left the theory of Newton and his successors?

To the statement, that the condition of accounting for all the known phenomena is often fulfilled equally well by two conflicting hypotheses, Dr. Whewell makes answer that he knows "of no such case in the history of science, where the phenomena are at all numerous and complicated."[9] Such an affirmation, by a writer of Dr. Whewell's minute acquaintance with the history of science, would carry great authority, if he had not, a few pages before, taken pains to refute it,[10] by maintaining that even the exploded scientific hypotheses might always, or almost always, have been so modified as to make them correct representations of the phenomena. The hypothesis of vortices, he tells us, was, by successive modifications, brought to coincide in its results with the Newtonian theory and with the facts. The vortices did not indeed explain all the phenomena which the Newtonian theory was ultimately found to account for, such as the precession of the equinoxes; but this phenomenon was not, at the time, in the contemplation of either party, as one of the facts to be accounted for. All the facts which they did contemplate, we may believe on Dr. Whewell's authority to have accorded as accurately with the Cartesian hypothesis, in its finally improved state, as with Newton's.

But it is not, I conceive, a valid reason for accepting any given hypothesis, that we are unable to imagine any other which will account for the facts. There is no necessity for supposing that the true explanation must be one which, with only our present experience, we could imagine. Among the natural agents with which we are acquainted, the vibrations of an elastic fluid may be the only one whose laws bear a close resemblance to those of light; but we cannot tell that there does not exist an unknown cause, other than an elastic ether diffused through space, yet producing effects identical in some respects with those which would result from the undulations of such an ether. To assume that no such cause can exist, appears to me an extreme case of assumption without evidence.

I do not mean to condemn those who employ themselves in working out into detail this sort of hypotheses; it is useful to ascertain what are the known phenomena, to the laws of which those of the subject of inquiry bear the greatest, or even a great analogy, since this may suggest (as in the case of the luminiferous ether it actually did) experiments to determine whether the analogy which goes so far does not extend still further. But that, in doing this, we should imagine ourselves to be seriously inquiring whether the hypothesis of an ether, an electric fluid, or the like, is true; that we should fancy it possible to obtain the assurance that the phenomena are produced in that way and no other; seems to me, I confess, unworthy of the present improved conceptions of the methods of physical science. And at the risk of being charged with want of modesty, I cannot help expressing astonishment that a philosopher of Dr. Whewell's abilities and attainments should have written an elaborate treatise on the philosophy of induction, in which he recognises absolutely no mode of induction except that of trying hypothesis after hypothesis until one is found which fits the phenomena; which one, when found, is to be assumed as true, with no other reservation than that if on re-examination it should appear to assume more than is needful for explaining the phenomena, the superfluous part of the assumption should be cut off. And this without the slightest distinction between the cases in which it may be known beforehand that two different hypotheses cannot lead to the same result, and those in which, for aught we can ever know, the range of suppositions, all equally consistent with the phenomena, may be infinite.[11]

Sec. 7. It is necessary, before quitting the subject of hypotheses, to guard against the appearance of reflecting upon the scientific value of several branches of physical inquiry, which, though only in their infancy, I hold to be strictly inductive. There is a great difference between inventing agencies to account for classes of phenomena, and endeavouring, in conformity with known laws, to conjecture what former collocations of known agents may have given birth to individual facts still in existence. The latter is the legitimate operation of inferring from an observed effect, the existence, in time past, of a cause similar to that by which we know it to be produced in all cases in which we have actual experience of its origin. This, for example, is the scope of the inquiries of geology; and they are no more illogical or visionary than judicial inquiries, which also aim at discovering a past event by inference from those of its effects which still subsist. As we can ascertain whether a man was murdered or died a natural death, from the indications exhibited by the corpse, the presence or absence of signs of struggling on the ground or on the adjacent objects, the marks of blood, the footsteps of the supposed murderers, and so on, proceeding throughout on uniformities ascertained by a perfect induction without any mixture of hypothesis; so if we find, on and beneath the surface of our planet, masses exactly similar to deposits from water, or to results of the cooling of matter melted by fire, we may justly conclude that such has been their origin; and if the effects, though similar in kind, are on a far larger scale than any which are now produced, we may rationally, and without hypothesis, conclude either that the causes existed formerly with greater intensity, or that they have operated during an enormous length of time. Further than this no geologist of authority has, since the rise of the present enlightened school of geological speculation, attempted to go.

In many geological inquiries it doubtless happens that though the laws to which the phenomena are ascribed are known laws, and the agents known agents, those agents are not known to have been present in the particular case. In the speculation respecting the igneous origin of trap or granite, the fact does not admit of direct proof, that those substances have been actually subjected to intense heat. But the same thing might be said of all judicial inquiries which proceed on circumstantial evidence. We can conclude that a man was murdered, though it is not proved by the testimony of eye-witnesses that some person who had the intention of murdering him was present on the spot. It is enough, for most purposes, if no other known cause could have generated the effects shown to have been produced.

The celebrated speculation of Laplace concerning the origin of the earth and planets, participates essentially in the inductive character of modern geological theory. The speculation is, that the atmosphere of the sun originally extended to the present limits of the solar system; from which, by the process of cooling, it has contracted to its present dimensions; and since, by the general principles of mechanics, the rotation of the sun and of its accompanying atmosphere must increase in rapidity as its volume diminishes, the increased centrifugal force generated by the more rapid rotation, overbalancing the action of gravitation, has caused the sun to abandon successive rings of vaporous matter, which are supposed to have condensed by cooling, and to have become the planets. There is in this theory no unknown substance introduced on supposition, nor any unknown property or law ascribed to a known substance. The known laws of matter authorize us to suppose that a body which is constantly giving out so large an amount of heat as the sun is, must be progressively cooling, and that, by the process of cooling, it must contract; if, therefore, we endeavour, from the present state of that luminary, to infer its state in a time long past, we must necessarily suppose that its atmosphere extended much farther than at present, and we are entitled to suppose that it extended as far as we can trace effects such as it might naturally leave behind it on retiring; and such the planets are. These suppositions being made, it follows from known laws that successive zones of the solar atmosphere might be abandoned; that these would continue to revolve round the sun with the same velocity as when they formed part of its substance; and that they would cool down, long before the sun itself, to any given temperature, and consequently to that at which the greater part of the vaporous matter of which they consisted would become liquid or solid. The known law of gravitation would then cause them to agglomerate in masses, which would assume the shape our planets actually exhibit; would acquire, each about its own axis, a rotatory movement; and would in that state revolve, as the planets actually do, about the sun, in the same direction with the sun's rotation, but with less velocity, because in the same periodic time which the sun's rotation occupied when his atmosphere extended to that point. There is thus, in Laplace's theory, nothing, strictly speaking, hypothetical: it is an example of legitimate reasoning from a present effect to a possible past cause, according to the known laws of that cause. The theory therefore is, as I have said, of a similar character to the theories of geologists; but considerably inferior to them in point of evidence. Even if it were proved (which it is not) that the conditions necessary for determining the breaking off of successive rings would certainly occur; there would still be a much greater chance of error in assuming that the existing laws of nature are the same which existed at the origin of the solar system, than in merely presuming (with geologists) that those laws have lasted through a few revolutions and transformations of a single one among the bodies of which that system is composed.