CHAPTER II.

OF INDUCTIONS IMPROPERLY SO CALLED.

Sec. 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 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 anything 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 reassertion 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, cannot 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.

Sec. 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 cannot 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 an 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 cannot 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[2]) 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 an 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 cannot 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 algebraical 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, cannot but perceive that in raising a binomial to a power, the coefficients must depend on the laws of permutation and combination: and as soon as this is recognised, 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.

Sec. 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 cannot 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 anything of the nature of an induction in this process? Did he infer anything 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 anything to the power of prediction already possessed.

Sec. 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,[3] "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 connexion. 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 endeavouring 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 recognised it; just as the island was an island before it had been 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 cannot 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 cannot 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 anything 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 cannot 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 afterwards 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."[4] 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 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 skilful 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, and facts were disclosed which were not reconcileable 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 excentrics 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 afterwards 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 detruisant sans retour les hypotheses primitives, envisagees comme lois reelles du monde, a soigneusement maintenu leur valeur positive et permanente, la propriete de representer commodement les phenomenes quand il s'agit d'une premiere ebauche. Nos ressources a cet egard sont meme bien plus etendues, precisement a cause que nous ne nous faisons aucune illusion sur la realite des hypotheses; ce qui nous permet d'employer sans scrupule, en chaque cas, celle que nous jugeons la plus avantageuse."[5]

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 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.[6]

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, towards 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 connexion 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 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.

Sec. 5. Dr. Whewell has replied at some length to the preceding observations, re-stating his opinions, but without (as far as I can perceive) adding anything 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*.[7] 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, cannot 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 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 connexion 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 everything 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.