CHAPTER XIII.

MISCELLANEOUS EXAMPLES OF THE EXPLANATION OF LAWS OF NATURE.

§ 1. Some of the most remarkable instances which have occurred since the great Newtonian generalization, of the explanation of laws of causation subsisting among complex phenomena, by resolving them into simpler and more general laws, are to be found among the speculations of Liebig in organic chemistry. These speculations, though they have not yet been sufficiently long before the world to entitle us positively to assume that no well-grounded objection can be made to any part of them, afford, however, so admirable an example of the spirit of the Deductive Method, that I may be permitted to present some specimens of them here.

It had been observed in certain cases, that chemical action is, as it were, contagious; that is to say, a substance which would not of itself yield to a particular chemical attraction, (the force of the attraction not being sufficient to overcome cohesion, or to destroy some chemical combination in which the substance was already held), will nevertheless do so if placed in contact with some other body which is in the act of yielding to the same force. Nitric acid, for example, does not dissolve pure platinum, which may "be boiled with this acid without being oxidized by it, even when in a state of such fine division that it no longer reflects light." But the same acid easily dissolves silver. Now if an alloy of silver and platinum be treated with nitric acid, the acid does not, as might naturally be expected, separate the two metals, dissolving the silver, and leaving the platinum; it dissolves both: the platinum as well as the silver becomes oxidized, and in that state combines with the undecomposed portion of the acid. In like manner, "copper does not decompose water, even when boiled in dilute sulphuric acid; but an alloy of copper, zinc, and nickel, dissolves easily in this acid with evolution of hydrogen gas." These phenomena cannot be explained by the laws of what is termed chemical affinity. They point to a peculiar law, by which the oxidation which one body suffers, causes another, in contact with it, to submit to the same change. And not only chemical composition, but chemical decomposition, is capable of being similarly propagated. The peroxide of hydrogen, a compound formed by hydrogen with a greater amount of oxygen than the quantity necessary to form water, is held together by a chemical attraction of so weak a nature, that the slightest circumstance is sufficient to decompose it; and it even, though very slowly, gives off oxygen and is reduced to water spontaneously (being, I presume, decomposed by the tendency of its oxygen to absorb heat and assume the gaseous state). Now it has been observed, that if this decomposition of the peroxide of hydrogen takes place in contact with some metallic oxides, as those of silver, and the peroxides of lead and manganese, it superinduces a corresponding chemical action upon those substances; they also give forth the whole or a portion of their oxygen, and are reduced to the metal or to the protoxide; although they do not undergo this change spontaneously, and there is no chemical affinity at work to make them do so. Other similar phenomena are mentioned by Liebig. "Now no other explanation," he observes, "of these phenomena can be given, than that a body in the act of combination or decomposition enables another body, with which it is in contact, to enter into the same state."

Here, therefore, is a law of nature of great simplicity, but which, owing to the extremely special and limited character of the phenomena in which alone it can be detected experimentally, (because in them alone its results are not intermixed and blended with those of other laws,) had been very little recognised by chemists, and no one could have ventured, on experimental evidence, to affirm it as a law common to all chemical action; owing to the impossibility of a rigorous employment of the Method of Difference where the properties of different kinds of substance are involved, an impossibility which we noticed and characterized in a previous chapter.(90) Now this extremely special and apparently precarious generalization has, in the hands of Liebig, been converted, by a masterly employment of the Deductive Method, into a law pervading all nature, in the same way as gravitation assumed that character in the hands of Newton; and has been found to explain, in the most unexpected manner, numerous detached generalizations of a more limited kind, reducing the phenomena concerned in those generalizations into mere cases of itself.

The contagious influence of chemical action is not a powerful force, and is only capable of overcoming weak affinities: we, may, therefore, expect to find it principally exemplified in the decomposition of substances which are held together by weak chemical forces. Now the force which holds a compound substance together is generally weaker, the more compound the substance is; and organic products are the most compound substances known, those which have the most complex atomic constitution. It is, therefore, upon such substances that the self-propagating power of chemical action is likely to exert itself in the most marked manner. Accordingly, first, it explains the remarkable laws of fermentation, and some of those of putrefaction. "A little leaven," that is, dough in a certain state of chemical action, impresses a similar chemical action upon "the whole lump." The contact of any decaying substance, occasions the decay of matter previously sound. Again, yeast is a substance actually in a process of decomposition from the action of air and water, evolving carbonic acid gas. Sugar is a substance which, from the complexity of its composition, has no great energy of coherence in its existing form, and is capable of being easily converted (by combination with the elements of water) into carbonic acid and alcohol. Now the mere presence of yeast, the mere proximity of a substance of which the elements are separating from each other, and combining with the elements of water, causes sugar to undergo the same change, giving out carbonic acid gas, and becoming alcohol. It is not the elements contained in the yeast which do this. "An aqueous infusion of yeast may be mixed with a solution of sugar, and preserved in vessels from which the air is excluded, without either experiencing the slightest change." Neither does the insoluble residue of the yeast, after being treated with water, possess the power of exciting fermentation. (Here we have the method of Difference). It is not the yeast itself, therefore; it is the yeast in a state of decomposition. The sugar, which would not decompose and oxidize by the mere presence of oxygen and water, is induced to do so when another oxidation is at work in the midst of it.

By the same principle Liebig is enabled to explain many cases of malaria; the pernicious influence of putrid substances; a variety of poisons; contagious diseases; and other phenomena. Of all substances, those composing the animal body are the most complex in their composition, and are in the least stable condition of union. The blood, in particular, is the most unstable compound known. It is, therefore, not surprising that gaseous or other substances, in the act of undergoing the chemical changes which constitute, for instance, putrefaction, should, when brought into contact with the tissues by respiration or otherwise, and still more when introduced by inoculation into the blood itself, impress upon some of the particles a chemical action similar to its own; which is propagated in like manner to other particles, until the whole system is placed in a state of chemical action more or less inconsistent with the chemical conditions of vitality.

Of the three modes in which we observed in the last chapter that the resolution of a special law into more general ones may take place, this speculation exemplifies the second. The laws explained are such as this, that yeast puts sugar into a state of fermentation. Between the remote cause, the presence of yeast, and the consequent fermentation of the sugar, there has been interpolated a proximate cause, the chemical action between the particles of the yeast and the elements of air and water. The special law is thus resolved into two others, more general than itself: the first, that yeast is decomposed by the presence of air and water; the second, that matter undergoing chemical action has a tendency to produce similar chemical action in other matter in contact with it. But while the investigation thus aptly exhibits the second mode of the resolution of a complex law, it no less happily exemplifies the third; the subsumption of special laws under a more general law, by gathering them up into one more comprehensive expression which includes them all. For the curious fact of the contagious nature of chemical action is only raised into a law of *all* chemical action by these very investigations; just as the Newtonian attraction was only recognised as a law of all matter when it was found to explain the phenomena of terrestrial gravity. Previously to Liebig's investigations, the property in question had only been observed in a few special cases of chemical action; but when his deductive reasonings have established that innumerable effects produced upon weak compounds, by substances none of whose known peculiarities would account for their having such a power, might be explained by considering the supposed special property to exist in all those cases, these numerous generalizations on separate substances are brought together into one law of chemical action in general: the peculiarities of the various substances being, in fact, eliminated, just as the Newtonian deduction eliminated from the instances of terrestrial gravity the circumstance of proximity to the earth.

§ 2. Another speculation of the same chemist, which, if it should ultimately be found to agree with all the facts of the extremely complicated phenomenon to which it relates, will constitute one of the finest examples of the Deductive Method on record, is his theory of respiration.

The facts of respiration, or in other words the special laws which it is attempted to explain from, and resolve into, more general ones, are, that the blood in passing through the lungs absorbs oxygen and gives out carbonic acid gas, changing thereby its colour from a blackish purple to a brilliant red. The absorption and exhalation are evidently chemical phenomena; and the carbon of the carbonic acid must have been derived from the body, that is, must have been absorbed by the blood from the substances with which it came into contact in its passage through the organism. Required to find the intermediate links--the precise nature of the two chemical actions which take place; first, the absorption of the carbon or of the carbonic acid by the blood, in its circulation through the body; next, the excretion of the carbon, or the exchange of the carbonic acid for oxygen, in its passage through the lungs.

Dr. Liebig believes himself to have found the solution of this *vexata quæstio* in a class of chemical actions in which scarcely any less acute and penetrating inquirer would have thought of looking for it.

Blood is composed of two parts, the serum and the globules. The serum absorbs and holds in solution carbonic acid in great quantity, but has no tendency either to part with it or to absorb oxygen. The globules, therefore, are concluded to be the portion of the blood which is operative in respiration. These globules contain a certain quantity of iron, which from chemical tests is inferred to be in the state of oxide.

Dr. Liebig recognised, in the known chemical properties of the oxides of iron, laws which, if followed out deductively, would lead to the prediction of the precise series of phenomena which respiration exhibits.

There are two oxides of iron, a protoxide and a peroxide. In the arterial blood the iron is in the form of peroxide: in the venous blood we have no direct evidence which of the oxides is present, but the considerations to be presently stated lead to the conclusion that it is the protoxide. As arterial and venous blood are in a perpetual state of alternate conversion into one another, the question arises, in what circumstances the protoxide of iron is capable of being converted into the peroxide, and *vice versâ*. Now the protoxide readily combines with oxygen in the presence of water, forming the hydrated peroxide: these conditions it finds in passing through the lungs; it derives oxygen from the air, and finds water in the blood itself. This would already explain one portion of the phenomena of respiration. But the arterial blood, in quitting the lungs, is charged with hydrated peroxide: in what manner is the peroxide brought back to its former state?

The chemical conditions for the reduction of the hydrated peroxide into the state of protoxide, are precisely those which the blood meets with in circulating through the body; namely, contact with organic compounds.

Hydrated peroxide of iron, when treated with organic compounds (where no sulphur is present) gives forth oxygen and water, which oxygen, attracting the carbon from the organic substance, becomes carbonic acid; while the peroxide, being reduced to the state of protoxide, combines with the carbonic acid, and becomes a carbonate. Now this carbonate needs only come again into contact with oxygen and water to be decomposed; the carbonic acid being given off, and the protoxide, by the absorption of oxygen and water, becoming again the hydrated peroxide.

The mysterious chemical phenomena connected with respiration can now, by a beautiful deductive process, be completely explained. The arterial blood, containing iron in the form of hydrated peroxide, passes into the capillaries, where it meets with the decaying tissues, receiving also in its course certain non-azotised but highly carbonised animal products, in particular the bile. In these it finds the precise conditions required for decomposing the peroxide into oxygen and the protoxide. The oxygen combines with the carbon of the decaying tissues, and forms carbonic acid, which, though insufficient in amount to neutralize the whole of the

protoxide, combines with a portion (one-fourth) of it, and returns in the form of a carbonate, along with the other three-fourths of the protoxide, through the venous system into the lungs. There it again meets with oxygen and water: the free protoxide becomes hydrated peroxide: the carbonate of protoxide parts with its carbonic acid, and by absorbing oxygen and water, enters also into the state of hydrated peroxide. The heat evolved in the transition from protoxide to peroxide, as well as in the previous oxidation of the carbon contained in the tissues, is considered by Liebig as the cause which sustains the temperature of the body. But into this portion of the speculation we need not enter.(91)

This example displays the second mode of resolving complex laws, by the interpolation of intermediate links in the chain of causation; and some of the steps of the deduction exhibit cases of the first mode, that which infers the joint effect of two or more causes from their separate effects; but to trace out in detail these exemplifications may be left to the intelligence of the reader. The third mode is not employed in this example, since the simpler laws into which those of respiration are resolved (the laws of the chemical action of the oxides of iron) were laws already known, and do not acquire any additional generality from their employment in the present case.

§ 3. The property which salt possesses of preserving animal substances from putrefaction is resolved by Liebig into two more general laws, the strong attraction of salt for water, and the necessity of the presence of water as a condition of putrefaction. The intermediate phenomenon which is interpolated between the remote cause and the effect, can here be not merely inferred but seen; for it is a familiar fact, that flesh upon which salt has been thrown is speedily found swimming in brine.

The second of the two factors (as they may be termed) into which the preceding law has been resolved, the necessity of water to putrefaction, itself affords an additional example of the Resolution of Laws. The law itself is proved by the Method of Difference, since flesh completely dried and kept in a dry atmosphere does not putrefy, as we see in the case of dried provisions, and human bodies in very dry climates. A deductive explanation of this same law results from Liebig's speculations. The putrefaction of animal and other azotised bodies is a chemical process, by which they are gradually dissipated in a gaseous form, chiefly in that of carbonic acid and ammonia; now to convert the carbon of the animal substance into carbonic acid requires oxygen, and to convert the azote into ammonia requires hydrogen, which are the elements of water. The extreme rapidity of the putrefaction of azotised substances, compared with the gradual decay of non-azotised bodies (such as wood and the like) by the action of oxygen alone, he explains from the general law that substances are much more easily decomposed by the action of two different affinities upon two of their elements, than by the action of only one.

The purgative effect of salts with alkaline bases, when administered in concentrated solutions, is explained from the two following principles: Animal tissues (such as the stomach) do *not* absorb concentrated solutions of alkaline salts; and such solutions *do* dissolve the solids contained in the intestines. The simpler laws into which the complex law is here resolved, are the second of the two foregoing principles combined with a third, namely that the peristaltic contraction acts easily upon substances in a state of solution. The negative general proposition, that animal substances do not absorb these salts, contributes to the explanation by accounting for the absence of a counteracting cause, namely, absorption by the stomach, which in the case of other substances possessed of the requisite chemical properties, interferes to prevent them from reaching the substances which they are destined to dissolve.

§ 4. From the foregoing and similar instances, we may see the importance, when a law of nature previously unknown has been brought to light, or when new light has been thrown upon a known law by experiment, of examining all cases which present the conditions necessary for bringing that law into action; a process fertile in demonstrations of special laws previously unsuspected, and explanations of others already empirically known.

For instance, Faraday discovered by experiment, that voltaic electricity could be evolved from a natural

magnet, provided a conducting body were set in motion at right angles to the direction of the magnet: and, this he found to hold not only of small magnets, but of that great magnet, the earth. The law being thus established experimentally, that electricity is evolved, by a magnet, and a conductor moving at right angles to the direction of its poles, we may now look out for fresh instances in which these conditions meet. Wherever a conductor moves or revolves at right angles to the direction of the earth's magnetic poles, there we may expect an evolution of electricity. In the northern regions, where the polar direction is nearly perpendicular to the horizon, all horizontal motions of conductors will produce electricity; horizontal wheels, for example, made of metal; likewise all running streams will evolve a current of electricity which will circulate round them; and the air thus charged with electricity may be one of the causes of the Aurora Borealis. In the equatorial regions, on the contrary, upright wheels placed parallel to the equator will originate a voltaic circuit, and waterfalls will naturally become electric.

For a second example; it has recently been found, chiefly by the researches of Professor Graham, that gases have a strong tendency to permeate animal membranes, and diffuse themselves through the spaces which such membranes inclose, notwithstanding the presence of other gases in those spaces. Proceeding from this general law, and reviewing a variety of cases in which gases lie contiguous to membranes, we are enabled to demonstrate or to explain the following more special laws: 1st. The human or animal body, when surrounded with any gas not already contained within the body, absorbs it rapidly; such, for instance, as the gases of putrefying matters: which helps to explain malaria. 2nd. The carbonic acid gas of effervescing drinks, evolved in the stomach, permeates its membranes, and rapidly spreads through the system, where, as suggested in a former note, it probably combines with the iron contained in the blood. 3rd. Alcohol taken into the stomach passes into vapour and spreads through the system with great rapidity; (which, combined with the high combustibility of alcohol, or in other words its ready combination with oxygen, may perhaps help to explain the bodily warmth immediately consequent on drinking spirituous liquors.) 4th. In any state of the body in which peculiar gases are formed within it, these will rapidly exhale through all parts of the body; and hence the rapidity with which, in certain states of disease, the surrounding atmosphere becomes tainted. 5th. The putrefaction of the interior parts of a carcase will proceed as rapidly as that of the exterior, from the ready passage outwards of the gaseous products. 6th. The exchange of oxygen and carbonic acid in the lungs is not prevented, but rather promoted, by the intervention of the membrane of the lungs and the coats of the blood vessels between the blood and the air. It is necessary, however, that there should be a substance in the blood with which the oxygen of the air may immediately combine; otherwise instead of passing into the blood, it would permeate the whole organism: and it is necessary that the carbonic acid, as it is formed in the capillaries, should also find a substance in the blood with which it can combine; otherwise it would leave the body at all points, instead of being discharged through the lungs.

§ 5. The following is a deduction which confirms, by explaining, the old but not undisputed empirical generalization, that soda powders weaken the human system. These powders, consisting of a mixture of tartaric acid with bicarbonate of soda, from which the carbonic acid is set free, must pass into the stomach as tartrate of soda. Now, neutral tartrates, citrates, and acetates of the alkalis are found, in their passage through the system, to be changed into carbonates; and to convert a tartrate into a carbonate requires an additional quantity of oxygen, the abstraction of which must lessen the oxygen destined for assimilation with the blood, on the quantity of which the vigorous action of the human system partly depends.

The instances of new theories agreeing with and explaining old empiricisms, are innumerable. All the just remarks made by experienced persons on human character and conduct, are so many special laws, which the general laws of the human mind explain and resolve. The empirical generalizations on which the operations of the arts have usually been founded, are continually justified and confirmed on the one hand, or corrected and improved on the other, by the discovery of the simpler scientific laws on which the efficacy of those operations depends. The effects of the rotation of crops, of the various manures, and other processes of improved agriculture, have been for the first time resolved in our own day into known laws of chemical and organic action, by Davy and Liebig. The processes of the medical art are even now mostly empirical: their efficacy is concluded, in each instance, from a special and most precarious experimental generalization: but as

science advances in discovering the simple laws of chemistry and physiology, progress is made in ascertaining the intermediate links in the series of phenomena, and the more general laws on which they depend; and thus, while the old processes are either exploded, or their efficacy, in so far as real, explained, better processes, founded on the knowledge of proximate causes, are continually suggested and brought into use.(92) Many even of the truths of geometry were generalizations from experience before they were deduced from first principles. The quadrature of the cycloid is said to have been first effected by measurement, or rather by weighing a cycloidal card, and comparing its weight with that of a piece of similar card of known dimensions.

§ 6. To the foregoing examples from physical science, let us add another from mental. The following is one of the simple laws of mind: Ideas of a pleasurable or painful character form associations more easily and strongly than other ideas, that is, they become associated after fewer repetitions, and the association is more durable. This is an experimental law, grounded on the Method of Difference. By deduction from this law, many of the more special laws which experience shows to exist among particular mental phenomena may be demonstrated and explained:--the ease and rapidity, for instance, with which thoughts connected with our passions or our more cherished interests are excited, and the firm hold which the facts relating to them have on our memory; the vivid recollection we retain of minute circumstances which accompanied any object or event that deeply interested us, and of the times and places in which we have been very happy or very miserable; the horror with which we view the accidental instrument of any occurrence which shocked us, or the locality where it took place, and the pleasure we derive from any memorial of past enjoyment; all these effects being proportional to the sensibility of the individual mind, and to the consequent intensity of the pain or pleasure from which the association originated. It has been suggested by the able writer of a biographical sketch of Dr. Priestley in a monthly periodical, that the same elementary law of our mental constitution, suitably followed out, would explain a variety of mental phenomena hitherto inexplicable, and in particular some of the fundamental diversities of human character and genius. Associations being of two sorts, either between synchronous, or between successive impressions; and the influence of the law which renders associations stronger in proportion to the pleasurable or painful character of the impressions, being felt with peculiar force in the synchronous class of associations; it is remarked by the writer referred to, that in minds of strong organic sensibility synchronous associations will be likely to predominate, producing a tendency to conceive things in pictures and in the concrete, richly clothed in attributes and circumstances, a mental habit which is commonly called Imagination, and is one of the peculiarities of the painter and the poet; while persons of more moderate susceptibility to pleasure and pain will have a tendency to associate facts chiefly in the order of their succession, and such persons, if they possess mental superiority, will addict themselves to history or science rather than to creative art. This interesting speculation the author of the present work has endeavoured, on another occasion, to pursue farther, and to examine how far it will avail towards explaining the peculiarities of the poetical temperament. It is at least an example which may serve, instead of many others, to show the extensive scope which exists for deductive investigation in the important and hitherto so imperfect Science of Mind.

§ 7. The copiousness with which I have exemplified the discovery and explanation of special laws of phenomena by deduction from simpler and more general ones, was prompted by a desire to characterize clearly, and place in its due position of importance, the Deductive Method; which in the present state of knowledge is destined henceforth irrevocably to predominate in the course of scientific investigation. A revolution is peaceably and progressively effecting itself in philosophy, the reverse of that to which Bacon has attached his name. That great man changed the method of the sciences from deductive to experimental, and it is now rapidly reverting from experimental to deductive. But the deductions which Bacon abolished were from premisses hastily snatched up, or arbitrarily assumed. The principles were neither established by legitimate canons of experimental inquiry, nor the results tested by that indispensable element of a rational Deductive Method, verification by specific experience. Between the primitive method of Deduction and that which I have attempted to characterize, there is all the difference which exists between the Aristotelian physics and the Newtonian theory of the heavens.

It would, however, be a mistake to expect that those great generalizations, from which the subordinate truths

of the more backward sciences will probably at some future period be deduced by reasoning (as the truths of astronomy are deduced from the generalities of the Newtonian theory,) will be found, in all, or even in most cases, among truths now known and admitted. We may rest assured, that many of the most general laws of nature are as yet entirely unthought of; and that many others, destined hereafter to assume the same character, are known, if at all, only as laws or properties of some limited class of phenomena; just as electricity, now recognised as one of the most universal of natural agencies, was once known only as a curious property which certain substances acquired by friction, of first attracting and then repelling light bodies. If the theories of heat, cohesion, crystallization, and chemical action, are destined, as there can be little doubt that they are, to become deductive, the truths which will then be regarded as the *principia* of those sciences would probably, if now announced, appear quite as novel as the law of gravitation appeared to the cotemporaries of Newton; possibly even more so, since Newton's law, after all, was but an extension of the law of weight--that is, of a generalization familiar from of old, and which already comprehended a not inconsiderable body of natural phenomena. The general laws, of a similarly commanding character, which we still look forward to the discovery of, may not always find so much of their foundations already laid.

These general truths will doubtless make their first appearance in the character of hypotheses; not proved, nor even admitting of proof, in the first instance, but assumed as premisses for the purpose of deducing from them the known laws of concrete phenomena. But this, though their initial, cannot be their final state. To entitle an hypothesis to be received as one of the truths of nature, and not as a mere technical help to the human faculties, it must be capable of being tested by the canons of legitimate induction, and must actually have been submitted to that test. When this shall have been done, and done successfully, premisses will have been obtained from which all the other propositions of the science will thenceforth be presented as conclusions, and the science will, by means of a new and unexpected Induction, be rendered Deductive.

END OF VOL. I.

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FOOTNOTES

1 In the later editions of Archbishop Whately's *Logic* and *Rhetoric* there are some expressions, which, though indefinite, resemble a disclaimer of the opinion here ascribed to him. If I have imputed that opinion to him erroneously, I am glad to find myself mistaken; but he has not altered the passages in which the opinion appeared to me to be conveyed, and which I still think inconsistent with the belief that Induction can be reduced to strict rules.

2 Archbishop Whately.

3 This important theory has recently been called in question by a writer of deserved reputation, Mr. Samuel Bailey; but I do not conceive that the grounds on which it has been admitted as an established doctrine for a century past, have been at all shaken by that gentleman's objections. I have elsewhere said what appeared to me necessary in reply to his arguments (*Westminster Review, for October 1842*.) It may be necessary to add, that some other processes of comparison than those described in the text (but equally the result of experience), appear occasionally to enter into our judgment of distances by the eye.

4 Computation or Logic, chap. ii.

5 In the original, "had, *or had not*." These last words, as involving a subtlety foreign to our present purpose, I have forborne to quote.

6 It would, perhaps, be more correct to say that inflected cases are names and something more; and that this addition prevents them from being used as the subjects of propositions. But the purposes of our inquiry do not demand that we should enter with scrupulous accuracy into similar minutiæ.

7 Notare to mark; *con*notare, to mark *along with*; to mark one thing *with* or *in addition to* another.

8 Archbishop Whately, who in the more recent editions of his *Elements of Logic* has aided in reviving the important distinction treated of in the text, proposes the term "Attributive" as a substitute for "Connotative," (p. 122, 9th ed.) The expression is, in itself, appropriate; but, as it has not the advantage of being connected with any verb, of so markedly distinctive a character as "to connote," it is not, I think, fitted to supply the place of the word Connotative in scientific use.

9 It would be well if this degeneracy of language took place only in the hands of the untaught vulgar; but some of the most remarkable instances are to be found in terms of art, and among technically educated persons, such as English lawyers. *Felony*, for example, is a law term, with the sound of which all are familiar; but there is no lawyer who would undertake to tell what a felony is, otherwise than by enumerating the various offences which are so called. Originally the word felony had a meaning; it denoted all offences, the penalty of which included forfeiture of lands or goods; but subsequent acts of parliament have declared various offences to be felonies without enjoining that penalty, and have taken away the penalty from others which continue nevertheless to be called felonies, insomuch that the acts so called have now no property whatever in common, save that of being unlawful and punishable.

10 Before quitting the subject of connotative names, it is proper to observe, that the first writer who, in our own times, has adopted from the schoolmen the word *to connote*, Mr. Mill, in his *Analysis of the Phenomena of the Human Mind*, employs it in a signification different from that in which it is here used. He uses the word in a sense coextensive with its etymology, applying it to every case in which a name, while pointing directly to one thing, (which is consequently termed its signification,) includes also a tacit reference to some other thing. In the case considered in the text, that of concrete general names, his language and mine are the converse of one another. Considering (very justly) the signification of the name to lie in the attribute, he speaks of the word as *noting* the attribute, and *connoting* the things possessing the attribute. And he describes abstract names as being properly concrete names with their connotation dropped: whereas, in my view, it is the *de*notation which would be said to be dropped, what was previously connoted becoming the whole

signification.

In adopting a phraseology at variance with that which so high an authority, and one which I am less likely than any other person to undervalue, has deliberately sanctioned, I have been influenced by the urgent necessity for a term exclusively appropriated to express the manner in which a concrete general name serves to mark the attributes which are involved in its signification. This necessity can scarcely be felt in its full force by any one who has not found by experience, how vain is the attempt to communicate clear ideas on the philosophy of language without such a word. It is hardly an exaggeration to say, that some of the most prevalent of the errors with which logic has been infected, and a large part of the cloudiness and confusion of ideas which have enveloped it, would, in all probability, have been avoided, if a term had been in common use to express exactly what I have signified by the term *to connote*. And the schoolmen, to whom we are indebted for the greater part of our logical language, gave us this also, and in this very sense. For although some of their general expressions countenance the use of the word in the more extensive and vague acceptation in which it is taken by Mr. Mill, yet when they had to define it specifically as a technical term, and to fix its meaning as such, with that admirable precision which always characterizes their definitions, they clearly explained that nothing was said to be connoted except *forms*, which word may generally, in their writings, be understood as synonymous with *attributes*.

Now, if the word *to connote*, so well suited to the purpose to which they applied it, be diverted from that purpose by being taken to fulfil another, for which it does not seem to me to be at all required; I am unable to find any expression to replace it, but such as are commonly employed in a sense so much more general, that it would be useless attempting to associate them peculiarly with this precise idea. Such are the words, to involve, to imply, &c. By employing these, I should fail of attaining the object for which alone the name is needed, namely, to distinguish this particular kind of involving and implying from all other kinds, and to assure to it the degree of habitual attention which its importance demands.

11 Or rather, all objects except itself and the percipient mind; for, as we shall see hereafter, to ascribe any attribute to an object necessarily implies a mind to perceive it.

12 Philosophy of the Inductive Sciences, vol. i. p. 40.

13 This doctrine is laid down in the clearest and strongest terms by M. Cousin, whose observations on the subject are the more worthy of attention, as, in consequence of the ultra-German and ontological character of his philosophy considered generally, they may be regarded as the admissions of an opponent.

"Nous savons qu'il existe quelque chose hors de nous, parceque nous ne pouvons expliquer nos perceptions sans les rattacher à des causes distinctes de nous-mêmes; nous savons de plus que ces causes, dont nous ne connaissons pas d'ailleurs l'essence, produisent les effets les plus variables, les plus divers, et même les plus contraires, selon qu'elles rencontrent telle nature ou telle disposition du sujet. Mais savons-nous quelque chose de plus? et même, vu le caractère indéterminé des causes que nous concevons dans les corps, y a-t-il quelque chose de plus à savoir? Y a-t-il lieu de nous enquérir si nous percevons les choses telles qu'elles sont? Non évidemment.... Je ne dis pas que le problème est insoluble, *je dis qu'il est absurde et enferme une contradiction*. Nous *ne savons pas ce que ces causes sont en elles-mêmes*, et la raison nous défend de chercher à le connaître: mais il est bien évident *à priori*, qu'*elles ne sont pas en elles-mêmes ce quelles sont par rapport à nous*, puisque la présence du sujet modifie nécessairement leur action. Supprimez tout sujet sentant, il est certain que ces causes agiraient encore puisqu'elles continueraient d'exister; mais elles agiraient autrement; elles seraient encore des qualités et des propriétés, mais qui ne resembleraient à rien de ce que nous connaissons. Le feu ne manifesterait plus aucune des propriétés que nous lui connaissons: que serait-il? C'est ce que nous ne saurons jamais. *C'est d'ailleurs peut-être un problème qui ne répugne pas seulement à la nature de notre esprit, mais à l'essence même des choses.* Quand même en effet on supprimerait par la pensée tous les sujets sentants, il faudrait encore admettre que nul corps ne manifesterait ses propriétés autrement qu'en relation avec un sujet quelconque, et dans ce cas *ses propriétés ne seraient encore que relatives*: en sorte

qu'il me paraît fort raisonnable d'admettre que les propriétés déterminées des corps n'existent pas independamment d'un sujet quelconque, et que quand on demande si les propriétés de la matière sont telles que nous les percevons, il faudrait voir auparavant si elles sont en tant que déterminées, et dans quel sens il est vrai de dire qu'elles sont."--*Cours d'Histoire de la Philosophie Morale au 18me siècle*, 8me leçon.

14 An attempt, indeed, has been made by Reid and others, to establish that although some of the properties we ascribe to objects exist only in our sensations, others exist in the things themselves, being such as cannot possibly be copies of any impression upon the senses; and they ask, from what sensations our notions of extension and figure have been derived? The gauntlet thrown down by Reid was taken up by Brown, who, applying greater powers of analysis than had previously been applied to the notions of extension and figure, showed clearly what are the sensations from which those notions are derived, viz. sensations of touch, combined with sensations of a class previously too little adverted to by metaphysicians, those which have their seat in our muscular frame. Whoever wishes to be more particularly acquainted with this excellent specimen of metaphysical analysis, may consult the first volume of Brown's *Lectures*, or Mill's *Analysis of the Mind*.

On this subject also, M. Cousin may be quoted in favour of conclusions rejected by some of the most eminent thinkers of the school to which he belongs. M. Cousin recognises, in opposition to Reid, the essential *subjectivity* of our conceptions of the primary qualities of matter, as extension, solidity, &c., equally with those of colour, heat, and the remainder of what are called secondary qualities.--*Cours*, ut supra, 9me leçon.

15 Analysis of the Human Mind, i. 126 et seqq.

16 Dr. Whewell (*Of Induction*, p. 10) questions this statement, and asks, "Are we to say that a mole cannot dig the ground, except he has an idea of the ground, and of the snout and paws with which he digs it?" I thought it had been evident that I was here speaking of rational digging, and not of digging by instinct.

17 "From hence also this may be deduced, that the first truths were arbitrarily made by those that first of all imposed names upon things, or received them from the imposition of others. For it is true (for example) that *man is a living creature*, but it is for this reason, that it pleased men to impose both these names on the same thing."--*Computation or Logic*, ch. iii. sect. 8.

18 "Men are subject to err not only in affirming and denying, but also in perception, and in silent cogitation.... Tacit errors, or the errors of sense and cogitation, are made by passing from one imagination to the imagination of another different thing; or by feigning that to be past, or future, which never was, nor ever shall be; as when, by seeing the image of the sun in water, we imagine the sun itself to be there; or by seeing swords, that there has been or shall be, fighting, because it uses to be so for the most part; or when from promises we feign the mind of the promiser to be such and such; or, lastly, when from any sign we vainly imagine something to be signified which is not. And errors of this sort are common to all things that have sense."--*Computation or Logic*, ch. v., sect. 1.

19 Ch. iii. sect. 3.

20 Book iv. ch. vii.

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22 Few among the great names in mental science have met with a harder measure of justice from the present generation than Locke; the unquestioned founder of the analytic philosophy of mind, but whose doctrines were first caricatured, then, when the reaction arrived, cast off by the prevailing school even with contumely, and who is now regarded by one of the conflicting parties in philosophy as an apostle of heresy and sophistry, while among those who still adhere to the standard which he raised, there has been a disposition in later times to sacrifice his reputation in favour of Hobbes; a great writer, and a great thinker for his time, but inferior to Locke not only in sober judgment but even in profundity and original genius. Locke, the most candid of philosophers, and one whose speculations bear on every subject the strongest marks of having been wrought out from the materials of his own mind, has been mistaken for an unworthy plagiarist, while Hobbes has been extolled as having anticipated many of his leading doctrines. He did anticipate many of them, and the present

is an instance in what manner it was generally done. They both rejected the scholastic doctrine of essences; but Locke understood and explained what these supposed essences really were; Hobbes, instead of explaining the distinction between essential and accidental properties, and between essential and accidental propositions, jumped over it, and gave a definition which suits at most only essential propositions, and scarcely those, as the definition of Proposition in general.

23 The always acute and often profound author of *An Outline of Sematology* (Mr. B. H. Smart) justly says, "Locke will be much more intelligible if, in the majority of places, we substitute 'the knowledge of' for what he calls 'the idea of' " (p. 10). Among the many criticisms on Locke's use of the word Idea, this is the only one which, as it appears to me, precisely hits the mark; and I quote it for the additional reason that it precisely expresses the point of difference respecting the import of Propositions, between my view and what I have spoken of as the Conceptualist view of them. Where a Conceptualist says that a name or a proposition expresses our Idea of a thing, I should generally say (instead of our Idea) our Knowledge, or Belief, concerning the thing itself.

24 If we allow a differentia to what is not really a species. For the distinction of Kinds, in the sense explained by us, not being in any way applicable to attributes, it of course follows that although attributes may be put into classes, those classes can be admitted to be genera or species only by courtesy.

25 In the fuller discussion which Archbishop Whately has given to this subject in his later editions, he almost ceases to regard the definitions of names and those of things as, in any important sense, distinct. He seems (9th ed. p. 145) to limit the notion of a Real Definition to one which "explains anything *more* of the nature of the thing than is implied in the name;" (including under the word "implied," not only what the name connotes, but everything which can be deduced by reasoning from the attributes connoted). Even this, as he adds, is usually called, not a Definition, but a Description; and (as it seems to me) rightly so called. A Description, I conceive, can only be ranked among Definitions, when taken (as in the case of the zoological definition of man) to fulfil the true office of a Definition, by declaring the connotation given to a word in some special use, as a term of science or art; which special connotation of course would *not* be expressed by the proper definition of the word in its ordinary employment.

Mr. De Morgan, exactly reversing the doctrine of Archbishop Whately, understands by a Real Definition one which contains *less* than the Nominal Definition, provided only that what it contains is sufficient for distinction. "By *real* definition I mean such an explanation of the word, be it the whole of the meaning or only part, as will be sufficient to separate the things contained under that word from all others. Thus the following, I believe, is a complete definition of an elephant: An animal which naturally drinks by drawing the water into its nose, and then spirting it into its mouth."--*Formal Logic*, p. 36. Mr. De Morgan's general proposition and his example are at variance; for the peculiar mode of drinking of the elephant certainly forms no part of the meaning of the word elephant. It could not be said, because a person happened to be ignorant of this property, that he did not know what an elephant means.

26 In the only attempt which, so far as I know, has been made to refute the preceding argumentation, it is maintained that in the first form of the syllogism,

A dragon is a thing which breathes flame, A dragon is a serpent, Therefore some serpent or serpents breathe flame,

"there is just as much truth in the conclusion as there is in the premisses, or rather, no more in the latter than in the former. If the general name serpent includes both real and imaginary serpents, there is no falsity in the conclusion; if not, there is falsity in the minor premiss."

Let us, then, try to set out the syllogism on the hypothesis that the name serpent includes imaginary serpents. We shall find that it is now necessary to alter the predicates; for it cannot be asserted that an imaginary

creature breathes flame: in predicating of it such a fact, we assert by the most positive implication that it is real and not imaginary. The conclusion must run thus, "Some serpent or serpents either do or are *imagined* to breathe flame." And to prove this conclusion by the instance of dragons, the premisses must be, A dragon is *imagined* as breathing flame, A dragon is a (real or imaginary) serpent: from which it undoubtedly follows, that there are serpents which are imagined to breathe flame; but the major premiss is not a definition, nor part of a definition; which is all that I am concerned to prove.

Let us now examine the other assertion--that if the word serpent stands for none but real serpents, the minor premiss (A dragon is a serpent) is false. This is exactly what I have myself said of the premiss, considered as a statement of fact: but it is not false as part of the definition of a dragon; and since the premisses, or one of them, *must* be false, (the conclusion being so,) the real premiss cannot be the definition, which is true, but the statement of fact, which is false.

27 "Few people" (I have said in another place) "have reflected how great a knowledge of Things is required to enable a man to affirm that any given argument turns wholly upon words. There is, perhaps, not one of the leading terms of philosophy which is not used in almost innumerable shades of meaning, to express ideas more or less widely different from one another. Between two of these ideas a sagacious and penetrating mind will discern, as it were intuitively, an unobvious link of connexion, upon which, though perhaps unable to give a logical account of it, he will found a perfectly valid argument, which his critic, not having so keen an insight into the Things, will mistake for a fallacy turning on the double meaning of a term. And the greater the genius of him who thus safely leaps over the chasm, the greater will probably be the crowing and vain-glory of the mere logician, who, hobbling after him, evinces his own superior wisdom by pausing on its brink, and giving up as desperate his proper business of bridging it over."

28 Contraries: All A is B No A is B

Subtraries: Some A is B Some A is not B

Contradictories: All A is B Some A is not B

Also contradictories: No A is B Some A is B

Respectively subalternate: All A is B; No A is B Some A is B; and Some A is not B

29 His conclusions are, "The first figure is suited to the discovery or proof of the properties of a thing; the second to the discovery or proof of the distinctions between things; the third to the discovery or proof of instances and exceptions; the fourth to the discovery, or exclusion, of the different species of a genus." The reference of syllogisms in the last three figures to the *dictum de omni et nullo* is, in Lambert's opinion, strained and unnatural: to each of the three belongs, according to him, a separate axiom, co-ordinate and of equal authority with that *dictum*, and to which he gives the names of *dictum de diverso* for the second figure, *dictum de exemplo* for the third, and *dictum de reciproco* for the fourth. See part i. or *Dianoiologie*, chap. iv. § 229 *et seqq.*

Mr. De Morgan's "Formal Logic, or the Calculus of Inference, Necessary and Probable," (a work published since the statement in the text was made,) far exceeds in elaborate minuteness Lambert's treatise on the syllogism. Mr. De Morgan's principal object is to bring within strict technical rules the cases in which a conclusion can be drawn from premisses of a form usually classed as particular. He observes, very justly, that from the premisses Most Bs are Cs, most Bs are As, it may be concluded with certainty that some As are Cs, since two portions of the class B, each of them comprising more than half, must necessarily in part consist of the same individuals. Following out this line of thought, it is equally evident that if we knew exactly what proportion the "most" in each of the premisses bear to the entire class B, we could increase in a corresponding degree the definiteness of the conclusion. Thus if 60 per cent of B are included in C, and 70 per cent in A, 30

per cent at least must be common to both; in other words, the number of As which are Cs, and of Cs which are As, must be at least equal to 30 per cent of the class B. Proceeding on this conception of "numerically definite propositions," and extending it to such forms as these:--"45 Xs (or more) are each of them one of 70 Ys," or "45 Xs (or more), are no one of them to be found among 70 Ys," and examining what inferences admit of being drawn from the various combinations which may be made of premisses of this description, Mr. De Morgan establishes universal formulæ for such inferences; creating for that purpose not only a new technical language, but a formidable array of symbols analogous to those of algebra.

Since it is undeniable that inferences, in the cases examined by Mr. De Morgan, can legitimately be drawn, and that the ordinary theory takes no account of them, I will not say that it was not worth while to show in detail how these also could be reduced to formulae as rigorous as those of Aristotle. What Mr. De Morgan has done was worth doing once (perhaps more than once, as a school exercise); but I question if its results are worth studying and mastering for any practical purpose. The practical use of technical forms of reasoning is to bar out fallacies: but the fallacies which require to be guarded against in ratiocination properly so called, arise from the incautious use of the common forms of language; and the logician must track the fallacy into that territory, instead of waiting for it on a territory of his own. While he remains among propositions which have acquired the numerical precision of the Calculus of Probabilities, the enemy is left in possession of the only ground on which he can be formidable. The "quantification of the predicate," an invention to which Sir William Hamilton attaches so much importance as to have raised an angry dispute with Mr. De Morgan respecting its authorship, appears to me, I confess, as an accession to the art of Logic, of singularly small value. It is of course true, that "All men are mortal" is equivalent to "Every man is *some* mortal." But as mankind certainly will not be persuaded to "quantify" their predicates in common discourse, they want a logic which will teach them to reason correctly with propositions in the usual form, by furnishing them with a type of ratiocination to which propositions can be referred, retaining that form. Not to mention that the quantification of the predicate, instead of being a means of bringing out more clearly the meaning of the proposition, actually leads the mind out of the proposition, into another order of ideas. For when we say, All men are mortal, we simply mean to affirm the attribute mortality of all men; without thinking at all of the *class* mortal in the concrete, or troubling ourselves about whether it contains any other beings or not. It is only for some artificial purpose that we ever look at the proposition in the aspect in which the predicate also is thought of as a class-name, either including the subject only, or the subject and something more.

30 Suprà, p. 129.

31 Logic, p. 239 (9th ed.)

32 It is hardly necessary to say, that I am not contending for any such absurdity as that we *actually* "ought to have known" and considered the case of every individual man, past, present, and future, before affirming that all men are mortal: although this interpretation has been, strangely enough, put upon the preceding observations. There is no difference between me and Archbishop Whately, or any other defender of the syllogism, on the practical part of the matter; I am only pointing out an inconsistency in the logical theory of it, as conceived by almost all writers. I do not say that a person who affirmed, before the Duke of Wellington was born, that all men are mortal, *knew* that the Duke of Wellington was mortal; but I do say, that he *asserted* it; and I ask for an explanation of the apparent logical fallacy, of adducing in proof of the Duke of Wellington's mortality, a general statement which presupposes it. Finding no sufficient resolution of this difficulty in any of the writers on Logic, I have attempted to supply one.

33 Of Induction, p. 85.

34 For August 1846.

35 There is a striking passage in the Metaphysics of Aristotle (commencement of chap. iii.) on the necessity of beginning the study of a subject by a clear perception of its difficulties. {~GREEK CAPITAL LETTER

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36 The reviewer misunderstands me when he supposes me to say that "the conclusion must be admitted *before* we can admit the major premiss." What I say is, that there must be ground for admitting it *simultaneously*, or else the major premise is not proved.

37 Mechanical Euclid, pp. 149 *et seqq.*

38 We might, it is true, insert this property into the definition of parallel lines, framing the definition so as to require, *both* that when produced indefinitely they shall never meet, and *also* that any straight line which intersects one of them shall, if prolonged, meet the other. But by doing this we by no means get rid of the assumption; we are still obliged to take for granted the geometrical truth, that all straight lines in the same plane, which have the former of these properties, have also the latter. For if it were possible that they should not, that is, if any straight lines other than those which are parallel according to the definition, had the property of never meeting although indefinitely produced, the demonstrations of the subsequent portions of the theory of parallels could not be maintained.

39 Whewell's *Philosophy of the Inductive Sciences*, i. 130.

40 Dr. Whewell (*Of Induction* p. 84) thinks it unreasonable to contend that we know by experience, that our idea of a line exactly resembles a real line. "It does not appear," he says, "how we can compare our ideas with the realities, since we know the realities only by our ideas." We know the realities (I conceive) by our eyes. Dr. Whewell surely does not hold the "doctrine of perception by means of ideas," which Reid gave himself so much trouble to refute.

Dr. Whewell also says, that it does not appear why this resemblance of ideas to the sensations of which they are copies, should be spoken of as if it were a peculiarity of one class of ideas, those of space. My reply is, that I do not so speak of it. The peculiarity I contend for is only one of degree. All our ideas of sensation of course resemble the corresponding sensations, but they do so with very different degrees of exactness and of reliability. No one, I presume, can recall in imagination a colour or an odour with the same distinctness and accuracy with which almost every one can mentally reproduce an image of a straight line or a triangle. To the extent, however, of their capabilities of accuracy, our recollections of colours or of odours may serve as subjects of experimentation, as well as those of lines and spaces, and may yield conclusions which will be true of their external prototypes. A person in whom, either from natural gift or from cultivation, the impressions of colour were peculiarly vivid and distinct, if asked which of two blue flowers was of the darkest tinge, though he might never have compared the two, or even looked at them together, might be able to give a confident answer on the faith of his distinct recollection of the colours; that is, he might examine his mental pictures, and find there a property of the outward objects. But in hardly any case except that of simple geometrical forms, could this be done by mankind generally, with a degree of assurance equal to that which is given by a contemplation of the objects themselves. Persons differ most widely in the precision of their recollection, even of forms: one person, when he has looked any one in the face for half a minute, can draw an accurate likeness of him from memory; another may have seen him every day for six months, and hardly know whether his nose is long or short. But everybody has a perfectly distinct mental image of a straight line, a circle, or a rectangle. And every one concludes confidently from these mental images to the corresponding outward things.

41 Phil. Ind. Sc. i. 59-61.

42 Ibid. 57.

43 Ibid. 54, 55.

44 "If all mankind had spoken one language, we cannot doubt that there would have been a powerful, perhaps a universal, school of philosophers, who would have believed in the inherent connexion between names and things, who would have taken the sound *man* to be the mode of agitating the air which is essentially communicative of the ideas of reason, cookery, bipedality, &c." De Morgan, *Formal Logic*, p. 246.

45 It would be difficult to name a man more remarkable at once for the greatness and the wide range of his mental accomplishments, than Leibnitz. Yet this eminent man gave as a reason for rejecting Newton's scheme of the solar system, that God *could not* make a body revolve round a distant centre, unless either by some impelling mechanism, or by miracle:--"Tout ce qui n'est pas explicable," says he in a letter to the Abbé Conti, "par la nature des créatures, est miraculeux. Il ne suffit pas de dire: Dieu a fait une telle loi de nature; donc la chose est naturelle. Il faut que la loi soit exécutable par les natures des créatures. Si Dieu donnait cette loi, par exemple, à un corps libre, de tourner à l'entour d'un certain centre, *il faudrait ou qu'il y joignît d'autres corps qui par leur impulsion l'obligeassent de rester toujours dans son orbite circulaire, ou quil mît un ange à ses trousses, ou enfin il faudrait qu'il y concourût extraordinairement*; car naturellement il s'écartera par la tangente."--*Works of Leibnitz*, ed. Dutens, iii. 446.

46 Phil. Ind. Sc. ii. 174.

47 Phil. Ind. Sc. i., 238.

48 Phil. Ind. Sc. i. 237.

49 Ibid. 213.

50 Ibid. 384, 385.

51 In his recent pamphlet (p. 81), Dr. Whewell greatly attenuates the opinion here quoted, reducing it to a surmise "that if we could conceive the composition of bodies distinctly, we might be able to see that it is necessary that the modes of their composition should be definite." The passage in the text asserts that we already see, or may and ought to see, this necessity; giving as the reason, that no other mode of combination is conceivable. That Dr. Whewell should ever have made this statement, is enough for the purposes of my illustration. To what he now says I have nothing to object. Undoubtedly, if we understood the ultimate molecular composition of bodies, we might find that their combining with one another in definite proportions is, in the present order of nature, a *necessary consequence* of that molecular composition; and has thus the only kind of necessity of which, in my view of the subject, any law of nature is susceptible. But in that case, the doctrine would be taken out of the class of axioms altogether. It would be no longer an ultimate principle, but a mere derivative law; regarded as necessary, not because self-evident, but because demonstrable.

52 The *Quarterly Review* for June 1841, contains an article of great ability on Dr. Whewell's two great works, the writer of which maintains, on the subject of axioms, the doctrine advanced in the text, that they are generalizations from experience, and supports that opinion by a line of argument strikingly coinciding with mine. When I state that the whole of the present chapter was written before I had seen the article, (the greater part, indeed, before it was published,) it is not my object to occupy the reader's attention with a matter so unimportant as the degree of originality which may or may not belong to any portion of my own speculations, but to obtain for an opinion which is opposed to reigning doctrines, the recommendation derived from a striking concurrence of sentiment between two inquirers entirely independent of one another. I embrace the opportunity of citing from a writer of the extensive acquirements in physical and metaphysical knowledge and the capacity of systematic thought which the article evinces, passages so remarkably in unison with my own views as the following:--

"The truths of geometry are summed up and embodied in its definitions and axioms.... Let us turn to the axioms, and what do we find? A string of propositions concerning magnitude in the abstract, which are equally true of space, time, force, number, and every other magnitude susceptible of aggregation and subdivision. Such propositions, where they are not mere definitions, as some of them are, carry their inductive origin on the face of their enunciation.... Those which declare that two straight lines cannot inclose a space, and that two straight lines which cut one another cannot both be parallel to a third, are in reality the only ones which express characteristic properties of space, and these it will be worth while to consider more nearly. Now the only clear notion we can form of straightness is uniformity of direction, for space in its ultimate analysis is nothing but an assemblage of distances and directions. And (not to dwell on the notion of continued contemplation, *i.e.*, mental experience, as included in the very idea of uniformity; nor on that of transfer of the contemplating being from point to point, and of experience, during such transfer, of the homogeneity of the interval passed over) we cannot even propose the proposition in an intelligible form, to any one whose experience ever since he was born has not assured him of the fact. The unity of direction, or that we cannot march from a given point by more than one path direct to the same object, is matter of practical experience long before it can by possibility become matter of abstract thought. *We cannot attempt mentally to exemplify the conditions of the assertion in an imaginary case opposed to it, without violating our habitual recollection of this experience, and defacing our mental picture of space as grounded on it.* What but experience, we may ask, can possibly assure us of the homogeneity of the parts of distance, time, force, and measurable aggregates in general, on which the truth of the other axioms depends? As regards the latter axiom, after what has been said it must be clear that the very same course of remarks equally applies to its case, and that its truth is quite as much forced on the mind as that of the former by daily and hourly experience ... *including always, be it observed, in our notion of experience, that which is gained by contemplation of the inward picture which the mind forms to itself in any proposed case, or which it arbitrarily selects as an example--such picture, in virtue of the extreme simplicity of these primary relations, being called up by the imagination with as much vividness and clearness as could be done by any external impression, which is the only meaning we can attach to the word intuition, as applied to such relations.*"

And again, of the axioms of mechanics:--"As we admit no such propositions, other than as truths inductively

collected from observation, even in geometry itself, it can hardly be expected that, in a science of obviously contingent relations, we should acquiesce in a contrary view. Let us take one of these axioms and examine its evidence: for instance, that equal forces perpendicularly applied at the opposite ends of equal arms of a straight lever will balance each other. What but experience, we may ask, in the first place, can possibly inform us that a force so applied will have any tendency to turn the lever on its centre at all? or that force can be so transmitted along a rigid line perpendicular to its direction, as to act elsewhere in space than along its own line of action? Surely this is so far from being self-evident that it has even a paradoxical appearance, which is only to be removed by giving our lever thickness, material composition, and molecular powers. Again we conclude, that the two forces, being equal and applied under precisely similar circumstances, must, if they exert any effort at all to turn the lever, exert equal and opposite efforts: but what *à priori* reasoning can possibly assure us that they *do* act under precisely similar circumstances? that points which differ in place *are* similarly circumstanced as regards the exertion of force? that universal space may not have relations to universal force--or, at all events, that the organization of the material universe may not be such as to place that portion of space occupied by it in such relations to the forces exerted in it, as may invalidate the absolute similarity of circumstances assumed? Or we may argue, what have we to do with the notion of angular movement in the lever at all? The case is one of rest, and of quiescent destruction of force by force. Now how is this destruction effected? Assuredly by the counter-pressure which supports the fulcrum. But would not this destruction equally arise, and by the same amount of counteracting force, if each force simply pressed its own half of the lever against the fulcrum? And what can assure us that it is not so, except removal of one or other force, and consequent tilting of the lever? The other fundamental axiom of statics, that the pressure on the point of support is the sum of the weights ... is merely a scientific transformation and more refined mode of stating a coarse and obvious result of universal experience, viz. that the weight of a rigid body is the same, handle it or suspend it in what position or by what point we will, and that whatever sustains it sustains its total weight. Assuredly, as Mr. Whewell justly remarks, 'No one probably ever made a trial for the purpose of showing that the pressure on the support is equal to the sum of the weights' ... But it is precisely because in every action of his life from earliest infancy he has been continually making the trial, and seeing it made by every other living being about him, that he never dreams of staking its result on one additional attempt made with scientific accuracy. This would be as if a man should resolve to decide by experiment whether his eyes were useful for the purpose of seeing, by hermetically sealing himself up for half an hour in a metal case."

On the "paradox of universal propositions obtained by experience," the same writer says: "If there be necessary and universal truths expressible in propositions of axiomatic simplicity and obviousness, and having for their subject-matter the elements of all our experience and all our knowledge, surely these are the truths which, if experience suggest to us any truths at all, it ought to suggest most readily, clearly, and unceasingly. If it were a truth, universal and necessary, that a net is spread over the whole surface of every planetary globe, we should not travel far on our own without getting entangled in its meshes, and making the necessity of some means of extrication an axiom of locomotion.... There is, therefore, nothing paradoxical, but the reverse, in our being led by observation to a recognition of such truths, as *general* propositions, coextensive at least with all human experience. That they pervade all the objects of experience, must ensure their continual suggestion *by* experience; that they are true, must ensure that consistency of suggestion, that iteration of uncontradicted assertion, which commands implicit assent, and removes all occasion of exception; that they are simple, and admit of no misunderstanding, must secure their admission by every mind."

"A truth, necessary and universal, relative to any object of our knowledge, must verify itself in every instance where that object is before our contemplation, and if at the same time it be simple and intelligible, its verification must be obvious. *The sentiment of such a truth cannot, therefore, but be present to our minds whenever that object is contemplated, and must therefore make a part of the mental picture or idea of that object which we may on any occasion summon before our imagination.... All propositions, therefore, become not only untrue but inconceivable*, if ... axioms be violated in their enunciation."

Another high authority (if indeed it be another authority) may be cited in favour of the doctrine that axioms rest on the evidence of induction. "The axioms of geometry themselves may be regarded as in some sort an

appeal to experience, not corporeal, but mental. When we say, the whole is greater than its part, we announce a general fact, which rests, it is true, on our ideas of whole and part; but, in abstracting these notions, we begin by considering them as subsisting in space, and time, and body, and again, in linear, and superficial, and solid space. Again, when we say, the equals of equals are equal, we mentally make comparisons, in equal spaces, equal times, &c., so that these axioms, however self-evident, are still general propositions so far of the inductive kind, that, independently of experience, they would not present themselves to the mind. The only difference between these and axioms obtained from extensive induction is this, that, in raising the axioms of geometry, the instances offer themselves spontaneously, and without the trouble of search, and are few and simple; in raising those of nature, they are infinitely numerous, complicated, and remote, so that the most diligent research and the utmost acuteness are required to unravel their web and place their meaning in evidence."--SIR J. HERSCHEL's *Discourse on the Study of Natural Philosophy*, pp. 95, 96.

53 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 (in p. 15 of his pamphlet) "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. 8) 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 endeavoured to shew 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.

54 Suprà, p. 214.

55 Phil. Ind. Sc. ii. 213, 214.

56 Ibid.

57 Phil. Ind. Sc. ii. p. 173.

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

59 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 (p. 25): "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 shew 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 successively 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 reconcileable 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' 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; a kind of motion which the vortices did not, and as it was rectilineal, could not, explain. The one explanation, therefore, absolutely excludes the other. Either the planets are not moved by vortices, or they do not move by the law by which heavy bodies fall. It is impossible 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.

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

reconcileable; 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 cannot both be true, still less, surely, can different predictions. Dr. Whewell quarrels (on what ground it is not necessary 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 cannot 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.

60 Of Induction, p. 33.

61 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 said to 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.

62 Infra, chap. xxi.

63 Infra, chap. xxi, xxii.

64 Dr. Whewell (*Of Induction*, p. 16) will not allow these and similar erroneous opinions 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 defence, 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 what really gives them 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 *à priori* prejudice does not prevent the erroneous opinion from being sincerely regarded as a legitimate conclusion from experience; but is, on the contrary, the very thing which predisposes the mind to that interpretation of experience.

Thus much in defence 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.

65 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, (*Prospective Review* for February 1850,) 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 cannot 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 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; although that was the element of active force. 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.

66 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, since we should still have to enumerate specially the negative conditions of each phenomenon, instead of regarding them as implicitly contained in the positive laws of the various other agencies in nature.

67 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.

68 The reviewer of Dr. Whewell in the *Quarterly Review*.

69 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. ch. 2). In the meantime 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.

70 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 *à 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 cannot *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 anything *more* than antecedence and consequence.

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

72 Prospective Review for February 1850.

73 Vide supra, p. 267, note.

74 In combating the theory, that Volition is the universal cause, I have purposely abstained from one of the strongest positive arguments against it--that volitions themselves obey causes, and even external causes, namely, the inducements, or motives, which determine the will to act; because an objector might say that to employ this argument would be begging the question against the freedom of the will. Though it is not begging the question to affirm a doctrine, referring elsewhere for the proof of it, I am unwilling without necessity to build any part of my reasoning on a proposition which I am aware that those opposed to me in the present discussion do not admit.

75 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.

76 Unless, indeed, the consequent was generated not by the antecedent, but by the means we 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.

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

78 For this speculation I am indebted to Mr. Alexander Bain.

79 This view of the necessary coexistence 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.

80 Pp. 159-162.

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

82 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 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 analyse, 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 *à 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 favourable as this to Nature's experimental talents, her experiment is of little value except in corroboration of a conclusion already attained through other means.

83 Discourse, pp. 156-8, and 171.

84 Outlines of Astronomy, p. 584.

85 Dr. Whewell, in his reply, expresses a very unfavourable 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 (pp. 44-6):

"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 shew 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. 66.)

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 so to reduce it without knowing *to what*, 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 cannot 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 practised 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 cannot 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 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 burnt; this is B C; I touch it, and am burnt; 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 shewing, the pyramid of science is supplied with its base. In vain he attempts to escape from this truth 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. 47), nor must any of them be drawn from mental and social subjects (p. 53), nor from ordinary observation and practical life (pp. 11-15). 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 hypotheses; 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 takes advantage of their absence to represent

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, (see, on this point, the second chapter of the present Book) fall under the Method of Agreement. The law of "falling bodies," namely that they describe spaces proportional to the squares of the times, was historically a 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 favour with Dr. Whewell; for it is the peculiarity of his system not to recognise, 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.

86 Ante, p. 378.

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

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

89 There is no danger of confounding this acceptation of the term with the peculiar employment of the phrase "tangential force" in the theory of the planetary perturbations.

90 Suprà, p. 420.

91 As corroborating the opinion that the protoxide of iron in the venous blood is only partially carbonated, the fact has been suggested, that the system shows great readiness to absorb an extra quantity of carbonic acid, as furnished in effervescing drinks. In such cases the acid must combine with something, and that something is not improbably the free protoxide. It would be worth ascertaining whether the protoxide itself or its carbonate has the greatest facility in absorbing oxygen and turning itself into hydrated peroxide in the lungs. If the carbonate, then the beneficial effect, on the animal economy, of drinks which give an artificial supply of carbonic acid to the system, would be, to that extent, deductively established.

92 It was an old generalization in surgery, that tight bandaging had a tendency to prevent or dissipate local

inflammation. This sequence, being, in the progress of physiological knowledge, resolved into more general laws, led to the important surgical invention made by Dr. Arnott, the treatment of local inflammation and tumours by means of an equable pressure, produced by a bladder partially filled with air. The pressure, by keeping back the blood from the part, prevents the inflammation, or the tumour, from being nourished; in the case of inflammation, it removes the stimulus, which the organ is unfit to receive: in the case of tumours, by keeping back the nutritive fluid it causes the absorption of matter to exceed the supply, and the diseased mass is gradually absorbed and disappears.

END OF THE PROJECT GUTENBERG EBOOK A SYSTEM OF LOGIC, RATIOCINATIVE AND INDUCTIVE (VOL. 1 OF 2)

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