# CHAPTER IX.

MISCELLANEOUS EXAMPLES OF THE FOUR METHODS.

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

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

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

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

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

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

Let us now bring our conclusion to the test of the Method of Difference. Setting out from the cases already mentioned, in which the antecedent is the presence of substances forming with the tissues a compound incapable of putrefaction, (and *a fortiori* incapable of the chemical actions which constitute life,) and the consequent is death, either of the whole organism, or of some portion of it; let us compare with these cases other cases, as much resembling them as possible, but in which that effect is not produced. And, first, "many insoluble basic salts of arsenious acid are known not to be poisonous. The substance called alkargen, discovered by Bunsen, which contains a very large quantity of arsenic, and approaches very closely in composition to the organic arsenious compounds found in the body, has not the slightest injurious action upon the organism." Now when these substances are brought into contact with the tissues in any way, they do not combine with them; they do not arrest their progress to decomposition. As far, therefore, as these instances go, it appears that when the effect is absent, it is by reason of the absence of that antecedent which we had already good ground for considering as the proximate cause.

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

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

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

Sec. 2. Let the object be[34] to ascertain the law of what is termed *induced* electricity; to find under what conditions any electrified body, whether positively or negatively electrified, gives rise to a contrary electric state in some other body adjacent to it.

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

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

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

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

The law thus strongly indicated admits of corroboration by the Method of Concomitant Variations. The Leyden jar is capable of receiving a much higher charge than can ordinarily be given to the conductor of an electrical machine. Now in the case of the Leyden jar, the metallic surface which receives the induced electricity is a conductor exactly similar to that which receives the primary charge, and is therefore as susceptible of receiving and retaining the one electricity, as the opposite surface of receiving and retaining the other; but in the machine, the neighbouring body which is to be oppositely electrified is the surrounding atmosphere, or any body casually brought near to the conductor; and as these are generally much inferior in their capacity of becoming electrified, to the conductor itself, their limited power imposes a corresponding limit to the capacity of the conductor for being charged. As the capacity of the neighbouring body for supporting the opposition increases, a higher charge becomes possible: and to this appears to be owing the great superiority of the Leyden jar.

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

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

It thus appears by the combined evidence of the Method of Agreement, the Method of Concomitant Variations, and the most rigorous form of the Method of Difference, that neither of the two kinds of electricity can be excited without an equal excitement of the other and opposite kind: that both are effects of the same cause; that the possibility of the one is a condition of the possibility of the other, and the quantity of the one an impassable limit to the quantity of the other. A scientific result of considerable interest in itself, and illustrating those three methods in a manner both characteristic and easily intelligible.[35]

Sec. 3. Our third example shall be extracted from Sir John Herschel's *Discourse on the Study of Natural Philosophy*, a work replete with happily-selected exemplifications of inductive processes from almost every department of physical science, and in which alone, of all books which I have met with, the four methods of induction are distinctly recognised, though not so clearly characterized and defined, nor their correlation so fully shown, as has appeared to me desirable. The present example is described by Sir John Herschel as "one of the most beautiful specimens" which can be cited "of inductive experimental inquiry lying within a moderate compass;" the theory of dew, first promulgated by the late Dr. Wells, and now universally adopted by scientific authorities. The passages in inverted commas are extracted verbatim from the Discourse.[36]

"Suppose *dew* were the phenomenon proposed, whose cause we would know. In the first place" we must determine precisely what we mean by dew: what the fact really is, whose cause we desire to investigate. "We must separate dew from rain, and the moisture of fogs, and limit the application of the term to what is really meant, which is the spontaneous appearance of moisture on substances exposed in the open air when no rain or *visible* wet is falling." This answers to a preliminary operation which will be characterized in the ensuing book, treating of operations subsidiary to induction.[37]

"Now, here we have analogous phenomena in the moisture which bedews a cold metal or stone when we breathe upon it; that which appears on a glass of water fresh from the well in hot weather; that which appears on the inside of windows when sudden rain or hail chills the external air; that which runs down our walls when, after a long frost, a warm moist thaw comes on." Comparing these cases, we find that they all contain the phenomenon which was proposed as the subject of investigation. Now "all these instances agree in one point, the coldness of the object dewed, in comparison with the air in contact with it." But there still remains the most important case of all, that of nocturnal dew: does the same circumstance exist in this case? "Is it a fact that the object dewed is colder than the air? Certainly not, one would at first be inclined to say; for what is to *make* it so? But ... the experiment is easy: we have only to lay a thermometer in contact with the dewed substance, and hang one at a little distance above it, out of reach of its influence. The experiment has been therefore made, the question has been asked, and the answer has been invariably in the affirmative. Whenever an object contracts dew, it *is* colder than the air."

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

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

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

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

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

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

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

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

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

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

And, finally, even on that great scale we are able to verify the result. The case is one of those rare cases, as we have shown them to be, in which nature works the experiment for us in the same manner in which we ourselves perform it; introducing into the previous state of things a single and perfectly definite new circumstance, and manifesting the effect so rapidly that there is not time for any other material change in the pre-existing circumstances. "It is observed that dew is never copiously deposited in situations much screened from the open sky, and not at all in a cloudy night; but *if the clouds withdraw even for a few minutes, and leave a clear opening, a deposition of dew presently begins*, and goes on increasing.... Dew formed in clear intervals will often even evaporate again when the sky becomes thickly overcast." The proof, therefore, is complete, that the presence or absence of an uninterrupted communication with the sky causes the deposition or non-deposition of dew. Now, since a clear sky is nothing but the absence of clouds, and it is a known property of clouds, as of all other bodies between which and any given object nothing intervenes but an elastic fluid, that they tend to raise or keep up the superficial temperature of the object by radiating heat to it, we see at once that the disappearance of clouds will cause the surface to cool; so that Nature, in this case, produces a change in the antecedent by definite and known means, and the consequent follows accordingly: a natural experiment which satisfies the requisitions of the Method of Difference.[38]

The accumulated proof of which the Theory of Dew has been found susceptible, is a striking instance of the fulness of assurance which the inductive evidence of laws of causation may attain, in cases in which the invariable sequence is by no means obvious to a superficial view.

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

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

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

1st. Paralysed muscles have greater irritability than healthy muscles. Now, paralysed muscles are later in assuming the cadaveric rigidity than healthy muscles, the rigidity lasts longer, and putrefaction sets in later and proceeds more slowly.

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

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

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

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

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

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

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

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

It has long been observed that in some cases of death by lightning, cadaveric rigidity either does not take place at all, or is of such extremely brief duration as to escape notice, and that in these cases putrefaction is very rapid. In other cases, however, the usual cadaveric rigidity appears. There must be some difference in the cause, to account for this difference in the effect. Now "death by lightning may be the result of, 1st, a syncope by fright, or in consequence of a direct or reflex influence of lightning on the par vagum; 2ndly, hemorrhage in or around the brain, or in the lungs, the pericardium, &c.; 3rdly, concussion, or some other alteration in the brain;" none of which phenomena have any known property capable of accounting for the suppression, or almost suppression, of the cadaveric rigidity. But the cause of death may also be that the lightning produces "a violent convulsion of every muscle in the body," of which, if of sufficient intensity, the known effect would be that "muscular irritability ceases almost at once." If Dr. Brown-Sequard's generalization is a true law, these will be the very cases in which rigidity is so much abridged as to escape notice; and the cases in which, on the contrary, rigidity takes place as usual, will be those in which the stroke of lightning operates in some of the other modes which have been enumerated. How, then, is this brought to the test? By experiments not on lightning, which cannot be commanded at pleasure, but on the same natural agency in a manageable form, that of artificial galvanism. Dr. Brown-Sequard galvanized the entire bodies of animals immediately after death. Galvanism cannot operate in any of the modes in which the stroke of lightning may have operated, except the single one of producing muscular convulsions. If, therefore, after the bodies have been galvanized, the duration of rigidity is much shortened and putrefaction much accelerated, it is reasonable to ascribe the same effects when produced by lightning, to the property which galvanism shares with lightning, and not to those which it does not. Now this Dr. Brown-Sequard found to be the fact. The galvanic experiment was tried with charges of very various degrees of strength; and the more powerful the charge, the shorter was found to be the duration of rigidity, and the more speedy and rapid the putrefaction. In the experiment in which the charge was strongest, and the muscular irritability most promptly destroyed, the rigidity only lasted fifteen minutes. On the principle, therefore, of the Method of Concomitant Variations, it maybe inferred that the duration of the rigidity depends on the degree of the irritability; and that if the charge had been as much stronger than Dr. Brown-Sequard's strongest, as a stroke of lightning must be stronger than any electric shock which we can produce artificially, the rigidity would have been shortened in a corresponding ratio, and might have disappeared altogether. This conclusion having been arrived at, the case of an electric shock, whether natural or artificial, becomes an instance in addition to all those already ascertained, of correspondence between the irritability of the muscle and the duration of rigidity.

All these instances are summed up in the following statement:--"That when the degree of muscular irritability at the time of death is considerable, either in consequence of a good state of nutrition, as in persons who die in full health from an accidental cause, or in consequence of rest, as in cases of paralysis, or on account of the influence of cold, cadaveric rigidity in all these cases sets in late and lasts long, and putrefaction appears late, and progresses slowly:" but "that when the degree of muscular irritability at the time of death is slight, either in consequence of a bad state of nutrition, or of exhaustion from over-exertion, or from convulsions caused by disease or poison, cadaveric rigidity sets in and ceases soon, and putrefaction appears and progresses quickly." These facts present, in all their completeness, the conditions of the Joint Method of Agreement and Difference. Early and brief rigidity takes place in cases which agree only in the circumstance of a low state of muscular irritability. Rigidity begins late and lasts long in cases which agree only in the contrary circumstance, of a muscular irritability high and unusually prolonged. It follows that there is a connexion through causation between the degree of muscular irritability after death, and the tardiness and prolongation of the cadaveric rigidity. This investigation places in a strong light the value and efficacy of the Joint Method. For, as we have already seen, the defect of that Method is, that like the Method of Agreement, of which it is only an improved form, it cannot prove causation. But in the present case (as in one of the steps in the argument which led up to it) causation is already proved; since there could never be any doubt that the rigidity altogether, and the putrefaction which follows it, are caused by the fact of death: the observations and experiments on which this rests are too familiar to need analysis, and fall under the Method of Difference. It being, therefore, beyond doubt that the aggregate antecedent, the death, is the actual cause of the whole train of consequents, whatever of the circumstances attending the death can be shown to be followed in all its variations by variations in the effect under investigation, must be the particular feature of the fact of death on which that effect depends. The degree of muscular irritability at the time of death fulfils this condition. The only point that could be brought into question, would be whether the effect depended on the irritability itself, or on something which always accompanied the irritability: and this doubt is set at rest by establishing, as the instances do, that by whatever cause the high or low irritability is produced, the effect equally follows; and cannot, therefore, depend upon the causes of irritability, nor upon the other effects of those causes, which are as various as the causes themselves; but upon the irritability, solely.

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

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

"For example: the return of the comet predicted by Professor Encke, a great many times in succession, and the general good agreement of its calculated with its observed place during any one of its periods of visibility, would lead us to say that its gravitation towards the sun and planets is the sole and sufficient cause of all the phenomena of its orbitual motion; but when the effect of this cause is strictly calculated and subducted from the observed motion, there is found to remain behind a *residual phenomenon*, which would never have been otherwise ascertained to exist, which is a small anticipation of the time of its reappearance, or a diminution of its periodic time, which cannot be accounted for by gravity, and whose cause is therefore to be inquired into. Such an anticipation would be caused by the resistance of a medium disseminated through the celestial regions; and as there are other good reasons for believing this to be a *vera causa*," (an actually existing antecedent,) "it has therefore been ascribed to such a resistance.[39]

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

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

"Many of the new elements of chemistry have been detected in the investigation of residual phenomena. Thus Arfwedson discovered lithia by perceiving an excess of weight in the sulphate produced from a small portion of what he considered as magnesia present in a mineral he had analysed. It is on this principle, too, that the small concentrated residues of great operations in the arts are almost sure to be the lurking places of new chemical ingredients: witness iodine, brome, selenium, and the new metals accompanying platina in the experiments of Wollaston and Tennant. It was a happy thought of Glauber to examine what everybody else threw away."[40]

"Almost all the greatest discoveries in Astronomy," says the same author,[41] "have resulted from the consideration of residual phenomena of a quantitative or numerical kind.... It was thus that the grand discovery of the precession of the equinoxes resulted as a residual phenomenon, from the imperfect explanation of the return of the seasons by the return of the sun to the same apparent place among the fixed stars. Thus, also, aberration and nutation resulted as residual phenomena from that portion of the changes of the apparent places of the fixed stars which was left unaccounted for by precession. And thus again the apparent proper motions of the stars are the observed residues of their apparent movements outstanding and unaccounted for by strict calculation of the effects of precession, nutation, and aberration. The nearest approach which human theories can make to perfection is to diminish this residue, this *caput mortuum* of observation, as it may be considered, as much as practicable, and, if possible, to reduce it to nothing, either by showing that something has been neglected in our estimation of known causes, or by reasoning upon it as a new fact, and on the principle of the inductive philosophy ascending from the effect to its cause or causes."

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

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

Sec. 6. Dr. Whewell has expressed 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:--[42]

"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 formulae 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 formulae through the history of the sciences, as they have really grown up; and show us that these four methods have been operative in their formation; or that any light is thrown upon the steps of their progress by reference to these formulae?"

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

There is a striking similarity between the objections here made against Canons of Induction, and what was alleged, in the last century, by as able men as Dr. Whewell, against the acknowledged Canon of Ratiocination. Those who protested against the Aristotelian Logic said of the Syllogism, what Dr. Whewell says of the Inductive Methods, that it "takes for granted the very thing which is most difficult to discover, the reduction of the argument to formulae such as are here presented to us." The grand difficulty, they said, is to obtain your syllogism, not to judge of its correctness when obtained. On the matter of fact, both they and Dr. Whewell are right. The greatest difficulty in both cases is first that of obtaining the evidence, and next, of reducing it to the form which tests its conclusiveness. But if we try to reduce it without knowing *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 processes reducible to one or other of those methods.

This difference between us accounts for the dissatisfaction which my examples give him; for I did not select them with a view to satisfy any one who required to be convinced that observation and experiment are modes of acquiring knowledge: I confess that in the choice of them I thought only of illustration, and of facilitating the *conception* of the Methods by concrete instances. If it had been my object to justify the processes themselves as means of investigation, there would have been no need to look far off, or make use of recondite or complicated instances. As a specimen of a truth ascertained by the Method of Agreement, I might have chosen the proposition "Dogs bark." This dog, and that dog, and the other dog, answer to A B C, A D E, A F G. The circumstance of being a dog, answers to A. Barking answers to *a*. As a truth made known by the Method of Difference, "Fire burns" might have sufficed. Before I touch the fire I am not 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 showing, the pyramid of science is supplied with its base. In vain he attempts to escape from this conclusion by laying the most arbitrary restrictions on the choice of examples admissible as instances of Induction: they must neither be such as are still matter of discussion (p. 265), nor must any of them be drawn from mental and social subjects (p. 269), nor from ordinary observation and practical life (pp. 241-247). They must be taken exclusively from the generalizations by which scientific thinkers have ascended to great and comprehensive laws of natural phenomena. Now it is seldom possible, in these complicated inquiries, to go much beyond the initial steps, without calling in the instrument of Deduction, and the temporary aid of 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 is misled by their absence into representing the Experimental Methods as serving no purpose in scientific investigation; forgetting that if those methods had not supplied the first generalizations, there would have been no materials for his own conception of Induction to work upon.

His challenge, however, to point out which of the four methods are exemplified in certain important cases of scientific inquiry, is easily answered. "The planetary paths," as far as they are a case of induction at all,[43] 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 *disprove* it, he is content: at least until a simpler hypothesis, equally consistent with experience, presents itself. If this be Induction, doubtless there is no necessity for the four methods. But to suppose that it is so, appears to me a radical misconception of the nature of the evidence of physical truths.

So real and practical is the need of a test for induction, similar to the syllogistic test of ratiocination, that inferences which bid defiance to the most elementary notions of inductive logic are put forth without misgiving by persons eminent in physical science, as soon as they are off the ground on which they are conversant with the facts, and not reduced to judge only by the arguments; and as for educated persons in general, it may be doubted if they are better judges of a good or a bad induction than they were before Bacon wrote. The improvement in the results of thinking has seldom extended to the processes; or has reached, if any process, that of investigation only, not that of proof. A knowledge of many laws of nature has doubtless been arrived at, by framing hypotheses and finding that the facts corresponded to them; and many errors have been got rid of by coming to a knowledge of facts which were inconsistent with them, but not by discovering that the mode of thought which led to the errors was itself faulty, and might have been known to be such independently of the facts which disproved the specific conclusion. Hence it is, that while the thoughts of mankind have on many subjects worked themselves practically right, the thinking power remains as weak as ever: and on all subjects on which the facts which would check the result are not accessible, as in what relates to the invisible world, and even, as has been seen lately, to the visible world of the planetary regions, men of the greatest scientific acquirements argue as pitiably as the merest ignoramus. For though they have made many sound inductions, they have not learnt from them (and Dr. Whewell thinks there is no necessity that they should learn) the principles of inductive *evidence*.