THE SOUTHERN
PRESBYTERIAN REVIEW.

VOL. XXXIV.—NO. 3.

JULY, MDCCCLXXXIII.

ARTICLE I.

THE COUNCIL OF APOSTLES AND PRESBYTERS AT JERUSALEM.

The Acts of the Apostles is the first chapter of the history of the Christian Church, the transition chapter from the history of the Church under the Jewish dispensation to the history of the same Church under the Christian dispensation. Although not designed to teach ecclesiastical polity, yet the principles and precedents furnished therein by apostolic precepts and practices are so numerous and specific, that it alone would be sufficient to reveal the constitution of the Church, if there were access to no other inspired writings. Notwithstanding the fact that the voice of inspiration was never heard beyond the first century, yet the advocates of Prelacy and Congregationalism appeal alike to the testimony of the post-apostolic age in support of their respective systems. Thus Mr. Litton, of the Episcopal Church, quoted by Bannerman in his "Church of Christ," makes the remarkable statement that the claims of Episcopacy are strong so long as the appeal is to the post-apostolic age, and become weak only when the appeal is made to Scripture. Canon Venables, in the *Encyclopædia Britannica*, article *Episcopacy*, furnishes the following still more explicit testimony to the same effect: "It may be desirable here to remove the confusion which may be produced by tion, guided entirely by the precepts of Christ, and depending on the influence of the Spirit, can use the language of the Council, "It seemed good to the Holy Ghost and to us." These principles exhibited by the Council at Jerusalem are also the principles of the Presbyterian system, the principles of *jure divino* Church government, the principles sustained by the word of God, the principles which shall yet triumph in the name of eternal truth.

S. L. MORRIS.

ARTICLE II.

THE NATURE OF PHYSICAL CAUSES AND THEIR INDUCTION.

In our previous sketch of the History of Inductive Reasonings, we found that the chief (and the difficult) question, the great problem of this species of logic, which continually emerged, was this: How does the inference seemingly made from the some, or the many, to the all, become valid for the all?

The settlement of this, as of the other fundamental doctrines of logic, must proceed upon right postulates as to psychology, and especially as to its highest branch, the original powers of the reason. In our criticism of the Sensualistic Philosophy of the Nineteenth Century, a parallel question as to the Deductive Logic is considered (see pp. 265–272). That question was the old one between the assailants and defenders of the utility and fruitfulness of the syllogism, with which the students of philosophy are acquainted. The followers of Locke, from his day to ours, have argued that, since a syllogism which concludes more in its third proposition than is predicated in its major premise, is confessedly faulty, all such reasonings must inevitably be either sophisms, or worthless, only teaching us what we must have known before in order to state our premise. Yet we saw Mill, after echoing this objection, confessing, what all men's common sense must concede, that the syllogism is the full expression to which all deductive reasoning is reduced. How was this paradox to be solved? It was shown that the solution is in recognising the *a priori* necessary and universal judgments of the reason. Admit that the mind is entitled to other judgments than the empirical, the intuitive namely, and that they are universal, then the synthesis of truths becomes a valid and fruitful source of new knowledge.

A similar resort to the doctrines of a true psychology must be made, again, to explain the Inductive Logic. This necessity has been disclaimed, on the ground that logic is a critical art, whose whole and only business is to test the validity, not of the contents, but of the forms of our elenchtic thought. This might be admitted; and yet it would remain true that these processes, which it is the business of logic to criticise, are psychological processes, and that the critical acts are also psychological processes. Moreover, as in the world of matter, the substance determines the form, so in the realm of thought, it is the quality of the contents of thought which determines the logical framework. The science of logic, therefore, must be grounded in a correct psychology.

That psychology must not be the sensationalist. We must hold that the mind has original powers of judging *a priori* necessary truths; powers which, although they may be awakened to exercise on occasion of some empirical perception, yet owe the validity of the judgments formed, not to sense-perception, but to the mind's own constitutive laws. This, then, is the metaphysical doctrine assumed as the basis of this discussion: that while the senses alone give us our individual idea of objective things, it is the original power of the reason which gives us our universal necessary judgments about objective things and their relations; and these same powers furnish the forms according to which we connect them into general knowledge. Those necessary and universal truths are primitive judgments, intuitively seen to be true, and not dependent for their authority upon the confirmation of observed instances, be they many or few. For these first truths and laws of the reason must be, in their order of production (though not in their date), prior to the observations of the senses and to all deductions therefrom, because they are necessary to construe the individual perceptions intelligibly, and to connect them for any purposes of reasoning. But it is our purpose here to postulate, and not to argue, this view of the mind's powers. For the latter, the reader must be referred to the work mentioned above ("Sens. Phil. of the 19th Cent. Considered," Chap. X. and XI.).

We have seen J. S. Mill's correct position, that the *law of causation* is the foundation of every inductive demonstration. We have also seen his inconsistent assertion, that our belief in this law is the result of an induction from experience. We have proved, on the contrary, that it is a necessary intuition of the reason. Whenever we observe a *phenomenon* or a new existence, the law of the reason ensures our assigning for it an adequate cause. It is impossible for us to think a thing or event as arising out of nothing. To think it as producing itself, would be the contradiction of thinking it acted before it existed. Nor can we avoid ascribing to the cause *power* efficient of the effect. The old objection, that we have no right to assume anything else than what the senses observe, a regular or uniform sequence between a certain antecedent and a certain consequent, is worthless to any one who has learned the true doctrine: that the reason is itself a source, and not a mere passive recipient, of cognitions. As, when sense-perception gives us only a cluster of properties belonging to body, the reason must supply the supersensuous notion of substance underlying and sustaining them, so when the senses perceive a cause preceding its effect, the reason compels us to supply the rational notion of efficient power in the cause. It is this, and this alone, which enables and qualifies the antecedent to be cause. And this power must be thought as efficient of the effect. This judgment involves the further belief that, wherever the cause is present, under the same conditions, the efficiency of its power ensures the same effect. Such is obviously the nature of the necessary judgment: "Same causes, same effects." A simple examination of our consciousness convinces us that our rational notion of substance involves the assurance of its continuity of being and permanency. As the rise of that substance *ex nihilo*, without any cause, is a proposition which cannot be rationally thought, so the cessation of that substance's continuity of being, or its return into *nihil* without a cause efficient of its destruction, is equally incredible. This intuitive confidence in the permanency of true substance, as thus defined, is not an inference from any observations, but a phase of the intuition, a source and premise of all our reasonings about substances; and a regulative law for construing every observation experiences give us about them. So we have a similar intuitive confidence in the persistency and uniformity of power, wherever it inheres. So long as power qualifies any being, it is, in its own nature, efficient of the same effect which it is once seen to produce. If we see the agent and the recipient of the effect again present, and do not witness the rise of the same effect, we intuitively and necessarily believe that some other power, whether visible or invisible, is intervening to modify or counteract the known power. This is the explanation of our belief in the "uniformity of nature" when the belief is legitimate. Nature is uniform just so far as the same powers are present, and her uniformities are nothing but the necessary results of the permanency of substances and powers. What we call laws of nature are only the regular methods of the actions of natural powers. We believe in those laws, only because we intuitively judge that each power or energy is, under the same circumstances, efficient of the same effects.

But this conception of regular laws in nature implies an assurance not only of the permanency of substances, but of their essential properties. That substances have two classes of properties, distinguished as *attributa* and *accidentia*, is obvious; and it is according to their permanency or mutability that we ascribe a quality to the one class or the other. How is it that we are authorised to entertain this assurance of the permanency of essential properties? The answer is, *because these properties make themselves known to our reason as powers.* If we reflect, we see that what we call a property of a body is only revealed to us by its emission of a power, producing an effect either on some other body, or on our own percipient senses, and through them on our own spirits. This truth has been seen by Dr. McCosh, for instance (in his "Divine Government, Physical and Moral," p. 78). The evidence assigned for the proposition seems inadequate: that we observe no body acts on itself, but only on another body in a certain relation to itself. The same writer, very singularly, excepts from his assertion those properties which affect our senses. Of all the properties of external things, he should have said that those which affect our senses directly, are most certainly powers. For it is only by some effect on our senses, propagating a perception, that we learn an effect has been produced on another body. What is perception? How do we convince ourselves of the reality of the external world? Consciousness, a subjective faculty, can of course only testify to the subjective part of the perceptive function. What, then, is the rational ground of that judgment of relation which, as we know, we all make between the perceptive cognition and the external source? Reflection convinces us that this ground is in the necessary and *intuitive judgment of cause.* We are conscious of a perception; we are also conscious we did not affect ourselves with it. But there can be no effect without a cause; therefore the object perceived must be a reality. It is frequently said that we derive, or at least we first see, the rational notion of power and efficiency in our own conscious volition; that we are conscious of the will to emit efficiency; that we see the effect, and that we thus form the notion of efficient power in cause. We have no disposition to dispute the fact that this may be one of the occasions upon which the reason presents her intuitive notion of power. But, whatever the change which she may observe, constituting a new *phenomenon* or state, whether in the subjective or objective sphere, she must supply the notion of cause and of efficient power. For the necessary law of her thinking is, *ex nihilo nihil.* The new effect could not have been, except there had preceded a sufficient cause. But when is cause sufficient? Only when it possesses power efficient of the new change.

Now, then, the first cognition which the mind can have of any objective thing, is *through experiencing an effect therefrom.* Is it not obvious, thence, that what we call properties of things are *only known to us as powers?* They are, simply, what are *able to affect us* with the perceptions. And since every perception is an effect, we only learn that any body has the property (or power) of affecting another body, by experiencing its power of affecting us. Hence, we should say that we know the properties of bodies which affect our senses as powers primarily; and those which we see affecting other bodies we know as also powers secondarily. Instead of saying that properties are powers, it would be more correct to say that powers are the only true properties. The notion of power is in order to the idea of property. Here, then, is the ground on which we expect a permanency in any essential property, as immutable as that which we intuitively ascribe to substance; it is because "the same causes produce the same effects."

But there are properties which are not permanent; and yet they can produce effects on us, and on other bodies. The distinction of "attributes" and "accidents" made by the scholastics is just. The solidity of congealed water, for instance, is certainly not an essential property of that substance; yet it has power to affect our tactual sense, and it also has a power of impact on other bodies which the liquid has not. Here is an apparent inconsistency—that we should infer the permanency of essential properties from the fact that they are causes; that the same causes produce the same effects—and yet concede power to properties which are not permanent. But the inconsistency is only seeming. The explanation is, that the change or state which was just now an effect, may in turn become a cause, and may not only depend on its cause, but have another effect depending on it. While its own prior cause propagates it, it may also propagate its effect; with the suspension of the action of its cause, it and its effect cease. The original cause has thus its progeny, not only of the first, but of the second and subsequent generations. Now, what is an "*accidens*," a property not permanent, except a mutable effect of some other property, which is a permanent cause?—mutable, because, while the power of essential property has no change, the conditions for its action may change. While the more original power or powers of the essential property is acting, its effect, the accidental property, is propagated; and this in turn may become cause, so long as it subsists. Thus, solidity is not an essential property of water; for this substance often exists uncongealed; the solidity is the result of a molecular energy, which is an essential property in the substance, and which is allowed to come into action by the departure of the caloric out of it. To understand this truth, we must avail ourselves of the old distinction between *active* and *passive powers.* Essential properties are active powers. Accidental properties are the results of passive powers in the bodies which exhibit them; of susceptibilities or powers of recipiency, by means of which the more original powers of the essential properties, either simple or combined, show through and give themselves these new and mutable expressions.

We remark, again, that it is obvious the permanency of the properties which we predicate of a class, or of a general term by which we name it, is essential to the validity of all general and scientific propositions. This, to the logician, needs no arguing. Hence it follows that it is all-important we shall be able to distinguish, in classifying, between permanent or essential properties and "*accidentia.*" How do we effect this? Here the rule quoted from Sir Isaac Newton comes to our aid. If we find that a given property is always present whenever the body is present, and that it is not affected with increment or diminution whatever other effects are wrought on the body, we may safely conclude that it is an essential property. This rule should be qualified by the following admission: It may be that the energy which we invariably see expressing itself through this property, is not the original energy, but is itself the next effect of a latent and undetected energy. If this were surely discovered, we should feel constrained to carry back the name and title of essential property to that original energy. For instance, we have been accustomed to regard caloric as an original energy in matter. Should it be that caloric is itself a result of a peculiar molecular motion in matter, or in some latent *medium*, we must give the name of original energy to that hitherto undetected cause. This, we suppose, Newton would have freely conceded. But this concession does not practically derange our inductive conclusions. For if there is the latent energy, and yet it always expresses itself through the known property, and if it is its necessary law to do so, any practical conclusion from it is as solid as though the latent cause had been seen. We are, in fact, reasoning from it, while we only leave it anonymous. But, it may be asked, does the fact that a body always exhibits a certain property *as often as we have observed it*, prove that property to be essential, and therefore permanent? Is not this the defective induction *per enumerationem simplicem?* We concede that it is nothing more. Hence it is all-important that we employ the other part of Newton's rule also, that upon frequent observations we see the property takes no increment or decrease, whatever changes are made upon the body. If the property stands that test, it is essential. But the application of this test is, as we shall see in the subsequent discussion, but an employment of the canon of "corresponding variations," one of the methods of induction by which a valid is distinguished from an invalid inference. It may be asked, Does the process of inductive reasoning begin so far back in our thinking, in the very formation of our concepts, as well as in deducing from them? We answer, Yes; the rational function must come into play, not only at an early stage of our processes of logical thought, but along with their very beginning. This is the very principle of true metaphysics.

We shall see that this is not the only case of inductive inference, which takes place in the very processes of generalisation. It has been too long and too heedlessly repeated, that the generalisations which give us our general concepts are *preliminary* to our processes of inference, and therefore cannot be inferential. Dugald Stewart, in repeating this statement, seems to have a view of its inaccuracy; for he immediately qualifies it by remarking that, while a given inferential process has no concern with the question whence or how the premises employed came, but only with the question whether they are correctly related; yet one or more of these premises may be itself an inference from a previous illation. This is the vital concession. A general proposition cannot be correctly affirmed, save of general terms. Hence it is also essential that the concepts named in those general terms be correctly framed. The question of their correctness may require to be settled by a logical process. Let it be considered now, that when we frame a general term, it must be understood to connote all the properties essential to the species. For instance, the general term horse must be held to signify each and every property essential to that species of quadrupeds. Let us suppose that, in a place new and strange to us, as the Shetland Isles, we meet with an individual quadruped, which we wish to classify. We see that, along with some quite striking differences, as of size and such like, it has several of the more obvious qualities of the horse species. May we refer it to that species? On the one hand, unless this individual quadruped has all and each of the properties essential to the species horse, we are not authorised to class it there. On the other hand, we have not seen all the possible properties of the Shetland individual: for instance, we have not dissected it; we have not yet satisfied ourselves, ocularly, that it may not be a *ruminant*, or that it may not present specific differences in its osteology. Yet we refer it to the species horse. It is obvious that in doing this, we make an induction, and it is an induction from a part to the whole. We know by observation that the individual has some of the equine properties; we infer that it has the rest of the essential properties. But all logicians agree that the induction from some to all is not necessarily valid. Are our general concepts themselves, then, only partially correct? How much uncertainty must not this throw over all our general reasonings? If we are not certain that a given thing really belongs to its class, we cannot predicate certainly about it what we have proved concerning the class.

Now, on this question, it may be remarked, first, that our references of individual things to their classes are often supported by only probable evidence, or incomplete inductions. And, therefore, our propositions, when applied to those individuals, have only probable truth. But in practical life, probabilities are far from valueless; if they are not universally accurate as guides of our action, they are generally so. But for the construction of a *science*, they do not suffice; for science claims *truth*, and not mere probability. Second, we all practise, in our customary generalisations, certain mental expedients to guard ourselves against erroneous classifications; expedients which we learn by experience, and which are, in fact, approximate uses of logical canons of induction; although we have not distinctly analysed and explained to ourselves the rules which we virtually employ and trust. This is that practical sagacity which the mind acquires in the process of its own self-education. By its help we greatly diminish the probabilities of error in our generalisations. This may be explained by the instance already mentioned: An inexperienced child and a shrewd observing adult, neither of whom is a trained logician or natural historian, see for the first time the Shetland pony. The child, impressed by the puny size, shaggy coat, and bushy fetlocks of the quadruped, may exclaim that it cannot be a horse. The experience of the man tells him that these peculiar appearances may be but *accidentia* of the Shetland variety, striking as they are; and he at once directs his observation to other characters in the little animal, which convince him that it is, nevertheless, a true horse. The more discriminative marks, the uncloven hoof, the character and number of the teeth, the relations of the limbs to each other, furnish him with the inference that the rest of the equine properties would all be found in it if it were thoroughly dissected. Third, this observer, although not a naturalist, makes a practical application of a general principle to guide his induction. His reason has told him that *the ends* of nature cannot but dictate morphologic laws, which insure the associating of certain characters together; so that where some of them are seen, the rest may be safely inferred. He does not call himself a philosopher; he does not name those ends "final causes." But, none the less, his reason has the partial guidance of the universal principle. He does, semi-consciously, a similar thing to that which Cuvier did, when he argued that no quadruped having graminivorous teeth would ever be found with claws on its feet, because the final cause of the Creator would never lead him to provide an animal with the instruments for seizing prey, which was ordained, in other parts of its structure, to live without prey. And when the philosophic naturalist's classifications are made with scientific certainty, by inferring the whole number of essential properties from the knowledge of a part of them, it is because he has converted the invalid induction into a valid one by the help of a necessary principle which he makes his major premise.

POWERS AND PROPERTIES PERMANENT.

But it is time we had returned to another point in our explanation. If essential properties are powers; and if, as such, they must be permanent; why are not their effects continuous? Whereas, it is notorious that properties are not always active in the production of effects. A property, like the attractive energy of a loadstone, may remain for ages without effecting the actual motion towards itself of the bit of iron which lies in an adjacent drawer of the cabinet. This demands explanation at our hands. The explanation is, that properties of created things are causes only potentially: in themselves only powers *in posse*. In order for the effluence of the actual power, a certain relation or relations must be established between the thing possessing the property, and another thing. Thus, the loadstone is always potentially an attractor of iron; but a certain proximity must be established, in order for the effect, motion, to take place. Such instances may be multiplied until we convince ourselves that the essential condition for all physical effects is the instituting of some particular relation between two bodies. Not until the appropriate relation is instituted, is the potentiality of the causal property released, so as to become an actual power. Until then, the property remains quiescent. If this doctrine is correct, the action of an elastic spring, held in a state of compression, is the parallel to the powers of natural things. The elasticity is doubtless in the compressed spring all the time, and expresses itself in a steady pressure upon the bolt or key which holds it. Let that bolt be withdrawn, and the elasticity is released, and produces the visible motion of the body propelled by the spring, hitherto quiescent. The condition of the action of every natural property is, then, its release from some restraining energy; the condition of the cessation of action is the restoration of that restraint. Is not this strictly conformed with the recognised relation in science between Statics and Dynamics, action and reaction?

The instances of the beginning and cessation of effects which we are best able to read, seem to be conformed to this view. The rise of the mercury in the tube of the barometer is ascribed to the counterpoising pressure of the atmosphere. This is a force which really exists perpetually; but it cannot produce this particular effect until a counteracting force is taken away from the top of the column of the mercury. As soon as this is removed, the mercury rises in its tube; when it is replaced, the atmosphere is no longer able to support the column; but the atmosphere has not lost a particle of its weight. Again: chemical affinities are deprived of many of their customary effects when organised bodies are presented to them. This is because there is another energy in the organism, the vital energy. Just so soon as this departs, the carbon, water, and nitrogen of the organism yield to the chemical energies, like other carbon, water, and nitrogen. Those energies are there, but cannot work "until that which letteth is taken out of the way."

This theory may be no more, as yet, than a probable hypothesis. But it substitutes another theory which has recently grown into much favor, and which is also only a plausible hypothesis. That is the theory of "the equivalency and transformation of energy." The conclusion from this doctrine, which is aimed at, is, that there is really but one kind of energy in the material universe; that as the caloric, for instance, which disappears from the sensible to the latent state in the volatilisation of water into steam, is transformed into an equivalent amount of elasticity in that steam, so caloric and elasticity are but two forms of the same energy. Now, much is yet lacking before this supposition is proved. The instances in which a body may be infused with a high degree of one form of energy, and then again deprived of it, while another energy in the same body remains constant, seem fatal to the inference that those energies are equivalent and transformable. Thus, a mass of metal may be greatly heated, and then refrigerated, while its gravity remains unchanged. Gravity, at least, then, cannot be thus correlated to caloric. The same argument seems to hold of all parallel cases.

Another seemingly fatal objection to the theory of the "equivalency and transformation of energy" has been urged by Clausius. What transformation and reflection of a force can take place, which is emitted on the exterior limit of the universe, and on a line of action away from existing bodies? Let the energy be, for instance, that of heat or light. Its reflection back into the universe in the form of the same, or of a transformed energy would appear equally impossible, since nothing exists, outside the universe, to be the medium of its reception or reflection. Hence, it would seem that, as a wedge of heated iron placed in a winter atmosphere must continuously lose its caloric until as cold as the surrounding medium, so a universe, a system of bodies energised under natural laws, must continually diffuse its energies until its motions declined into universal quiescence. The favorite corollary of the theory under debate is: the permanency and equality of the aggregates of cosmic forces through all time. But this corollary, we here see, cannot be true on that hypothesis. Yet, if it be not true, how shall the physicist maintain his fundamental position, the uniformity of nature? The alternative hypothesis we suggest solves the difficulty. The powers of nature are not all equivalent and transformable the one into the other. But the powers of nature are permanent; because true powers are essential properties, and essential properties are permanent. The *forms* of matter change; but the matter, whose are the essential properties, is indestructible.

But the only *a priori* argument advanced for the new theory, so far as we are informed, is this: That reason forbids us to suppose that a power which we see now existing and active, can anon, upon the completion of its effect, be annihilated and pass into nonentity. It has disappeared in that form; but they argue, it cannot be extinct. Hence, they conclude that it has reappeared in the form of its effect. There has been, not an annihilation, but a transformation of the energy. Now, this argument seems wholly neutralised by the view which we have suggested.

Grant that reason requires our believing in the permanency of powers, as much as of substances; this energy which we see acting temporarily, has not gone into its effect, but has retired into potentiality in the matter which it inhabits. The conditions of its release have terminated; it is again remanded from its active to its potential state. The same energy is in matter still, in the form of essential, permanent property;

and is again able to emit the same power and propagate a similar effect, whenever the conditions of release take place again. This theory of power, then, instead of reducing all the energies of nature to a single one, recognises as many distinct kinds of energy in material things, as there are certainly distinct and essential properties in matter. We may not have concluded accurately as to which properties are really distinct and essential. We may be mistaking two properties for essential ones, which will turn out to be two effects of some more latent essential property of matter. We may find that what we call heat, light, and electricity are but three phases of some one molecular energy, transformable into these equivalent effects. But we return to the more natural and obvious theory of Newton and his great contemporaries, that matter has more than one real, essential property, and more than one power. This theory of power is encumbered with none of the difficulties besetting the newer one. It coheres with the rational view which, as we have seen, compels us to regard essential properties of substances as nothing else than powers *in posse*, because we have cognition of them only as we see them producing effects.

THE AIM OF REAL INDUCTION.

But the main use of the inductive logic is to enable us to anticipate nature. Our beneficial power over her can only be gained by learning her ways. To be able to produce the given effect we desire, we must know the natural law under which that effect arises. Bacon has tersely expressed this truth at the beginning of his *Nov. Org.* "Human knowledge and power coincide, because ignorance of the cause maketh the effect to fail. For Nature is only conquered by obeying her; and that which in our contemplation hath the aspect of Cause, in our working hath the aspect of Rule." The thing we need to do is to predict what sequent will certainly follow such or such an antecedent. For only thus can we know these two things, the knowing of which constitutes all practical wisdom: how to produce the effect we desire, and how to foresee what shall befal us. Our first impulse is to attempt to learn nature's secret, by the mere observation and summing up of what we see occurring, with the circumstances of the occurrences. But when we have done this, and recorded our enumerations, experience speedily teaches us that we cannot yet certainly interpret and predict nature; since the same antecedents may not be relied on always to bring in the same sequents. Sometimes they may, and oftentimes they may not. The problem, then, is to distinguish between those observed sequences which certainly will hold in the future, and those which will not. And *between the antecedent and consequent of the former sort, there must be known to be a necessary tie: for it is self-evident that only a necessary tie can ensure the certain recurrence* of the second after the first. But it is equally evident, both to the human reason and experience, that nature has no necessary tie between her events, except that of *efficient cause.* Hence it appears that the sole remaining *problem of Induction is to distinguish the causal sequences we observe, from the accidental.* Whenever we see what we term an effect, a change, a newly beginning action or state, this necessary law of the reason assures us that it had its cause. Had not that cause been efficient of that effect, it would not have been true cause. It must, then, have communicated power. That power will always be efficient of the same effect, when it acts under the same conditions. Hence, when we have truly discriminated the cause from the mere antecedent, the *propter hoc* from the *post hoc,* we have found therein a certain and invariable law of nature. We have read nature's secret. We are now enabled to predict her future actions; and so far as we can procure the presence of the discovered cause and conditions, we can command nature, and produce the effects we desire. This, and this alone, is *inductive demonstration.* This position is substantiated also by the authority of the three most intelligent expounders of the inductive logic, whom we have quoted: by that of Lord Bacon, cited on p. 6; by that of Sir Isaac Newton, cited in his second Rule, on p. 8; and by that of Mr. Mill, p. 19. (See SOUTHERN PRESBYTERIAN REVIEW for January.)

He who ponders the last argument thoroughly, will see that there is no consistent explanation of the inductive demonstration possible, upon the plan of Mr. Hume's metaphysics. Let the *a* *priori* rational notion of efficient cause and power be discarded; let our judgment of cause be reduced to the mere observation of invariable sequence, without any supersensuous tie between antecedent and consequent supplied by the law of reason; let the vain distinction between efficient cause and physical cause be established, and the aim of science restricted to the inquiry for the physical cause, while the search after the efficient cause is discarded; and let the rational distinction between true cause and *conditio sine qua non* be obliterated; then, obviously, no necessary truth remains, from which any argumentative process can be constructed, to lift any series of observations above the uncertain level of an *inductio enumerationis simplicis.* Mr. Mill himself, while making the fatal denials enumerated above, is driven by the force of truth to say that such necessary, universal truth must be introduced from some whither, in order to give to *induction* the solid character of science. Whence can it be obtained, if not from the intuitive judgment of efficient cause? Experience, without this, only tells us that this has come after that a great many times. But the number of instances in which experience has not been, and will not be, able to observe whether the same consequent comes after that antecedent, is infinitely greater than the number of instances which have been experimentally observed. Hence we can never conclude by that method, whether the sequence we observe is the certain one in the future. The introductory citations showed the reader how the writers on this branch of logic waver and confuse and contradict each other. Is not the reason now disclosed? That so many of them have disdained the guidance of correct metaphysics.

The reader is now brought to the proper point of view to understand why the induction from a mere enumeration of agreeing instances can never rise above probability; and why it does, as we admit, raise a probable expectation of recurrence in the future. *So far as the observed presence of a given antecedent seemingly next before the consequent raises the probability that we see in that antecedent the true efficient cause*, just so far have we probable evidence that the consequent will follow it in future. Now, inasmuch as our rational intuition tells us that cause always immediately precedes effect, the *phenomenon* which is seemingly next before another may be in many cases taken for the nearest antecedent, and, therefore, the cause. But even this rule of probability is liable to many exceptions, which we are taught to make by our practical sagacity. We have invariably seen darkness preceding dawn; and that immediately. But we have never felt the least inclined to see the faintest probability therein, that the darkness was the cause of the dawn. Why not? Because our observation showed us a species of heterogeneity between the two events, which made us disinclined to look for the probable, or even the possible, cause of light in darkness. But in many other cases, as, when the tides were seen always to follow the rise of the moon to the meridian, the probability that the moon's coming was the true cause appeared; and as soon as Newton's theory of mutual attraction was stated, that probability appeared very strong.

But ordinarily the observed sequences can only raise a probability that we have found in the antecedent the true cause; for this reason: *that we know there are often such things as unobserved or latent or invisible causes.* For instance, the old empirical chemists knew that something turned the metal, when sufficiently heated, into the calx. They talked of an imponderable agent which they named *phlogiston.* They had not suspected that oxygen gas was the cause; for this gas is transparent, invisible, and its presence in the atmosphere had not been clearly ascertained. Had the frequently observed sequence, then, led them to the conclusion that *heat* was the efficient and sufficient cause of calcination, they would have concluded wrong. Farther experiment has taught us this error: some metals, as potassium, calcine rapidly in the midst of intense cold, if atmosphere and water be present. None of the metals calcine under heat, if atmosphere and water are both excluded, as well as all other oxygen-yielding compounds. Here, then, is the weakness of the induction by the mere enumeration of agreeing instances: *We have not yet found out but that an unobserved cause comes between the seeming antecedent and the effect,* the law of whose rise we wish to ascertain.

And here is the practical object of all the canons of inductive logic, and of all the observations and experiments by which we make application of them, to settle that question, *whether between this seeming antecedent and that effect, another hitherto undetected antecedent does not intervene?* Just so soon as we are sure there is no other, whether it be by many observations or few, we know that the observed antecedent is the true efficient cause; and that we have a law of nature which will hold true always, unless new conditions arise overpowering the causation. Not only is it possible that we may be assured of the absence of any undetected cause between the parts of the observed sequence by a few observations; we may sometimes reach the certainty, and thus the permanent natural law, by a single one. To do so, what we need is, to be in circumstances which authorise us to know certainly that no other antecedent than the observed one can have intruded unobserved. Such authority may sometimes be given by the testimony of consciousness. For instance, a party of explorers are travelling through a Brazilian forest, where every tree and fruit is new and strange to them. One of the travellers sees a fruit of brilliant color, fragrant odor, and pleasing flavor, which he plucks and eats. Soon after, his lips and mouth are inflamed and swollen in a most painful manner. The effect and the anguish are peculiar. His companions, who have eaten the same food, except this fruit, and breathed the same air, do not suffer. This traveller is certain, after one trial, that the fruit is poisonous, and unhesitatingly warns his companions with the prophecy: "If you eat this fruit, you will be poisoned." What constitutes his demonstration? His consciousness tells him that he has taken into his lips absolutely nothing, since the previous evening, that could cause the poisoning, except this unknown fruit. He remembers perfectly. He has tasted nothing except the coffee, the biscuits, and the dried beef which had been their daily and wholesome fare. But, *no effect—no cause.* This fruit, the sole antecedent of the painful effect, *must therefore be the true cause;* and must affect other human lips, other things being the same, in the same way. His utter ignorance of the fruit does not in the least shake his conclusion. The traveller has really made a valid application of the "method of residues." He has argued validly from a *post hoc* up to a *propter hoc.*

This is so important that it will not be amiss to illustrate it in another instance of inductive argument—that of the metals and calxes. The first observations seemed to show that heat was the cause of calcination. But when heat was applied to a metal excluded from atmosphere, it did not calcine. And when the metallic bases of the stronger alkalies, as potassium, were identified as metals, it was observed that this one of them calcined violently on a lump of ice. Hence the belief that heat was the efficient of calcination had to be given up—chemists had to confess that the apparent antecedent, heat, in their first experiments could not be the nearest antecedent, but that this, the true cause, was still latent. They had really corrected their erroneous induction by the joint method of "agreement and difference." It was reserved for Sir Humphrey Davy to show them the true efficient of calcination, in the invisible, undiscovered, but all important agent, oxygen-gas.

Once more; when the observed antecedent is of a character which our previous conclusions have not condemned as heterogeneous from the supposed effect, and therefore not very unlikely to be its cause; as we increase the number of the agreeing instances observed, we feel that our probable evidence that we have found the true cause, grows also. Why is this? It is because reason has assured us that this effect has its efficient cause next before it; and as this antecedent seems to appear again and again before it, and no other has yet been detected between them, it becomes more probable that there is no other intervening antecedent. If such is the case, then this antecedent is the cause.

THE METHODS OF INDUCTION.

We are now prepared to advance to the correct definition of the inductive demonstration. It may be, in form, an enthymeme, but always, in reality, is a syllogism, whose major premise is the universal necessary judgment of cause, or some proposition implied therein. This view of the inductive proceeding corresponds with that conclusion to which the reflection of twenty centuries has constantly brought back the philosophic mind: that all illative processes of thought are really syllogistic, and may be most completely stated in that form; and that, in fact, there is no other process of thought that is demonstrative. The history of philosophy has shown frequent instances of recalcitration against this result, as those of Locke, of Dr. Thomas Brown, and of their followers; but their attempts to discard syllogism, and to give some other description of the argumentative process of the understanding, have always proved futile. The old analysis of Aristotle still asserts its substantial sway; and successive logicians are constrained, perhaps reluctantly, the more maturely they examine, to return to his conclusion—that the syllogism gives the norm of all reasonings. If our definition of the inductive demonstration, then, can be substantiated, it will give to logic this inestimable advantage: of reconciling and simplifying its departments. The review of opinions given by us at the outset revealed this state of facts: that logicians felt, on the one hand, that no reasoning process could be conclusive, unless it could be shown to conform, somehow, to syllogism; and on the other, that the custom and fashion of distinguishing induction from deduction as different, or even opposite, kinds of argument, had become prevalent, if not irresistible. Consequently, the most of them, following the obscure hints of their leader, Aristotle, endeavored to account for induction as a different species of syllogism, in which we conclude from the some to the all, instead of concluding from the universal to the particular or the individual. And then immediately they were compelled, by the earliest and simplest maxims of their logic, to admit that such syllogisms are inconclusive! And they have to confess this in the face of this fact: that this induction is the *organon* of nearly all the sciences of physics and natural history; sciences whose results are so splendid, and so important to human progress! Such a result is not a little mortifying and discreditable to philosophy. But we hope to show that it is a needless result. It will appear that induction is not only syllogistic, and therefore within the pale of demonstrative argumentation, but regularly and lawfully syllogistic. Mill has had a sufficiently clear conviction of the necessity of accomplishing this, to teach (Vol. I., pp. 362–365) that the conclusions of this species of reasoning can only become solid when grounded in a universal truth.

This, he thinks, is our belief in the invariability of the law of causation. But he then (p. 345) very inconsistently adds, that this universal truth itself is but a wider induction, which approaches universal certainty sufficiently near, by reason of its breadth. This universal and necessary truth, we hope to show, is the intuition of cause for every effect, along with the truths involved therein.

To effect this, the methods of induction must be explained. When we speak of observed sequences, we mean a set of observed resembling cases where one state or change seems immediately to precede another change, or "effect," which we are studying. These cases may be observed by ourselves, or witnessed to us by others. The *fact* of the sequence is the only material thing. But, first, one's own observation must be honest and clear, and his record of the case exact. He must not see his hypothesis in the facts, but only what occurs there. And, second, a case taken on testimony should be fully ascertained by a judicial examination of the evidence. Having now this set of agreeing instances, more or less numerous, which gives us, as it stands, only an induction *per enumerationem simplicem*, our task is, so to reason from it as to discriminate the *propter hoc* from the *post hoc*. The result of this task, when successfully performed, is to give us a "law of nature," which is such because it is a law of true efficient causation. It is to effect this, we need the methods of logical induction. In stating them, the chief guide will be Mr. Mill, whose discussion in this point seems the most complete and just.

1. The "Method of Argument" is the following. Observation usually gives us sequences of this kind, viz., Not one antecedent, but a cluster of them appear to stand next before an effect or (more commonly) a cluster of effects. Such observation, no matter how often the like case recurs, fails to tell us which antecedent, or which combination of them, contains the efficient cause of either effect. We must observe farther, and compare cases. Like the algebraist, we will use letters as symbols, for the sake of clearness, calling the antecedents by the first letters of the alphabet, and the consequents by the latter. Let us suppose that the cases agree in this: one antecedent remains the same in each,

and the same effect appears after each cluster of antecedents, however the other antecedents may change. Thus, in case 1st, A+B+C are followed by X. In case 2d, A+D+E are followed by X. In case 3d, A+F+G are followed by X. Let it be postulated that these are all the antecedents: then the true cause of X must be among them. But in case 1st, neither D, nor E, nor F, nor G, could have caused X, for they were absent. In cases 2d and 3d, neither B nor C could have caused X, for they were absent. Therefore A was the true cause of X each time. The canon, or rule of elimination, or exclusion of seeming but false causes, then, is this: Whichever antecedent remains alone unchanged next before the same effect in all the known cases of sequence, is the true cause. The law of nature gotten in this case is, that A will always, *cæteris paribus*, produce X. The necessary universal truths on which we have proceeded are, that every effect must have some cause, and that, to be efficient cause, it must be present.

The converse process is also practicable. Let the cases observed be in the *a posteriori* order: several clusters of effects X+Y+Z, X+W+V, etc., are found to agree only in that among the antecedents A is constant. The counterpart canon will teach that X is the effect of A.

As an example of this method may be taken the earlier and simpler reasoning by which the tides were connected with the presence of the moon on the meridian. In one case the flood tide was observed, we will suppose, at the bottom of a bay penetrating the land towards the west. The observed antecedents were the passage of the moon over the meridian, and also a strong east wind. It did not appear whether the moon's attraction or the wind's force was the main cause. At the second observation, the flood-tide was preceded by the moon's coming to the meridian, and by a calm; at the third, by the moon and a south wind. The argument concludes that the moon is, all the time, the main cause.

But, simple as this process of exclusion seems, it is not yet a perfect demonstration in every case. This arises from three truths, which must be candidly admitted. First. Usually, we cannot know that the observed antecedents, A+B+C, are all the antecedents really present; because often true causes remain long latent. Second. The same effect, X, may be caused at different times by different true causes. For instance, fulminate of mercury explodes under heat; it also explodes under percussion. Sensible caloric is emitted by the solar rays; by compression of a gas; by friction; by chemical actions. If, then, we were safe from the presence of a latent cause among the antecedents, all that we should prove by the method of agreement would be: A is one cause of X (while there may be others). But this would be no mean result; for it would give us thus much of power over nature, that we should know (whether or not X could be produced by other means) we could always produce it when we could, *cæteris paribus*, produce A. Third. One effect may be the result of the combination of two or more causes. And this single effect may be the total of what would have been the two separate effects of the two causes, acting severally; as when two mechanical forces moving in different lines, propel a mass along the diagonal of the "parallelogram of forces." Or, the mixed effect may present itself in a new form, concealing, by its apparent heterogeneity, both the causations; as when the affinities of an acid and an alkali form a neutral salt, which exhibits neither acid nor alkaline reaction. In view of this third truth, it is evident the "method of agreement" may not tell us absolutely whether A is the cause of X, or A with which other antecedent combined. Again, since A may itself be, along with X, one of a pair of effects of a latent cause, all we can conclude is, either A is cause of X, or is an invariable function of an unknown cause of X. The method of agreement, then, does not give us an absolute demonstration, unless we have means of knowing that the observed antecedents, A+B+C, A+D+E, etc., are the only antecedents present in each sequence—that no casual antecedent is left undetected.

2. The "Method of Difference" is applicable to the following case. A set of sequences is ascertained, in which, when a given antecedent is present, a given consequent is also present; but when that antecedent is absent, that consequent is also absent. Thus, A+B+C are followed by X+Y+Z. But B+C are only followed by Y+Z. Here the reasoning proceeds on this pre-

mise: because this antecedent A cannot be excluded without excluding the effect X, it must be the efficient cause of X. The canon derived may be thus stated: Whenever the absence of a given antecedent is followed by the absence of the effect, all the other circumstances remaining the same, that is the true cause. The law may consequently be inferred, that A will always produce X, *cæteris paribus*. For instance, let the problem be to ascertain the true cause of the corrosion or calcination of a metal, as iron. It is found that sometimes heat and atmosphere are present; at other times heat without atmosphere. In the former cases corrosion always followed; but when the atmosphere was excluded, there was no corrosion. The cause of corrosion must, then, be in the air; farther experiment confirms this, by showing it is in the oxygen of the air.

So far, then, as we can *know* that the second set of sequences, in which the effect failed, *differed* from the former set in which it had place, *only in one circumstance*, we know that the true cause is in that circumstance. This is the canon on which most of our experimental inductions in practical life proceed. It is the one of which *experiment* usually seeks to make use. For it is this feature which experiment is most often able to realise; the reproduction, namely, of the identical sequence, abating one single known circumstance, which has been observed before. Hence the method of difference is both more feasible and more definite in its conclusions than the method of agreement. Indeed, the chief value of the latter is to suggest a probability which points to the hypothesis indicating the experiment which will test it. By the experiment thus suggested, an appeal is made to the method of difference, and the probability of the law of cause is either established or exploded.

But the method of difference, when most rigidly applied, only proves that A is one cause of X. It does not prove that X may not be also produced, in other times and places, by other causes. It may, however, be again remarked, that this gives us so much, at least: that A, given similar conditions, will always produce X. Reflection will show, also, that this method may be used in the counterpart, or *a posteriori* way. Whatever antecedent is always absent when the effect X fails, all other circumstances remaining the same, is a cause of X. But, because this canon proves that A always produces X, it does not follow by the converse that every X was produced by A. To the heedless mind, the two propositions may seem almost identical; but they are really different, and the second may be false. Its falsehood appears from the admission that similar effects are often produced at other times by wholly distinct and independent causes. Observation may have proved that all solar rays directly produce calefaction; but it is entirely erroneous to say all calefaction is from solar rays directly. Few cautions are more important than this, which reminds the inductive reasoner, that while like causes give like effects, like effects do not prove like causes.

In this reasoning, we, of course, use the word cause in the sense of concrete causal antecedent. If it is taken in the more abstract sense of the efficient energy present in the concrete causal antecedent, it may be a probable hypothesis, that the energy is the same in these several concrete causes. Thus, let the effect be calefaction. It may be caused by the sun's rays, or by combustion, or by some other form of chemical action, or by friction, or by percussion, or by a modified current of galvanism. This proves beyond a doubt that the same effect does not always come from the same (concrete) cause. But the physicist may claim that the molecular energy, causing the sensible effect of calefaction, may be the same energy in all these different antecedents. If so, there is an abstract sense in which the effect, calefaction, proceeds from the same cause all the time. To affirm or deny this is equally unnecessary to our purpose.

3. The third method may be regarded, from one point of view, as a double application of the first, or as a combination of the first and second. The method of difference, as we saw, is the one to which our intentional experiments usually appeal. Having observed a number of cases in which a cluster of antecedents, A + B + C, is followed by several consequents, X, Y, Z, and having surmised that A causes X, we construct a designed sequence, in which the cluster of antecedents is in all respects the same, except the exclusion of A. If X disappears out of the consequents,

we reason that A is a true cause of X. But in the study of nature, instances may well arise in which we cannot control the antecedents A + B + C, so as to procure the rise of B + C without A. What can we do? The third method answers: observe and record all the instances in nature where B + C occur without A, and probably with some other phenomenon, as B + C + D, or B + D + E, etc. If we find that all these clusters of antecedents, however else they may differ, agree in the omission of A and also in the failure of X, the probability is increased that A is an efficient cause of X. We have made two different applications of the method of agreement, one affirmative and the other privative, and they concur in pointing to A as a real cause of X. As an example: the question was, Which is the real efficient of the anodyne effect in crude opium? This is known to be a complex gum. It is also known to contain, as one of its "proximate principles," the alkaloid known as morphia. Every time the crude gum is given, including the morphia, an anodyne effect follows. This is no demonstration. Let us now suppose that organic chemistry has not yet given us the ability to extract the morphia alone from the crude gum; with an exact certainty that we took out nothing else and left the opium, in all other respects, what it was before. This inability prevents our resorting at once to the definite method of difference. But we may collect all known gums any ways akin to opium, containing other proximate principles which it contains, and administer them. If we find that among the various effects of the various drugs, the anodyne effect fails in all which lack morphia, we adopt the probable opinion that this is the real anodyne agent. But the wise physician will remember that this is short of demonstration. The uncertainty always attaching to the method of difference may be diminished, but cannot be annihilated by doubling the testimony. Thus, in the instance taken, the first set of cases would still leave some doubt whether some undiscovered element in the crude opium, or some combination thereof with known elements, might not be the efficient; and in the second set of cases, where morphia was absent, and the anodyne effect also failed, it would not be demonstrated but that the new drugs given contained some element counteracting an anodyne effect, which, but for this, might still have been emitted in the absence of morphia.

4. The fourth method has been termed that of residues. Cases which present a plurality of antecedents, followed by a plurality of consequents, are analysed by it until one pair is left unaccounted for. This may then be concluded to be cause and effect. The result observed is, that A+B+C are frequently followed by X+Y+Z. Now, if, in any valid way, it has been proved that A is the cause of X, and, if single, produces only X, and that B produces only Y, then, although we may not experimentally insulate Z in any separate case, it may be concluded that C is the true cause of Z. For, the causal efficiency of A having been traced into X and of B into Y, there is no source to which to ascribe Z, except to C. Every effect must have a present cause. Obviously, to render this method a complete demonstration, we should be able to know that A, B, and C are the only possible causes present. For if a fourth antecedent, D, remains in addition to C, it may be proved that A has expended its efficiency in producing X, and B in producing Y; and it will still be an unsettled problem, whether C or D, or a combination of the two, produces Z. The elimination is incomplete.

5. Another method remains, which may be applicable where, in consequence of the inability to experiment, the exact application of previous methods may be impracticable. This may be called the inference from *corresponding variations*. A given state or change, which we call A, is often seen to be followed by a change called X. This suggests, as has been so often said, only a probability that A is the efficient cause of X. But if a variation in the action of A is seen to be followed by a corresponding variation in the occurrence of X, the probability strengthens. If a second and a third variation in A is followed by still other corresponding changes in X, the evidence grows rapidly towards certainty. This variation in the antecedent may be not only in quantity, but also in direction of its action, or in some other circumstance; and still it gives us this inference. The nature of the proof is this: if a given antecedent had no power over a consequent, a modification of that antecedent would have no influence on that consequent. Hence, when the modification of the one is

invariably accompanied with a corresponding modification of the other, it seems plain that there must be some causal tie. But it is not, therefore, certain that the tie is direct; the two circumstances which change together may be connected as two functions of some more recondite cause. Until we are able by some experiment or reasoning to exclude this hypothesis, our induction by observing corresponding variations is not complete.

Examples of this method may be found in the conclusion that increments of heat are the causes of the successive expansions of the mercury in the thermometer. We observe that, the more heat, the more expansion; the less heat, the less expansion. Another application of this induction led to the discovery of the causes of the variations in the height of the tides. It was observed that when the conjunction or opposition of the sun and moon was most complete, the spring-tides occurred; when they were less complete, the tides were lower; and when the two luminaries were farthest from a conjunction or opposition, a whole quadrant apart in the ecliptic, the least, or neap-tides, occurred. Hence, we concluded that the concurrence of the traction of the moon's force with the sun's, in the same line, is the cause of the higher tide.

If the corresponding variations in the antecedent and consequent are variations in quantity, and especially if they maintain an exact proportion in their increase or decrease, such as can be measured by numerical *ratios*, the induction is very clear. The doubling of A results in the doubling of X, the effect; the quadrupling of A in the quadrupling of X, for instance. Then A is clearly the cause of X, or, at least, a regular function of a cause of which X is an analogous function. And the latter conclusion enables us to predict the future result as certainly as the former. But the variations may be in other circumstances than quantity. For instance, if a given body is surmised to be the cause of motion in another body, and if the direction of the produced motion changes regularly in correspondence with the changed direction of the first body, we conclude that our surmise is correct. Or else, again, both motions are functions of some force not yet detected, to which they are both related by a causal tie; so that the regularity of the observed law of motion is safely assumed.

These five methods of interpreting nature, with their canons, appear to present all the valid means in the possession of science. No other are suggested. But the following reasoning seems to show that there can be no other. If the antecedent, which seems to be next the effect, could be surely known in every case to be really the nearest antecedent, no canons of induction would need to be applied. The simple observation would directly show us the causal tie, and, therefore, the natural law. (It is only necessary to say, that by *nearest* antecedent is not meant the one nearest in time or space; for in this sense an inefficient may be as close to the effect as an efficient antecedent; but we mean the nearest in the sense of efficiency.) The whole problem, then, is to make sure that, between the effect and the nearest visible antecedent, some invisible or unnoted antecedent has not come. Now, the only ways to test this, in man's power, are by some elimination of parts of the sequences, or some variation of parts. The methods of agreement, difference, and residues, if applied in their direct and converse modes, exhaust all the eliminations practicable, whether of causal or non-causal antecedents, or of essential or non-essential sequents. The method of corresponding variations completes the use of the remaining resource. These methods are but the effectuating of that task which the sagacity of Lord Bacon pointed out: the separation of the irrelevant instances from our observed sequences, so that the truly causal ones may be disclosed. That which he foreshadowed, the slow and painstaking care of other philosophers has carried out to its details, and presented with more exactitude. It may be rash to assert that no other method for separating the *post hoc* from the *propter hoc* will be added by the future advancements of logic. Thus far this critical science has advanced in the ablest hands of our day.

Dr. Whewell impugns, indeed, these methods as artificial and fruitless. He questions whether it is by them truth is really discovered, and challenges Mr. Mill to name the important physical laws which the discoverers have professed to reach by either of these methods. The answer to this view is, first, to deny Whewell's allegation. All the valid inductions of common experience and of inductive science have been virtually made by these "methods." And, as we remarked, experiment, the great lever of induction in the physicist's hands, is both a virtual and a formal appeal to the "method of difference." The second answer is, that a logical science, in one sense, has not for its end the discovery of truth in the sense of the invention of it, but the proper function of logic is *to test* the processes of invention after they are suggested. Logic is the critical science. The syllogism, in its other or deductive aspect, is not the inventive *organon*. Its office is to sit as judge on the processes of deductive thought which claim to lead to truth. The function of the syllogism is to hold up its form as a standard of those relations of propositions which make illations valid, that the professed reasonings presented by the inventive faculty, suggestion, may be tried by that sure rule. So, the rules of the inductive syllogism are not claimed to be valuable because they are suggestive of unseen truths, but because they try and discriminate, in the suggestions supposed or claimed to be inductive, between the valid and the invalid. The processes which are active in leading to the unknown truth are observation, hypothesis, and the "scientific imagination," with experiment. Again, it is but seldom that the vigorous minds which have reasoned deductively to valuable truths, have expressed their arguments in formal syllogisms. Even geometers do not do this, with all the exactness of their noble science. The reasoner does not usually proceed farther than using *enthymemes* or *sorites* in the formal statement of his arguments; often he is not even so formal as this. But none the less is the syllogism the full form of each valid step; and the test of its validity is, in the last resort, whether the step can be stated in a syllogism of lawful mode and figure. So it may be true that a Galileo, a Newton, a Franklin, a Maury, may not have expressed his inductive argument in the technical form of either of the five methods. But if his induction is demonstrative, *he has virtually*, if informally, *employed them.* The test of its validity is, in the last resort, whether his inductive process can be expanded into one of them, and find in it its full and exact expression.

But it has been admitted that even these methods of induction do not always lead to absolutely demonstrated results. The insufficiency of the method of agreement was clearly evinced: either one of three contingencies (see p. 505) would vitiate the conclusion. Even the method of difference, the most exact of all, we found (see p. 507) only gave an absolutely certain result, on condition we could know positively that, between the two sequences, A+B+C, followed by X+Y+Z, and B+C followed by Y+Z, we had made no difference among the antecedents except the exclusion of A. But, obviously, that is a thing very hard for us, in most cases, to know positively, and in many cases impossible to know. Yet, if it is not known, our inference that A is the efficient of X, is not absolutely sure, because the possibility remains that the failure of X to appear among the second set of effects may be due, not solely to the absence of A from among the antecedents, but to that other unnoticed change which was made among them when removing A. Hence, another work remains before an inductive demonstration is complete. This is *Verification.*

Now, obviously, one approximate method of verification is to apply a second method and canon of induction, or a third, in addition to a first. If they give the same result, the probable evidence mounts up towards certainty with a multiplying ratio. But in many cases only one method is applicable. The most complete verification is obtained by experimenting backwards. Having reasoned to the conclusion that X is the effect of A, the student of nature constructs an experiment, in which A is made to arise alone. If X follows, and the conditions of the case are such he can know that no other antecedent capable of producing X has been present, his induction is verified. Of this the method of Franklin is an instance, when he completed the inductive argument that the lightning of the clouds is electricity. His experiments on electrical bodies, and his observation of the lightnings, had suggested the belief that the causal energy was the same. This was, so far, only an induction by comparison and simple enumeration of instances. The lightnings were apparently followed by some of the consequences of the electric energy. Now, if the two are in reality the same energy, the lightning should experimentally produce all the known effects of the electric excitement. To verify this, as is known, Franklin availed himself of the ingenious expedient of the kite. He thus found that a conductor, excited no otherwise than from the energy of the lightning cloud, emitted the spark, communicated the muscular shock, charged the Leyden jar, and did all that the electrical machine had done. Thus, an only probable induction was verified and raised to the rank of a certainty.

Verification is not confined to experiment; but sometimes a sagacious observation of nature will detect her giving the confirmation. Of this the most splendid instance is the confirmation of Sir Isaac Newton's hypothesis of the orbitual movements of the planets by the force of gravity. He had these data of probability. The law of *inertia* seemed to give a cause for a tangential motion absolutely constant. But Copernicus and Galileo had taught that the planetary motions were orbitual around the sun as a centre. There was the great mechanical law of the parallelogram of forces, which teaches as that the mass acted on by two *momenta* in two lines, will move in the diagonal. Add to the inherent tangential *momentum*, then, a centripetal force, and the orbitual motion seems accounted for. Of this orbitual compound motion, the centripetal element appeared as real a *falling to the centre* as that of the stone (or the famous apple) falling to the earth. But now our terrestrial experiences had taught him most familiarly how this falling to the earth is the effect of gravity. The lines pursued by all falling bodies tend to the earth's centre. Obviously the earth draws them to her centre. Now, this attraction of gravity acts not only at the earth's surface, but above its surface to the highest distances attained by mountains and balloons. It obviously acts on the clouds and their contents. *Why suppose it limited at all?* Make the supposition that it is universal, though diminishing in intensity with distance, and why may not this be the very reason of all these centripetal motions? Can one guess by what ratio the force of gravity will diminish with distance? If it expands itself in every direction around its centre, it would appear that its intensity in each point should diminish by the same ratio by which the surface of a sphere increases; that is, with the square of the *radius*. May it not be, then, that while the tangential motion of each planet is but the original impulse in a straight line, preserved absolutely constant by *inertia*, the centripetal or falling motion compounded therewith, is just the effect of this gravitation, acting with an energy inversely as the squares of the distances?

Such was the dazzling hypothesis. (We profess to state it, of course, not in the very words of Newton, but in the tenor of his expositors.) But he was too good a logician to assume it as proved; he had a probable induction thus far, nothing more. Verification was needful. He first established the law of planetary attraction, using Kepler's facts (or so-called laws) as his minor premises. Knowing thus the attraction between the moon and the earth, he supposed a piece of the moon brought to the surface of the earth, and from the established law of its attraction, computed the quantity and direction of the descent this piece would make in one second when it came to the tops of the highest mountains. He found that this was identical with the descent, both in direction aud amount, of a piece of the mountain, as acted on by *gravity*. From the identity of behavior he inferred (by Rule II. of his *Regulae Philosophandi*) that the force which makes the planetary attraction is identical with the force of gravity. Thus the grandest hypothesis ever constructed by a scientific man, was converted by this verification (afterwards extended to the other planets) into an established truth.

Thus it is successful verification which completes the inductive demonstration. Where no verification is possible, many, or even most, of our inductions may remain but probabilities. But they are not therefore wholly useless; for, first, they may guide the investigator in the invention of tentative hypotheses; and, second, as we have seen, they may lend to practical life a guidance which, though not certain, has its value. But such an induction has no right to be set up as a proposition in science.

INDUCTION IS SYLLOGISM.

It is now time that we returned and redeemed our promise to show that induction is but the old syllogistic logic, inasmuch as each demonstrative process is but an enthymeme, whose real major premise is the intuitive judgment of cause, or some corollary thereof. We are glad to have the powerful and very emphatic testimony of Mr. Mill to this doctrine. In Book III., Chap. 21, he says: "As we recognised in the commencement, and have been enabled to see more clearly in the progress of the investigation, the basis of all these logical operations is the law of causation. The validity of all the inductive methods depends on the assumption that every event, or the beginning of every phenomenon, must have some cause—some antecedent on the existence of which it is invariably and unconditionally consequent. In the method of agreement, this is obvious, that method avowedly proceeding on the supposition that we have found the true cause as soon as we have negatived every other. The assertion is equally true of the method of difference. That method authorises us to infer a general law from two instances: one in which A exists together with a multitude of other circumstances, and B follows: another, in which A being removed and all other circumstances remaining the same, B is prevented. What, however, does this prove? It proves that B, in the particular instance, cannot have had any other cause than A; but to conclude from this that A was the cause, or that A will, on other occasions, be followed by B, is only allowable on the assumption that B must have some cause; that among its antecedents in any single instance in which it occurs, there must be one which has the capacity of producing it at other times. This being admitted, it is seen that, in the case in question, that antecedent can be no other than A; but that, if it be no other than A, it must be A, is not proved, by these instances at least, but taken for granted. There is no need to spend time in proving that the same thing is true in the other inductive methods. The universality of the law of causation is assumed in them all."

Let us submit this assertion to a more critical examination; and first as to the method of agreement. Refer to p. 504. In the first case, or cluster of cases, we saw A+B+C followed (possibly among other effects) by X. In the second, A+D+E; and in the third, A+F+G, are also followed by X. The reasoning, rigidly stated, now proceeds thus (and that it may proceed strictly, it is necessary to make the supposition that no other causal antecedents are present except A, B, C, in the first case, etc., which, in practice, it will usually be very difficult to know): In the first case, the cause of X must have been either A or B or C, or some combination of them. Why? Because it is a universal *a priori* truth, that there is no effect without a cause. This step thrown into a formal syllogism will be:

1. No effect can arise without a cause.

2. But X arose preceded only by A+B+C,

Therefore A or B or C, or some combination of them, must be cause of X.

So, we prove that, in the second case, A+D+E, and in the third, A+F+G, must have caused X. But next we construct another syllogism:

1. A cause must be *present at the rise* of the effect (immediate corollary from the intuition of power and efficiency in cause).

2. B and C were absent in the 2d and 3d cases; D and E were absent in the 1st and 3d cases; F and G were absent in the 2d and 3d cases, while yet X was always present;

Therefore, none of these, but only A was cause of X each time.

But why the last part of our conclusion? Why may we not conclude that A was cause of X at one of its occurrences, and D at another, and G at another? A third syllogism precludes this:

1. "Like causes produce like effects."

2. None but A could be possible cause of all the Xs;

Therefore A was only cause of each X.

The method of difference (see p. 506) proceeds thus: In one case, or set of cases, A+B+C are followed by X+Y+Z. In another case, or set of cases, B+C are followed only by Y+Z. As we saw, to entitle us to proceed rigidly, we must know that in the second case, the absence of A is the only differing circumstance in the cluster of antecedents; that no other change in them has been made. We then conclude certainly that A caused X. The proceeding is a syllogism:

1. Like causes produce like effects.

2. But in the 2d case B+C did not produce X, which was present in the first case;

Therefore neither B nor C is cause of X. And, since there is no effect without its cause, A must be cause of X.

The third method of induction (see pp. 508, 509) was a combination of the two first, in which the affirmative result of the method of agreement was strengthened by the privative result of the method of difference. The syllogistic of the first part has been already given. In the second part, the process is like that of the method of difference.

1. Like causes always produce like effects.

2. But neither B+C+D, nor B, D, E, in the second class of instances, produced X;

Therefore neither of them is cause of X. But, as there can be no effect without a cause, A was the true cause of X.

The fourth method is that of residues (see p. 510). What observation gives us is a cluster of antecedents, A+B+C, usually followed by a cluster of effects, X+Y+Z. We prove that A produces only X, and B only Y. The inference which remains is, that C is the cause of Z. The syllogism is the following:

1. Like causes always produce like effects.

2. But A produces only X, and B only Y;

Therefore neither is cause of Z. But as there can be no effect without a cause, the remaining antecedent, C, must be cause of Z.

This formulation of the inference enables us to see with great clearness what are the conditions necessary to make it demonstrative. We must know, first, that A, B, and C are all the antecedents present which could be causal of Z; or, in other words, that there is no possible cause latent. We must know, first, that A or B produce only X and Y, and that Z is not also another effect of one of them or of their combination. For it is not impossible in itself that a cause may, under changed conditions, produce a second effect, different from the first, or at least differently conditioned. The intuition, Like cause, like effect, is only a universal truth while the cause is conditioned in the same way.

The last method of induction is that by noting the corresponding variations of antecedent and consequent. If a change in the circumstance of A is invariably followed by a corresponding change in X, we infer that A causes X. What is the analysis of this inference? Our intuition of cause is of that which has *efficient power* over its effect. This intuition involves the consequence that only an efficient cause could thus invariably propagate corresponding change in a sequent. But to make this consequence rigid, we must know that nothing varies in the cluster of antecedents, except that one of them which we suppose to be connected with the varying sequent. For, if other things among the antecedents vary, those other things may have to do with the variations in the sequent. But, with this caution, we may frame this syllogism:

1. Whatever sequent varies always with a given antecedent must receive its causal power.

2. But X varies always as A varies, no other change causal of X concurring;

Therefore X is the effect of A.

Thus, by the successive examination of all the methods of induction, it is shown that they are all virtually syllogistical. The simple and satisfactory conclusion is thus reached, which unifies our theory of logic, and which also secures for careful and sufficient inductions that apodeictic character which is so essential to make them scientific propositions, and which we yet saw denied to them by so many great logicians. Induction and deduction are not two forms of reasoning, but one and the same. The demonstrative induction is but that species of syllogism which, getting its minor premise from observed sequences of fact, gets its major premise from the intuition of cause.

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ERRATUM.—On page 504, line 27, for "method of argument," read "method of agreement."