THE SOUTHERN
PRESBYTERIAN REVIEW.

VOL. XXXIV.—NO. 1.

JANUARY, MDCCCLXXXIII.

ARTICLE I.

WHAT IS INDUCTIVE DEMONSTRATION?

The terms deduction, induction, are very currently used, and they seem to be regarded as signifying two contrasted methods of ascertaining truths. The description usually given in popular statements is, that, while deduction is the drawing down of an inference from a more general truth, induction is the leading in of a general truth from individual facts. There has doubtless been much bandying of the terms, which was not more intelligent than the word-play with that other pair of ambiguous terms, "analysis and synthesis." It is customary to say that Aristotle first examined and formulated the deductive logic or syllogism, and Bacon the inductive method. While almost entire barrenness is imputed to the syllogism, the glory of great fruit and utility is claimed for the induction. Some, indeed, are perspicacious enough to see that neither Aristotle nor Bacon was the inventor of the one or the other method of reasoning, any more than the first anatomists of human limbs were the inventors of walking. Nature has enabled men to walk, and ensured their doing so, with at least imperfect accuracy, by fashioning the parts of their limbs, nerves, bones, tendons, and muscles. The anatomist has only described what he found in the limbs by his dissecting knife. Men virtually syllogised before

Aristotle, and found inductive truths before Bacon. Yet even these more accurate historians seem to think that the two are opposite methods of logical progression.

These vague opinions of what induction is, are obviously unsafe. They lead to much invalid and even perilous reasoning. No stronger testimony against the unauthorised character of much that now calls itself physical science, under the cover of sophistical inductions, need be cited than that of J. Stuart Mill.[1] "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 familiar 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 bad induction than they were before Bacon wrote. . . 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." In these days, when the followers of physical research so often imagine the theologians to be in an active state of hostility against them and their sciences, it is well that we have this accusation from one as remote as possible from alliance with theology. This able witness proves at least so much: that every beam of light which can be thrown on the true nature of the inductive logic, though slender, is desirable; and will be useful both to purify the sciences of matter and to reconcile the conflict, if any such exists, between them and philosophy and theology.

We propose first to account for the vagueness which Mr. Mill has noted in the applications of this species of reasoning, by briefly displaying the uncertainties and discrepancies existing

[1] Logic, Vol. I., pp. 480, 481. 7th Edit., London, 1868.

among the logicians who have professed to treat of it. The modern admirers and expounders of Aristotle are found to deny that he did overlook the inductive method, and confine himself to the syllogistic; they claim that he formulated the one as really, if not as fully, as the other. But when they proceed to exhibit what they suppose to be the Aristotelian form of induction, they are not agreed. Thus, Grote's Aristotle (Vol. I., p. 268 etc., Murray, London) interprets him thus: "In syllogism as hitherto described, we concluded that A the major was predicable of C the minor, through B the middle. In the syllogism from induction we begin by affirming that A the major is predicable of C the minor; next we affirm that B the middle is also predicable of C the minor. The two premises, standing thus, correspond to the third figure of the syllogism (as explained in the preceding pages), and would not therefore justify anything more by themselves than a *particular* affirmative conclusion. But we reinforce them by introducing an extraneous assumption that the minor C is co-extensive with the middle B, and comprises the entire aggregate of individuals of which B is the universal, or class term." The instance Mr. Grote gives from Aristotle to explain the above is:

(1) Horse, mule, etc., etc., are long-lived.

(2) Horse, mule, etc., etc., are bileless.

(3) (Extraneous assumption.) The horse, mule, etc., etc., comprehend all the bileless animals—

(4) (Conclusion.) Hence, all bileless animals are long-lived.

Now, it is obvious to remark on this: that without the extraneous assumption the fourth proposition would not hold good as a universal truth. The third proposition, or extraneous assumption, then, is not an accessory, but an essential part of the logical process. But if Aristotle correctly defined syllogism as a process including the proof and conclusion in three terms and three propositions, this inductive process here supposed, whether valid or invalid, is not syllogism. A still more formidable question remains: How do we see that the extraneous assumption is warrantable? Are we entitled to assume that horse, mule, etc., etc., (an incomplete enumeration,) do contain all the bileless ani-

mals? Evidently, nothing contained in this formula authorises us. The process, then, as a proof of a general proposition, is inconclusive. It does not give us the form of a valid inductive proof, and is not the correct analysis of that mental process.

But Mr. Grote himself states that the prior commentators on Aristotle understand him differently. Thus—

(1) All horse, mule, etc., etc., is long-lived.

(2) All bileless is horse, mule, etc., etc.

(3) *Ergo*, all bileless is long-lived.

But Mr. Grote correctly remarks that, while, in form, this comes correctly under the first figure, it manifestly leaves the second proposition unwarranted, and authorises no universal conclusion. He also quotes M. Barthélemy St. Hilaire as explaining Aristotle thus: "Induction is, at bottom, but a syllogism, whose minor and middle are of equal extension. For the rest, there is but one sole way in which the minor and middle can be of equal extension: this is, that the minor shall be composed of *all* the individuals whose sum the middle represents. On the one part, *all* the individuals; on the other, the whole species which they form. The mind very readily makes the equation between these two equal terms." M. St. Hilaire is right, so far that, if this is the Aristotelian induction, it is perfectly valid. But it is equally clear that it is perfectly worthless, as we shall prove by the authority of Galileo. If we must ascertain the predicate to be true of each separate individual of the class, by a separate proof, before we can affirm that predicate of the class as a whole, then our general affirmation is certainly a safe one. But it can certainly teach us nothing, and authorise no progress in knowledge, because we have already learned in detail all it states, in our examination of the individuals. So Galileo. "Vincentio di Grazia objected to a proof from induction which Galileo adduced, because *all* the particulars were not enumerated. To which the latter justly replied that if induction were required to pass through all the cases, it would be either useless or impossible: impossible when the cases are innumerable; useless when they have each already been verified; since, then, the general proposition adds nothing to our knowledge." (Quoted in Whewell's Ind. Sciences, Vol. 2, p. 219.)

Whewell himself explains Aristotle after that general method of the commentators which Grote reprehends. Thus the former: "Induction is when, by means of one extreme term, we infer the other extreme term to be true of the middle term." This Whewell explains thus:

(1) Mercury, Venus, Mars, describe ellipses about the sun.

(2) All planets do what Mercury, Venus, Mars, do.

(3) *Ergo*, all planets describe ellipses about the sun. (Inductive Sciences, Vol. 2, p. 50.)

Again, we repeat, in our anxiety to have the reader see the real weak point in all these theories of induction, the fatal defect is in the second proposition. What authorises us to say that all planets do as Mercury, Venus, Mars, do? The theory of these authors gives us no answer; the assertion is not authorised; and the process, as a proof, worthless.

Ueberweg, Hist. of Phil., Vol. I., p. 156, represents Aristotle thus: "In induction (ἐπαγωγή, ὁ ἐξ ἐπαγωγῆς συλλογισμός) we conclude from the observation that a more general concept includes (several or) all of the individuals included under another concept of inferior extension, that the former concept is a predicate of the latter. (Analytics Prior. II., 23.) Induction leads from the particular to the universal (ἀπὸ τῶν καθέκαστα ἐπὶ τὰ καθόλου ἐφοδος. Topics, I., 10). The term ἐπαγωγή, for induction, suggests the ranging of particular cases together in files, like troops. The complete induction, according to Aristotle, is the only strictly scientific induction. The incomplete induction which, with a syllogism subjoined, constitutes the analogical inference (παράδειγμα), is principally of use to the orator."

We pass now from the Stagyrite logic to the method of Lord Bacon, which it is customary to represent as its antithesis. Bacon's claim to be the founder of modern physical science has been both asserted and contested. The verdict of Mill seems to be just: that he does deserve great credit, not so much for giving the real analysis of the inductive method, as for pointing us to the quarter where it lies. The very title of his *Novum Organum*, "Concerning the Interpretation of Nature," struck the correct key-note. The problem of all science, mental as well as physical (and it is

to be noted that Bacon claims, Book I., Aphorism 127, that his method is as applicable to mental and moral sciences as to material), is to interpret the facts given us by nature. The right method was doubtless pointed out when Bacon told the world, in the beginning of his *Novum Organum*, that instead of assuming general propositions, and then audaciously deducing from them, by syllogism, what causes and facts shall be, we are to begin in the opposite way, by the humble, patient, and accurate observations of facts, and then proceed, by legitimate inductions, to general and more general propositions concerning nature's laws.

Bacon says, Book II., Aph. 1, that as the work and design of human power is to induce upon a given body a new property or properties, so the work and design of human science is to discover the "form" of a given property. The whole tenor of his discussion shows that by "*natura*," he means any permanent property of a concrete individual thing. He himself has defined the sense in which he uses the word "form," with a clearness which admits of no debate. Thus, Book II., Aph. 17: "For when we speak of *forms*, we mean nothing else than those laws and determinations of pure activity which regulate and constitute some simple property (*naturam simplicem*), as caloric, light, weight, in every material thing and subject susceptible thereof." He admits that the old philosophy rightly declared, "*to know a thing truly, is to know it through its causes.*" These causes Aristotle had distinguished into four—the material cause, the formal cause, the efficient cause, and the final cause. In the investigation of nature, the inquiry after the final cause is out of place. He teaches elsewhere that it belongs to philosophy and natural theology. He also turns aside from inquiry into the material and the efficient causes, in their abstract senses. The problem of induction is to ascertain the regular law of the formal cause.

The directions for the interpretation of nature fall, then, under two general classes. The first show us how to derive general truths from experience; the second direct us how to apply these general truths to new experiments, which may further reveal nature. To deduce a general truth from experience, individual observations, there is, first, a task for the senses, that of accurate,

distinct observation of the individual facts of natural history; there is then a task for the memory, the tabulating of coördinate instances; and there is then the task of the intellect or reason, the real induction, which is the detection, among all the resembling and differing instances, of the universal law of cause. It is the last task in which the mind must have the aid of the proper canons of induction, by all attainable comparisons. Thus: let a muster, or array, be made of all the known individual instances in which the property which is the subject of inquiry is present. Then let another array be made of the known instances in which that property is absent. Then let another array be made of the known instances in which the property is present increased or diminished. When these sets of cases or arrays are carefully pondered and compared, the law (*forma*) of the property will begin to reveal itself by this principle: that whatever is always present with that property, or always absent when it is absent, or is found increased or diminished with it—that is the cause of the property. This inductive process is then illustrated at tedious length by an application to the inquiry, What is heat? First, a list is made of all known individual things in nature which exhibit heat, as solar rays, combustive masses, fermenting masses, quick-lime moistened, animal bodies, etc., etc. Then a list is made of bodies which exhibit no caloric, as the fixed stars, the moon, etc. Then lists are formed of objects more or less warm; and the *vindemiatio*, or induction to the true *forma*, or law of caloric, may be cautiously made. This is, that "*Caloric is an expansive motion*, repressed, and striving in the lesser parts of the warm body." (Book II., Aph. 18.) This first *vindemiatio* is then to be tested and confirmed by considering a number of *prerogatival instances;* which are particular instances presenting the property under such circumstances as give them the prerogative of determining the law of the property. Of such instances, twenty-five are enumerated! and with a refinement and intricacy of distinction which must be utterly confusing to a practical investigator.

The disparaging verdict which Mill pronounces upon this technical part of the Baconian *Organum*, must be admitted to be

just. Yet it should be mitigated by the fact that, cumbersome as the proposed canon is, it seems to have led Bacon, centuries in advance of his age, in the direction of the latest theory as to what caloric is. That theory now is, that caloric is a mode of molecular motion. Bacon's conclusion was that it is "the striving of an expansive but restrained motion in the lesser parts of a body"! His method was not mere groping: it foreshadowed an imperfect truth. In the light of fuller inquiries, Bacon's errors seem to have been these: that his contempt for the abstract in metaphysics led him to neglect the fundamental notion of *power in the efficient cause*, discriminating it so vitally from the material, formal, and final causes, and thus to depreciate the inquiry into efficient cause; that he had not pondered and settled this other truth of metaphysics, the relation between power and properties in individual things; and that he applied his induction, in his favorite examples, to detect the *forma*, or law of a property, instead of the *laws of effects*. It is the latter inquiry in which inductive science is really concerned, and the solution of which extends man's powers over nature. The thing we wish inductive philosophy to teach us is, How may we be sure to produce, in the future, a given desired *effect*, which has been known in the past?

The illustrious Newton, who did more than any other to throw lustre on the new method by its successful application, presents us, in his four Rules (Principia, 3d Book), a substantive advance upon the rude beginnings of Bacon. These rules are far from being exhaustive; nor are they stated in an analytic order, but they are the sound dictates of the author's experience and profound sagacity.

"1. We are not to admit other causes of natural things than such as both are true (not merely imaginary) and suffice for explaining their phenomena.

"2. Natural effects of the same kind are to be referred to the same causes, as far as can be done.

"3. The qualities of bodies which cannot be increased or diminished in intensity, and which belong to all bodies in which we can institute experiments, are to be held for qualities of all bodies whatever.

"4. In experimental philosophy, propositions collected from *phenomena* by induction are to be held as true, either accurately or approximately, notwithstanding contrary hypotheses, till other phenomena occur, by which they may be rendered either more accurate or liable to exception."

Sir William Hamilton, in his Logic, Lect 17th, describes his "inductive categorical syllogism" as "a reasoning in which we argue from the notion of all the constituent parts discretively, to the notion of the constituted whole collectively. Its general laws are identical with those of the deductive categorical syllogism; and it may be expressed, in like manner, either in the form of an intensive or of an extensive syllogism." This he calls "logical or formal induction." The process is precisely that which we have seen described by St Hilaire: When a given predication has been found true of every individual of a class, it is also true of the class as a whole. This is unquestionably true; but as unquestionably useless, as we have seen from the statement of Galileo. It gives us only a truism, and no new truth. But Hamilton proceeds to distinguish from this what he calls the "philosophical or real induction," in which the argument is not from all of the individuals in a class to the class as a whole; but from a part of the individuals to the whole. He says that the validity which this induction may have, is not from the logical law of identity, but from a certain presumption of the objective philosopher, founded on the constancy of nature. This species of induction proceeds thus:

(1) This, that, and the other magnet, attract iron.

(2) But this, that, and the other magnet, represent all magnets.

(3) *Ergo*, all magnets attract iron.

This doctrine he again enlarges in his 32d lecture, where he treats of modified logic, and deals with the "real or philosophical induction" expressly. He again makes it an inference from the many to the all. To the soundness of such an induction two things are requisite: that the cases colligated shall be of the same quality, and that they shall be of a number competent to ground the inference. But to the question, How many like cases are competent? he has no answer. This species of induction, he

admits, cannot give a categorical conclusion. It only raises a probability of truth, and leaves the conclusion a mere hypothesis, sustained by more or less of likelihood. That likelihood is, indeed, increased as a larger number of cases is compared, as the observation and comparison are made more accurate, as the agreement of cases is clear and precise, and as the existence of possible exceptions becomes less probable after thorough exploration. Hamilton concludes by quoting with approbation these words from Esser's Logic: "Induction and analogy guarantee no perfect certainty, but only a high degree of probability."

The objection against the Aristotelian syllogism of induction, which we urged on pages 3rd and 5th, had been stated by Archbishop Whately. Let it be put thus:

(1) This, that, and the other magnet, attract iron.

(2) But this, that, and the other magnet, etc., are conceived to constitute the genus magnet.

(3) *Ergo*, the genus magnet attracts iron.

Whately's objection is, that *the second proposition is manifestly false.* Hamilton pronounces this, which appears to us a fatal, "a very superficial objection." His reason is, that it is extra-logical; that logic is a formal science only; and that hence the correctness of its forms is not vitiated by the circumstance that some proposition expressed in them and correctly connected, so far as these forms go, with other propositions, is in fact untrue, and that the imaginary propositions with which the text-books of logic illustrate the logical forms answer just as well, whether they be really true or not. Hamilton is here clearly misled by a confusion of thought. Because an imaginary, or even a silly, proposition may serve to illustrate a rule of logic, when that rule is the subject of inquiry, it does not follow that, when the ascertainment of other truth by the use of the rules of logic is our object, that can be a good logic whose framework always and necessarily involves a false proposition. Blank cartridges may serve very well for the purposes of an artillery drill; it by no means follows that blank cartridges are adequate for actual artillery practice in war. Such artillery would be practically no artillery; for it would repulse absolutely no enemy. And such logic would be practically

no logic. Logic is a formal science. True. But it professes to give the general forms of elenchtic thought, by which the truth of the propositions of all other sciences, besides logic, may be ascertained. Hence, if it proposes to us a given form of thought which is always and necessarily invalid in every real science to which logic offers its method, that form is incorrect as a logical form. We affirm Whately's objection, then, in order to call the reader's attention again to the fatal weak spot in these theories of induction.

What, then, is Whately's own explanation of the inductive syllogism? See his Logic, Book IV., Chap. 1. He begins by justly distinguishing two uses of the word induction, which are entirely different. The one process is not a process of argument to the conclusion, but is wholly preliminary thereto, the ἐπαγωγή, or bringing in of like instances; the collecting process; and this is, in fact, nearer to the literal meaning of the word. The other process called induction, is the argumentative one, leading in the conclusion, as to the whole class, from the instances. Now, of this logical induction, Whately remarks that, instead of being different from the syllogistic, it is the same with it. And, indeed, unless we assert its sameness, we must give up the theory of the syllogism; for that theory is, that syllogism expresses the one form in which the mind performs every valid reasoning step. The logical induction is, then, says Whately, a syllogism in the first mode and figure, with its major premise suppressed. That suppressed major is always substantially the same in all logical inductions: *that what belongs to the individual cases observed, belongs to their whole class.* The induction by which we predict, in advance of individual examination, that all magnets will attract iron, would then stand thus, according to Whately:

(1) What belongs to the observed magnets, belongs to all magnets.

(2) But these observed magnets attract iron.

(3) *Ergo*, all magnets attract iron.

Now the reader will observe that Whately's process only inverts the order of the first two propositions in Hamilton's. For Whately's first is only a different way of expressing Hamilton's second: that

(2) "This, that, and the other magnet, represent all magnets."

The order of propositions given by Whately seems obviously the simple and correct one. But the difficulty he had propounded as to the Aristotelian form of the induction, recurs as to his: How have we ascertained our major premise, that what belongs to the observed magnets belongs to the whole class? Are we entitled to hold it as a universal truth? The same difficulty virtually meets Whately. It is amusing to find him attempting to parry this fatal difficulty in a way similar to that which Hamilton uses to parry him: "Induction, therefore, so far forth as it is an *argument*, may, of course, be stated syllogistically; but so far forth as it is a *process of inquiry*, with a view to obtain the premises of that argument, it is, of course, out of the province of logic." The evasion is as vain for Whately as it was for Hamilton. For that universal major premise, viz., that what belongs to the observed individual cases belongs to the whole class, can no more be the immediate non-logical result of a mere colligation of cases, than the conclusion itself of the inductive syllogism can be. Whately has himself admitted that if a premise used in a syllogism now in hand was a conclusion of any previous reasoning process, then our logic must concern itself about that premise also, and the mode by which we get it, as well as about the form of its relations to the other propositions in our present syllogism. Now, the universal major he claims, is not the mere expression of an extra-logical colligation—that is self-evident. Unless it is an original intuition, it must be the conclusion of a prior logical process. What is that process? Is this universal major valid? Whately gives us no sufficient answer; and thus his theory of inductive argument fails like the others. Yet, it presents us, as we shall see, one step in advance of the others, towards the right direction.

Dr. Whewell deserves mention also, by reason of his wide learning, extending into the domains of physics and metaphysics, and his authorship of a work, once a standard, devoted to this very subject. This is his "Philosophy of the Inductive Sciences." His view of induction may be seen in these citations (Vol. I., p. 22): Where "truths are obtained by beginning from observation

of external things, and by finding some notion in which the things, as observed, agree, the truths are said to be obtained by induction." Contrasting deduction with induction, he says, "Deductive truths are the results of relations among *our thoughts*. Inductive truths are relations which we perceive among existing *things*." And of the deductive process he thinks the geometrical demonstrations the best examples.

Now, the insufficiency of these descriptions is obvious from these remarks. Lines, angles, surfaces, solids, in geometry, are as truly *things* as any observed phenomena or effects in physics. Thus the distinction wholly fails. Again, Whewell has combined, in his description of induction, two processes of mind which are wholly distinct, and only one of which is a logical process. Both have, indeed, been called induction (in different senses), but the first is only a *colligation* of observed things or facts. This process only completes a general statement which gives correct expression to a series of individual observed facts, when taken as a whole. The instance given by another presents this process very simply: A navigator in unknown seas beholds land; he knows not whether it is continent or island. But he sails along its shores, noting its bays and headlands, and taking ocular evidence of the continuity of the whole coast, until he beholds again the same spot he first saw. He calls the land now an island. But he has made no *logical inference;* he has but colligated all his separate notes of the coasts, with their connecting continuity, into that general concept of which "island" is the correct name. Now, this is really what Kepler did when he performed what has so often been cited as a splendid instance of induction: from a number of observed angular motions of the sun in the ecliptic, he declared that the earth moved in an ellipse, with the sun at one of the *foci*. The real process was but to plot and colligate upon a plane surface, all the successive positions of the earth; whereupon inspection showed that the line she had pursued was elliptical. A still simpler and equally illustrious instance of this process was given when Maury enounced the general facts of his wind-and-current charts. His results were obtained by faithfully plotting, upon blank charts of the oceans,

the directions of the winds and currents, with the successive dates, from a multitude of actual observations in sailors' log-books. When this humble but noble work was patiently done, the general facts as to the directions of the winds and currents, at given seasons, revealed themselves to inspection. Here was a grand colligation, but, as yet, no inference. But we have a true instance of inductive inference when Newton derived the great law of the attraction of gravitation, as expressing the true cause of that elliptical circulation. Kepler had colligated only a general fact; Newton inducted a law of cause. Whewell seems, p. 23d, to confound them.

But on p. 48th he speaks, if still too indefinitely, yet more nearly to the truth. "Induction is familiarly spoken of as the process by which we collect a *general proposition* from a number of particular cases; and it appears to be frequently imagined that the general proposition results from a mere juxtaposition of the cases, or, at most, from merely conjoining and extending them." . . "This is an inadequate account of the matter." . . "There is *a conception of the mind* introduced into the general proposition, which did not exist in any of the observed facts." The phrase "conception of the mind" is indeed an inaccurate expression for the missing but all-important element of the logical induction. But Whewell had perceived so much: that this element of proof was not in the mere colligation of agreeing instances alone, but was to be furnished from another source. And he points our inquiries in the right direction, in seeking this vital premise among the intuitive judgments of the reason. It is to be found in that judgment which so many of these writers speak of as our *conviction of the uniformity of nature!* Thus, in substance, answer the most of them, as Hamilton and his great German authorities, Krug and Esser. But this is the question.

The comments of Lord Macaulay on the inductive method, in his famous Essay on Lord Bacon, justify the angry estimate of his comrade, Brougham, by their superficial character. But they may also serve to show how just the complaint of Mill is as to the confusion of the opinions of even educated men on this subject. Macaulay, with his usual plausible brilliancy, assures us that the

method of the *Novum Organum* was nothing more than the familiar experimental argument of the English squire as to the cause of his bodily ailments. The result of the squire's induction is to trace his sufferings to his indulgence in his favorite dainties. On the nights after free indulgence he suffered much. On nights when he had wholly abstained, he was free from pain. On nights when he had indulged sparingly, he suffered slightly. Here, intimates Macaulay, we have the whole Baconian process, the *comparentia instantiarum similium*, the *exclusiones instantiarum negativarum;* the *comparationes pluris aut minoris.* He seems to think that this embraces the inductive logic!

Fleming, in his "Vocabulary of Philosophy," after citing numerous definitions of induction, which exhibit the uncertainties and confusions criticised in these pages, gives his own statement thus: "By the principle of induction is meant the ground or warrant on which we conclude that what has happened in certain cases, which have been observed, will also happen in other cases which have not been observed. This principle is involved in the words of the wise man, Eccles. i. 9: 'The thing that hath been, it is that which shall be; and that which is done is that which shall be done.' In nature there is nothing insulated. All things exist in consequence of a sufficient reason; all events occur according to the efficacy of proper causes. In the language of Newton, *Effectuum naturalium ejusdem generis eædem sunt causæ.* The same causes produce the same effects. The principle of induction is an application of the principle of causality," etc. Of this description we may say what was said of Whewell's, but with more emphatic approval: that it points us in the right direction.

We now introduce the definitions of three contemporary American logicians. The Rev. Dr. McCosh says (Div. Gov., p. 289): "Induction is an orderly observation of facts, accompanied by analysis; or, as Bacon expresses it, the 'necessary exclusions' of things indifferent, and this followed by a process of generalisation, in which we seize on the points of agreement."

Professor Bowen, Logic, p. 380, teaches that induction is from some observed cases to the many not observed; and he passes this verdict on the process: "But just so far as they" (induc-

tions) "are means to these ends, they lose the character of pure or demonstrative reasonings, the syllogisms to which they are reducible are faulty, either in matter, as having a major premise the universality of which is merely *probable;* or in form, as containing an undistributed middle."

"Induction, properly so called, concerns the matter of thought, and concludes from *some* to *all.*"

Dr. Porter, Elements of Intellectual Science, Abr. Ed., p. 393, says: "Judgments of induction differ from simple judgments in several important particulars. (In the simple judgments we bring the individuals under the appropriate common concept.) In induction we proceed farther: we add to those simple judgments yet another, viz., that what we have found to be true of these, may be received as true of all others like them. The ground of the first judgment is facts observed and compared. The ground of the second is what is called the *analogy of nature.* A judgment of induction is, then, *a judgment of comparing observation, enlarged by a judgment of analogy.* The judgment of observation is founded on *an observed similarity;* the judgment of induction on *an interpreted indication.*"

We have postponed to the last the notice of two celebrated philosophers, Dugald Stewart and John Stuart Mill, because they both exhibit, as a common trait, the influence of their countryman, Hume, in wresting their views from the truth. Stewart (Vol. 3d, Chap. 4th, of the Method of Inquiry pointed out in the Experimental, or Inductive, Logic), amidst many elegant, but confused, digressions, reaches substantially the same view of inductive reasoning with his predecessors. P. 246. "When, by thus comparing a number of cases agreeing in some circumstances, but differing in others, and all attended with the same result, a philosopher connects, as a general law of nature, the event with its *physical cause*, he is said to proceed according to the method of *induction.*" "In drawing a general physical conclusion from particular facts, we are guided merely by our instinctive expectation of the continuance of the laws of nature; an expectation which, implying little, if any, exercise of the reasoning powers, operates alike on the philosopher and on the savage." . . "To

this belief in the permanent uniformity of physical laws, Dr. Reid long ago gave the name of the *inductive principle.*"

Stewart seems to admit by implication what we have seen Hamilton and Bowen assert so plainly, that the physical induction can give only a probable evidence, and can never demonstrate absolutely a universal truth. For Stewart, in commenting on the interesting fact that the inductive method is applicable in mathematics, reminds us that it was only by this method Newton proved the binomial theorem; and then proceeds to argue, pp. 318, 319, that, had this theorem not really been sustained by some principle more valid than is found in any physical induction, mathematicians would not have accepted it as universally true for all exponents of the $(a+x)$. All the proof, says he, which Newton seemed to have of the binomial theorem, was to expand the products, by actual multiplication, of the $(a+x)$ to the 2d, the 3d, the 4th, and to such a number of powers, as satisfied him that the laws he found prevailing for the number of terms, and the exponents and coefficients in all the products actually inspected, might be trusted to prevail in all other powers, however high. Now, had this been really all, Stewart thinks we should have had, in this mathematical formula, a specimen of induction exactly like physical induction. And he evidently thinks it could not have been demonstrative of the universal truth, but only evidential of the probable truth of the *formula* for untried cases. He thinks there is really, latent in the process of Newton, a further evidence, which is demonstrative: that when the actual multiplications are pursued to several powers, the mind sees a reason why the coefficients and exponents not only do, but must, follow the law observed by inspection in the products expanded. Does not this imply that in the case of physical inductions, a similar *desideratum* is lacking? Surely. But Stewart does not supply it. Surely, he cannot think that he finds it in "permanent uniformity of physical laws," which he regards as the inductive principle; for he thinks it is instinctive, rather than rational. Thus he leaves his system of inductive logic as baseless of solid foundation as the others.

But the worst legacy of the philosophy of Hume he leaves us,

is his distinction between the physical cause and the efficient cause. The physical cause is the invariable actual antecedent of the *phenomenon* regarded as effect. The efficient cause is the secret unseen power the mind imputes; and he declares the word *power* expresses an attribute of mind, not of matter. He expressly declares that the object of induction is to seek, not the efficient, but the physical cause. Pp. 230, 231. And his reasons are but the deceptive ones of the sensationalistic philosophy which misled, in part, even Brown and Stewart, and so much more sadly, Mill: that observation of physical sequences gives us nothing but a regular antecedent and consequent; so that physical science should have to do with nothing more. That this often repeated conclusion is utterly sophistical appears from these two tests: observation of physical phenomena gives us no general concepts; for all philosphers agree that nature presents *to the eye* nothing but *individual* things and *phenomena*. Shall physical science, therefore, have no business with general concepts and universal propositions? Again, nature presents to the eye no inference of any kind. Shall physical science then discard inference? Carry out this argument, and man's relation to nature must sink to that of the cunning brute, the ant or the beaver. Hence it appears that, if there is to be any science or any theory, elements must be contributed to it from the subjective powers of the mind, as well as from the outward observed facts and things. Stewart was the more unpardonable for making this concession against the inquiry for the efficient cause, for that he is not really a sensationalist, but admits the mind has intuitive notions and judgments. He should have remembered that, granting what the eyes observe in the rise of a *phenomenon* is only its regular antecedent, we rationally supply to the real causal antecedent, as its own property, the notion of *power*. Just as when by the senses we perceive a cluster of properties of a concrete thing, the law of the reason necessitates our supplying the notion of *substance*. It is impossible for us to think the antecedent which seems next the effect the real next antecedent, unless we judge it to emit the *power* efficient of the effect. In a word, the physical cause can, in truth, be none other than the efficient cause. If we do not know, by

sense-perception, what the power is, we rationally know that it is; if we do not know its τὸ πῶς, we do know its τὸ ὅτι. Hence, its reality is as proper a ground for argument and inference as the reality of any concrete body. Do we know what the energy we call electricity is? Yet we construct a thousand experiments to seek it, and inferences from its power. Stewart ought to have affirmed, then, precisely what he denied; what Newton affirmed: that the real object of the inductive inference is *to find the efficient cause.*

We shall see that the chief, the only useful, problem of induction is, to ascertain the certain laws of given effects. *How can an antecedent bring the effect certainly after it, unless it be efficient thereof?* To limit induction, as Stewart and Mill do, to the ascertainment only of the physical antecedent, is to forbid induction from ever rising above the probabilities of mere enumerated sequences, whose worthlessness to science Bacon has so well exposed. Have we not the clue, in this refusal of the search after the efficient cause, to the imperfections and confusions of their treatment? We repeat, the reversal of this *dictum* of theirs is vital.

Mill is at once the best and the worst of all the English-speaking logicians, in his treatment of the inductive logic. His insight into its true nature is far the most profound and correct; and his technical canons of induction the most simple and accurate at once. But his error as to the rudimental doctrine, which underlies all his admirable discriminations, is the most obstinate. To him eminently belongs the credit of vindicating for the inductive logic the character of a true demonstration, and of showing where that demonstration is founded. Having set aside the inaccurate uses of the word induction, he defines as follows (Bk. III., Ch. II., § 1):

"*Induction*, then, is that operation of the mind by which we infer that what we know to be true in a particular case or cases, will be true in all cases which resemble the former in certain assignable respects." (Chap. III., Sec. 1.) "It consists in inferring from some individual instances in which a *phenomenon* is observed to occur, that it occurs in all instances of a certain

class; namely, in all which resemble the former in what are regarded as the material circumstances." But since the mere observation of a similarity of sequence in a number of instances does by no means authorise this expectation as to instances not observed—a truth which Mill here implicitly recognises, and elsewhere expressly acknowledges—the all-important question remains, What is it that authorises the mind to infer positively, in the case of the valid induction, that the unobserved instances will be like the observed? He answers (§ 1): "The proposition that the course of nature is uniform, is the fundamental principle or general axiom of induction." "If we throw the whole course of any inductive argument into a series of syllogisms, we shall arrive by more or fewer steps at an ultimate syllogism, which will have for its major premise the principle or axiom of the uniformity of the course of nature." Again (Chap. V., § 1), recognising the general law of logic, that only universal premises can yield universal conclusions in the mathematical reasonings, he admits that it must be so likewise in inductive reasonings. "This fundamental law must resemble the truths of geometry in their most remarkable peculiarity, that of never being, in any instance whatever, defeated or suspended by any change of circumstances." But where do we find such a universal principle? He answers: "*This law is the law of causation.*" (§ 2.) "On the universality of this truth depends the possibility of reducing the inductive process to rules." "The notion of *cause* is the root of the whole theory of induction." And most emphatically (in Chap. XXI., § 1) having expounded his canons of induction, for discriminating between the sequences which authorise, and those which do not authorise, expectation of the same *phenomena* recurring, he says: "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."

But this excellent doctrine he then fatally neutralises by the doctrine of the sensationalists concerning the notion of causation. This he declares to be of empirical origin (Chap. V., § 2): "The only notion of a cause which the theory of induction requires, is

such a notion as can be gained from experience." He deems that the tie of power, which we think the reason, but not the senses, sees between cause and effect, is "such as cannot, or at least does not, exist between any physical fact and that other physical fact on which it is invariably consequent, and which is popularly termed its *cause*." He distinguishes, with Reid and Stewart, between the physical and the efficient cause, and declares that induction concerns itself only about the physical cause. With him, causation is "*invariable, unconditional antecedence;*" nothing more.

Again (Chap. V., § 3), after referring to the truth that a sequent effect is not usually found to be the regular result of a sole antecedent, but of a cluster of several antecedent *phenomena* and states, he claims that all these regular antecedents are equally cause, and that the mind has no ground for assigning efficiency to one more than another. He seeks to abolish the distinction between the efficient causes and the conditions of an effect. If one eats of poisonous food and dies, we have no reason to call the poison the *cause* of the death, rather than the idiosyncrasy of the man's constitution, the accidental state of his health at the time, and the state of the atmosphere, for all had some concurrent influence to occasion the result. "The real cause is the whole of these antecedents; and we have, philosophically speaking, no right to give the name of cause to one of them, exclusively of the others."

These *dicta*, as we shall show, are subversive of the author's own better doctrine, cited in the previous paragraph. For it is easy to see that, if they were true, they would be fatal to that certainty and universality which he has himself correctly demanded for the major premise of all inductions. Waiving, for the present, the discussion of the question, whether our notion of causation is empirical, we would point out that there is, obviously, no invariable, no certain connexion between the mere condition of an effect and its actual rise. This condition must be present, if it is a *conditio sine qua non*, in order to the rise of the effect; but it may be duly present, and yet the effect may not come. This simple remark shows that, were efficient cause no more in-

variably connected with effect than is a condition, then cause and effect would not have any of that uniformity and universal certainty of effect which, Mill admits, is essential to ground the inductive argument. But he asserts that the condition is part cause, and as much entitled to be viewed as real cause as any other part of the antecedents supposed to be more efficient. Thus he contradicts himself. This suggests the further argument, that our common sense is not mistaken in ascribing an efficiency or power to the cause such as it does not ascribe to the occasion; because we know, experimentally, that the true cause has a connexion with the effect more necessary than the occasion has. Oftentimes conditions may be changed, and yet the regular effect continue to occur; but if the truly causal antecedent be lacking, all the appointed conditions remain dumb and barren of effect, though duly present. For instance: in order that germination may result, there must be moisture, warmth, and vegetable vitality in the seed. Can any reasoning man believe that moisture or warmth is as essentially efficient of the growth as the vital energy is? No. For he sees that all the water in the sea and all the caloric in the sunbeams conjoined, would never produce growth until the vital germ is added. But as soon as this is present, in addition to the other two, the growth regularly takes place. They are conditions, this alone efficient cause of living, vegetable growth. Mill has evidently been unconsciously deceived by the fact that there are effects in which more than one *vera causa* concur as efficients, in addition to certain conditions. Thus, in the case of a moving body, driven by two forces in different lines, each force is *true cause* of the resulting diagonal motion, in addition to the other *conditions* of mobility.

But to us this appears to be the crowning proof of error in this doctrine of Mill, that often we find conditions of effects which are merely negative. Yet they may be conditions *sine qua non*. The burglar was enabled to effectuate his felonious purpose of burning the dwelling by reason of the absence of the fire-engine. How could an engine, *which was absent*, exert efficiency in the destruction of the house? The very amount of this condition was, that this engine exerted absolutely no efficiency, did nothing in the case.

The error of Mill's doctrine appears also when it is carried into psychology. Our author is, in a sense, a Necessitarian, or, at least, a Determinist, in his theory of volition. Now, when a given volition rose, the whole set of conditions attending its rise included a certain subjective motive, which was a complex of a certain judgment and appetency; and a certain objective inducement, not to say other circumstances, conditioning the feasibility of the volition. According to Mill, this whole cluster of conditions, taken together, should be regarded as the cause of that volition; and one element has as much right to be regarded as efficient thereof as another. Then, the objective inducement and the subjective motive were as really efficient, the one as the other? Where, then, was the agent's rationality and free agency? In the objective presentation of the inducement, the man's spontaneity had no concern, in any shape. To him, that presentation was as absolutely necessitated as the fall of a mass unsupported. Hence, if that objective inducement was as truly *cause* of his volition as his inward appetency was, his free-agency was a delusion, and his act of soul was absolutely necessitated. But of his exercise of these attributes in that volition, his consciousness assured him. We thus vindicate that philosophy of common sense which distinguishes the real efficient from the mere conditions of an effect. It is the presence of the former which determines and produces the effect; the others are merely conditions *recipient* of that effect.

This review of the history of the inductive logic the reader will find to be not a useless expenditure of his time. It has not only traced the growth of the doctrine in its progress towards correctness; but it has familiarised his mind to the terms and ideas with which he has to deal in the further study. It has given us opportunity to criticise and establish the proper views on some points, like the one last discussed, which will be found vital to the development. And above all, it has disclosed to us the true problem which yet remains to be solved, to complete that development. The most important points of this review to be resumed are these: that "induction" has been used to describe three distinct processes of the mind—of which the first is the colligating of many resem-

bling percepts into one general concept of the mind; the second is the inference to the truth of the predication concerning the whole from its ascertained truth concerning each and all of the individuals of that whole; and the third is, the inference from some observed instances to all the other unobserved instances of the class.

That the first of these processes the writers we have consulted declare to be no logical process at all, but only a preliminary thereto; that the second was found by us perfectly valid, but also perfectly useless, except as a compendious form for recording knowledge already ascertained; that the third is the useful process of the inductive inquiry, and the only one which really extends our knowledge or our power over the previously unknown. But the vital problem about this process is, *how* the ascertainment of only some of the resembling instances entitles us to infer a universal rule, which shall be held true of cases absent in space, or future in time, from the sphere of the actual observation? That the answer given is, our expectation of the "uniformity of nature" is what entitles us; and that the best of our teachers, as Newton, Fleming, and Mill, ground that expectation in the law of causation.

But that we may comprehend the difficulty and gravity of the main problem, we must inquire whether this expectation of the uniformity of nature is valid, and whence it is derived. Does nature, in fact, present an aspect of uniformity? Far from it. A very great part of her *phenomena* are unexpected and unintelligible to men. The unlikely and the unexpected is often that which occurs. Whole departments of nature refuse to disclose any orderly law to man's investigations, as the department of meteorology refused to our fathers; so that the results which arise are well described to our apprehension by the phrase, "as fickle as the winds." That the aspect of nature is to the popular and unscientific observer almost boundlessly variable and seemingly capricious, is shown by the sacrifices of the Romans to the goddess *Fortuna*, whom they supposed to rule a large part of the affairs of men, and whose throne they painted as a globe revolving with a perpetual but irregular lubricity. What else do we

mean by our emphatic confessions of our blindness to the future, than that the evolutions of nature are endlessly variable to our apprehension; and for that reason, baffle our foresight? See Mill, Chap. 21: "It is not true, as a matter of fact, that mankind have always believed that all the successions of events were uniform and according to fixed laws. The Greek philosophers, not even excepting Aristotle, recognised Chance and Spontaneity as among the agents in nature," etc., etc. So, Baden Powell, Essay on the Inductive Phil., pp. 98–100. No writer has made more impressive statements of this uncertainty of the aspects of nature than that idolater of the inductive sciences, Auguste Comte. His *Philosophie Positive* says of her energies: "Their multiplicity renders the effects as irregularly variable as if every cause had failed to be subjected to any precise condition. It is only where natural causes work in their greatest simplicity and smallest number, that any appearance of invariable order is obvious to the common observer. As soon as the number of concurring or competing causes becomes larger, and the combinations more intricate, the resultant *phenomena* begin to wear to us the aspect of a disorder which obeys no regular law whatever." Such is Comte's confession. This suggests the question, What, then, authorised the observer to postulate this proposition, that "nature is uniform"? Shall it be said that he is authorised to do so because his inductions have led him to detect latent laws of order amidst nature's seeming confusions? But the postulate of nature's uniformity was, as it appears, necessary to his first inductions. Whence did he derive it at the beginning? Is his induction all reasoning in a circle? The same philosopher has also pointed out this general fact, that the departments of nature, in which her causes are few and simple, and her movements therefore uniform, are the very ones which are farthest from man and from his control; while in those departments which are nearest to him, which most concern him, and which it is most desirable for him to control, causations are most innumerable and complicated, and all principle of uniform order most latent. The heavenly bodies move in orbits, under the operation of two forces only; and hence their movements are manifestly regular, intel-

ligible, and capable of exact prediction. Astronomy is the most exact of the physical sciences. But these stars are the farthest bodies from us, and the ones over which we can have absolutely no control. As we approach nearer to our human interests and persons, natural causations become more numerous and intricate. The chemistry which governs in the composition of our food and medicines, presents us with physical energies much more numerous and subtile than the two forces, centrifugal and centripetal; and in that science results are far less regular and capable of prediction by us, just as they are nearer and more important to us. But when we come still nearer, to the vital energies which govern our health, disease, pain, or ease and death, there the appearance of uniformity is least, and the fortuity seemingly greatest. No man knoweth "what a day may bring forth." How, then, are we warranted to set out with this assumption of the "uniformity of nature"? How is it that we claim to account for her actual complications and apparent fortuities, thus embarrassing us at every turn, by our hypothesis of the inter-actings of *latent laws;* when the very question is, whether these irregularities do not refute the very idea of permanent law in her realm?

If it be urged that there are regularities amidst the seeming fortuities of nature, and that induction may proceed from these regularly recurrent instances, we shall be met with another difficulty. It is demonstrable that no amount of mere regularity in a recurring sequence can amount to demonstration that the same sequence will recur in the future. The customary apprehension of the inductive argument seems to be thus: that if a given *phenomenon* be actually observed to go immediately before another a *sufficient number of times*, this justifies the postulating of a regular law. And such, in fact, is the amount of most of the so-called scientific observation and argument. If one asks, *How many* observations of the same recurring sequence are sufficient to reveal, and thus to prove, a law; no consistent answer is given to us. And let it be supposed that any answer whatsoever were given us—as that fifty or five hundred entirely agreeing instances would be sufficient to establish a law—then we must ask, *What is there different* in the last crowning instance, say the

five-hundredth, which makes it conclusive of a law, when the four hundred and ninety-nine were not? The argument was begun on the assumption that they were to be all agreeing instances; for the disagreeing instances would rather cross and contradict the induction than strengthen it. And yet this five-hundredth must have something in it different from the four hundred and ninety-ninth, for that is conclusive where this was not. To this difficulty also we get no consistent answer.

In truth, the inquiry has proceeded far enough among the inductive logicians, to prove thus much, absolutely, that this species of induction, which does no more than count up agreeing instances of sequence, can never be a demonstration. Bacon calls it the "*Inductio per enumerationem simplicem.*" His verdict against its validity may be found in the *Nov. Organum*, L. I., Apothegm 105: "Some other form of induction than has been hitherto in use, must be excogitated in establishing an axiom" (general principle). "And this is necessary, not only for discovering and proving what they call *first truths*, but also the lesser and the mediate axioms; in fine, all axioms. For an induction which proceedeth by simple enumeration, is a puerile affair, and gives a precarious conclusion, and is liable to peril from a contradictory instance; and oftentimes it pronounces from fewer instances than is meet, and only from such as lie readiest at hand." So Mill (Book III., Chap. III., § 2): "To an inhabitant of Central Africa, fifty years ago, no fact probably appeared to rest on more uniform experience than this, that all human beings are black. To Europeans, not many years ago, the proposition, all swans are white, appeared an equally unequivocal instance of uniformity in the course of nature. Further experience has proved to both that they were mistaken." (See also Chap. XXI., Vol. II., p. 101.) So speak all the thoughtful writers. The invalidity of such induction is also proved by familiar examples. Experience observes the invariable death of our fellow-men. We confidently expect all living men, including ourselves, will die. Experience has, with equal certainty, shown us night always preceding day within the limits of twenty-four hours; for we live between the arctic circles. But no man dreams that night or

darkness *causes the day;* and if he concluded that the sequence must hold as he has seen it, he would be refuted by the first winter within the arctic circle. Every man who rises early enough, hears the cock crow invariably before the dawn; no man infers that the cock's crowing causes dawn, or must necessarily precede it. Babbage's calculating machine presented a curious refutation of this species of induction. Its machinery could be so adjusted by the maker, as to present to the eye a certain series of numbers, increasing by a given law, and this was continued through instances so numerous as to weary the spectator. Did he now conclude that these numerous agreeing instances revealed to him the necessary law of the machine? He was speedily refuted by seeing it change the law of the series by its own automatic action.

But does not such an enumeration of agreeing instances teach anything? We reply that it does raise a probability of a law which may be found to regulate the future rise of similar instances. The more numerous the agreeing instances summed up, the more this probability will usually grow; and when, by our own observation and the testimony of our fellow-men, the agreeing instances become exceedingly numerous, and none of a contradictory character appear, the probability may mount towards a virtual certainty. The ground of this will appear when we have advanced farther into the discussion. It must also be conceded that inferences which have only probability, may be of much practical value in common life, and serve a certain purpose even in the proceedings of science. Bishop Butler has taught us that, to a great extent, probability is the guide of life. Junctures often arise when it is not only man's wisdom, but his clear duty, to act upon only probable anticipations of results. In science, also, these imperfect inductions have their use, which is this, to guide to some probable but only provisional hypothesis, which is taken only as a guide to experiments that are made for the conclusive investigation of nature. What we observe, then, of this induction by mere enumeration of agreeing instances is, that it is not useless; but it can never give demonstrated truths. But science requires, in its final results, complete demonstration.

Not a few logicians, among whom Hamilton is to be numbered, in view of this imperfection in the mere induction from the many to all, have roundly declared that induction can never give more than probable evidence of its laws. (Logic, Lecture 32d, end.) He asserts that it is impossible for it to teach, like the deductive syllogism, any necessary laws of thought or of nature! Must we concede this? Is the problem, the gravity of which was indicated, indeed hopeless? Must we admit that all the sciences of induction, and all the practical rules of life, which are virtually also inductive, are forever uncertain, presenting us only probabilities, and remaining but plausible hypotheses which await the probable or possible refutation from wider investigations? This we cannot believe. We claim a demonstrative force for this species of evidence, when it is properly constructed. We must substantiate such a view, or else candidly surrender the proud claim and name of *science* for our opinions upon all the natural phenomena. Real demonstration cannot be grounded in uncertainties, however much they be multiplied. They can only be grounded, as Mill has most truly declared—however inconsistently for his own logic—in necessary truths. Moreover, the common sense of mankind rejects the conclusion that all its inductions are only probable. Some of them we know to be certain, and experience never fails to confirm their certainty. The question, then, recurs, which is the great problem of this species of logic, How does the inference seemingly made from the some or the many to the all, become valid for the all?

R. L. Dabney.