III.—THE PLACE OF HYPOTHESIS IN EXPERIMENTAL SCIENCE.¹

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THE following pages are intended to establish (1) that hypothesis is the principal organon of discovery in experimental science ; (2) that hypothesis is based on analogy ; (3) that scientific hypotheses are not empirically verifiable. I shall then proceed to enunciate what I conceive to be the postulates of experimental science and the *rationale* of verification, and indicate briefly a view of the relation between physics and metaphysics.

That hypothesis is the principal organon of discovery in experimental science will doubtless seem a hard saying to not a few. The celebrated "*Hypotheses non fingo*" of Newton seems to act upon many thinkers like a charm ; they do not see that the emphasis is to be laid upon the *fingo*. They regard hypothesis as at best but a doubtful auxiliary to observation and experiment, and conceive that the main occupation of experimental science consists in establishing, by what are known as the experimental methods, the existence of invariable relations of coexistence, antecedence and sequence between phenomena. It is not necessary for me to dispute the possibility of establishing by observation and experiment the existence of relations which in a certain sense are invariable. This at present I am concerned neither to affirm nor to deny. I contend, however, that when established such relations do not amount to laws of nature.

In the first place it must be remarked that an uniformity of coexistence or of antecedence and sequence may mean either (1) an observed uniformity or (2) an hypothetical uniformity. So far, however, as laws of nature may consist of hypothetical uniformities, it is clear that the experimental methods alone are powerless to discover them. On the other hand, it can, I think, be demonstrated that no relations of coexistence, antecedence and sequence that are not hypothetical are in the strict sense laws of nature.

If by uniformities of relation we are to understand merely

¹ The substance of this paper was read before the Philosophical Society on 30th November, 1886.

observed uniformities, the impotence of the experimental methods to establish laws of nature becomes patent. Observation, eked out by memory and experiment, may establish an uniformity, but such uniformity only furnishes science with a *datum* on which to found an hypothesis. The experimental methods so elaborately analysed by Mill and Lotze are essentially methods of elimination. The same phenomenon is observed to be sequent, now upon one, now upon another, combination of antecedent phenomena. If then the several sets of antecedent phenomena have a common and only one common element, it is presumable that in the absence of the common element the sequent phenomenon would not occur; and if by observation or experiment we obtain a case in which, no new element being present in the antecedent, the common element is wanting and the sequent phenomenon no longer occurs, we infer—what? Simply that as often as the case is repeated without change in the conditions, the result will be the same. Into the question of the warrant for this inference I do not here enter. The reader is entitled to assume that the warrant is the uniformity of nature, or the principle of identity, or any other principle that he may prefer. All that I am concerned to maintain is that the relation thus established does not amount to a law of causation, even if causation be no more than unconditional invariability of antecedence and sequence¹ between phenomena. The experimental methods cannot establish the existence of any unconditional relations between phenomena. All that they can establish is the existence of relations, the invariability of which is contingent upon the conditions remaining constant. They establish, in fact, conditional invariabilities of relation. Nay more: science distinctly negatives the existence of unconditionally invariable relations between phenomena.

Geology has unrolled the records of past ages during which the forces of nature, though identical in kind with those now operating on the earth, were nevertheless so differently compounded as to produce widely different effects. Most of the sequences of events which were then observable on the planet must have been in striking contrast to any that are now observable; and whether we accept the nebular hypothesis or no, it is clear that the sequences of events now observable in the solar and sidereal systems are far from similar to those which might once have been observed, and

¹ I say sequence because I hold that consequence is more than the empirical theory of causation is entitled to.

that the existing distribution of matter and motion is undergoing a gradual transformation which, when complete, will present to future observers a set of relations of coexistence, antecedence and sequence totally different from any which we observe to-day. If causation were really unconditionally invariable antecedence and sequence between phenomena, none but periodical events could with any plausibility be said to be subject to causation, and of them only such as could be proved to be not subject to modification by counteracting causes ; nor even so would any date be assignable when *post hoc* might ripen into *propter hoc*. In fact, however, it cannot be maintained that any observable events there are of which the recurrence is unconditionally invariable. The 'composition' of causes stands in the way.

It is indeed impossible, consistently with the empirical theory of causation, to give a meaning to this expression 'composition of causes'. Two causes may neutralise one another, or be so compounded as to produce a different result from that which either of them operating singly would have produced. Were causation unconditional invariability of antecedence and sequence, this would be impossible. If unconditional invariability has any meaning, an unconditionally invariable relation of antecedence and sequence is one which is in no way modifiable. The composition of two unconditionally invariable events is a contradiction in terms. Nay, in strictness of speech, events are not capable of composition at all ; they can only be related in the way either of coexistence, or of antecedence and sequence. It may perhaps be said that the empirical theory of the composition of causes merely means that the simultaneous occurrence of several events is invariably followed by an event different from that which would have invariably supervened upon any one of them occurring singly. This, however, is to abandon the idea of unconditionality. An event which is only followed by another event provided some other event also occurs, is not an unconditionally invariable antecedent, and therefore, on the empirical theory of causation, no cause.

In short, if we define causation as unconditional invariability of antecedence and sequence between phenomena, we have no choice but to hold that only those antecedents upon which the same event always ensues and will ensue "so long as the present constitution of things endures," no matter what other antecedents are combined with them and "under all changes of circumstances," are causes ; but, as we do not know that any such antecedents there are, the consistency of

the theory would thus be saved at the expense of its significance.¹

If, however, the idea of unconditionality be abandoned and causation be defined as conditionally invariable antecedence and sequence between phenomena, then the difficulty presented by the so-called 'plurality of causes' has to be met. If an event may supervene upon any one of several independent antecedents, in what sense can any one of such antecedents be said to be the invariable antecedent of that effect? In the sense, it may perhaps be urged, that, given any one of them, the effect invariably follows. This, however, is to abandon uniformity of antecedence, reducing causation to mere uniformity of sequence; it is necessary to constitute even a conditionally invariable antecedent, not merely that the sequent should invariably follow upon it, but that in default of the antecedent happening the sequent should not happen. If there are several possible antecedents of a given event, no one of them can answer the description of even a conditionally invariable antecedent. Heat, *e.g.*, may be generated in a variety of ways. The Method of agreement enables us to say that, given one or other of certain antecedents, heat will always, in the absence of counteracting causes, be generated. The method of difference establishes that, in the absence of one or other of the said antecedents, heat will not be generated; but it does not entitle us to say that any one of these antecedents is even the conditionally invariable antecedent of the generation of heat; for, though the conditions remain otherwise strictly identical, heat may be generated either by friction, or percussion, or electricity. The method of difference is powerless to eliminate the plurality of causes; that can only be done by hypothesis—by assuming, *e.g.*, in the instance of heat, that at bottom the various modes in which it is generated are identical as being all modes of motion.

Moreover, even in cases into which the plurality of causes does not enter, it cannot without absurdity be maintained that causation is mere conditional invariability of antecedence and sequence. On such a theory high tide would have to be reckoned the cause of low tide, and *vice versa*. If then, discarding the idea that either unconditionality or antecedence

¹ "That which will be followed by a given consequent, when and only when some third circumstance also exists, is not the cause, even though no case should ever have occurred in which the phenomenon took place without it" (Mill's *Logic*, 8th ed., bk. iii., c. 5, § 6). It is safe to say that no observable event is ever followed by another except conditionally upon some third circumstance being given.

the necessary elements in causation, we define cause as the totality of the conditions which given a phenomenon invariably occurs, which not given it as invariably does not occur, we have to ask what we mean by 'totality of conditions'.

The phrase may mean either the totality of the phenomenal or sensible conditions of a phenomenon, or the totality of its conditions, insensible as well as sensible. Only in the former sense can the totality of the conditions of a phenomenon be ascertained by observation and experiment; but in this limited sense the conditions of a phenomenon are merely so many particular facts, and the fullest account which could be given of them would amount to no more than a description of the phenomenon, its concomitants and antecedents. On the other hand, the causes with which science is concerned are, as I shall show in the sequel, always insensible, the laws which it formulates always hypotheticalal. If, however, the expression 'totality of conditions' is to include the insensible as well as the sensible conditions of phenomena, it is too wide for practical utility, since the absolute totality of the conditions which determine a phenomenon is only knowable by omniscience. Whatever scientific men mean by cause, they mean neither the empirically uniform antecedents and concomitants of a phenomenon nor the totality of its conditions: they mean more than the one and less than the other.

What then, the reader may ask with some impatience, do you say that they mean? I answer: the distinction between cause and effect is simply one mode of the distinction between the real and the apparent. Physics starts with the postulate that the observed order is not the real order. It regards sensible appearances and their empirically uniform interconnexions as merely indices of a real order, which can only be apprehended by means of the exercise of a faculty which may with indifference be called reason or scientific imagination. Such a conception is implied in the mere use of the terms object and objective, and, as object and subject are correlative, is based upon the rock of self-consciousness. What the object or substance is to a group of sensible perceptions, that the cause is to an event or sequence of events. Physical science is an attempt to give precision to that conception of the world, as other than that which it appears, which is implicit in ordinary experience, and for this purpose its chief instrument is hypothesis. Observation and experiment are indeed indispensable, but only for the purpose of furnishing hypothesis with correct data and testing the adequacy, or, as Lewes well said, the "effectiveness," of a

given hypothesis. Physics assumes what empirical philosophy, if it does not deny, at least will not assert, *viz.*, the existence of a world of noumenal causes and "real essences"; and it is with the attempt to define the nature of these noumenal causes and real essences that induction in the strict sense begins. It is probably because they come to the subject with minds clouded by sensational metaphysics that logicians of the empirical school have overlooked this fact. They have described rather the method of their own philosophy than that of physical science, which is not so much empirical as theoretic.

Had the aim of the early physicists been merely to observe and tabulate uniformities of relation, it is not too much to say that neither astronomy nor physics would have come into being. By the method of observing and registering relations of coexistence, antecedence and sequence alone, we could never have come by the knowledge even of the sun, moon and stars. Had causation meant for the early thinkers invariability of antecedence and sequence (and how could they have distinguished between conditional and unconditional invariability?), they would have noted the fact that the emergence of a luminous disc from below the horizon was the uniform antecedent of day and its disappearance the uniform antecedent of night, and there the matter must have ended. The hypothesis that both phenomena were occasioned by the motion of a vast body so distant from the earth as to present to them the appearance of a mere disc of light could only have been framed by men who believed in a world transcending experience and sought to define it. Pythagoras in substituting the idea of the sun as the centre of the planetary system for that of the earth, the Alexandrian astronomers in reconciling the apparent motions of the sun and planets with the immobility of the earth by their elaborate theory of epicycles and eccentrics, Copernicus in showing that the Pythagorean theory, while simpler than the Ptolemaic, explained at once the apparent motions of the sun and planets and the apparent immobility of the earth,—all employed a method which was certainly inductive, but at the same time boldly set at nought the testimony of the senses and transcended experience. The motions of the planets are no more phenomena than are the ultimate atoms of which their substance is supposed to be constituted; they are not empirically verifiable, but we know that if they take place they must present to us the appearances which we see, and we assume their existence because we are unable to deduce the phenomena from any

hypothesis which introduces a larger measure of unity into our conception of the objective conditions of the said phenomena. Similarly, physics originated in the attempt to define the essential nature of material substance. Thales saw all things in water, Anaximenes in air, Heraclitus in fire, Anaxagoras supposed matter to be constituted of minute invisible particles, but apparently failed to conceive of these particles as essentially homogeneous. An immense stride was taken in this direction when Democritus attempted to explain the secondary qualities of matter as resulting from differences in the arrangement of the ultimate particles. In modern times all the secondary qualities except flavour and odour have been resolved into modes of molecular motion. Had not the thinkers to whom we owe the molecular theory believed in a "real essence" underlying material phenomena, and in the possibility of defining that "real essence"—had they been, in a word, imbued with the principles of modern empirical philosophy, and content to abide in the outward shows of things, noting and registering such relations as were uniform—physics had never been.

The causes of events in the sense explained may or may not be conceived as events or sequences of events, but the relation between cause and effect is always one of coexistence; nor does science rest content with a cause which is merely an event or a sequence of events.

The first hypothesis concerning the cause of an event usually consists in treating it as the index of some other but insensible event suggested by analogy. Logicians of the empirical school regard analogy as distinct from induction, and treat it with scant respect; but, as Lotze has pointed out (*Logic*, § 274), hypothesis ought to rest upon analogy.

Young is said by a flash of genius to have seen, in the darkness which resulted from the fusion of two beams of light in Grimaldi's experiment, the analogue of the stillness produced by the neutralisation of one wave by another. This was the germ of the undulatory theory of light. In the case of the molecular theory of sound the analogy was more obvious. There the *experimentum crucis* was the ringing of a bell in a receiver nearly empty of air, the sound being scarcely audible. It was then clear that in so far as the vibrations of the resonant surface failed of affecting the ear, it was for want of propagation by the air, and the analogy of the ripple propagated in a gradually widening circle round an object dropped into water came to hand at once. If the latter was explicable as an agitation of particles, each of which after communicating its motion to its neighbour re-

turned to rest, so also might conceivably the propagation of sound be explained. It also lay on the surface that pitch might vary as the velocity of the undulations ; and, the fact (proved by Newton) that the rate of propagation is constant for all degrees of loudness according with the fact that the rate of propagation of visible waves is constant for all degrees of amplitude or range of swing, it was inferred that loudness varies with the amplitude of the sound-wave. These essential points settled, it only remained to adapt the theory to the exigencies of special cases. Even Helmholtz's analysis of the composition of musical notes is of this kind. From the familiar fact that harmony is pleasant he inferred that a musical note was probably itself a harmony, and the results obtained by magnifying sound by mechanical appliances accorded with his hypothesis.

In like manner the theory that the celestial luminaries are solid bodies similar to the earth can only have been reached by a process of analogical reasoning. Anaxagoras, *e.g.*, was clearly reasoning by analogy when he broached the theory that the sun and stars were masses of stone ignited by the force of rotation. Again, the molecular theory involves the hypothetical endowment of that which to sense is perfectly simple with a complex structure analogous to that with which we are familiar in composite bodies. It also is a mere analogy, unverifiable empirically but accepted because it enables us to assimilate the infra-sensible to the sensible world.

Nay, the very conception of an object is an analogical transference of the unity and identity which we know as self to that which in itself is a mere cluster of perceptions, and the idea of force, as Mr. Spencer is fond of telling us, is derived by analogy from the experience of volition.

Now if hypothesis is, as I hold, the most important part of the inductive process, it follows that the principle of induction is aptly expressed in the much-criticised Newtonian canon : "Effectuum naturalium ejusdem generis eadem sunt causæ".¹ On the strength of a similarity in the phenomena, which may be obvious or may be recondite, a similarity in the causes which determine them is inferred, and an attempt is then made to deduce the phenomenon from the hypothesis, which is accepted or rejected according as it does or does not admit of such deduction. Hence a subtle power of detecting recondite resemblances, boldness in assuming essential identity of conditions on the strength thereof, and

¹ Cp. Lotze's *Logic*, p. 317.

patience in making the observations and ingenuity in contriving the experiments necessary to test the theory, are alike indispensable to scientific progress. I have said that science does not rest content with explaining phenomena in terms of events. This is especially true of modern science, which is chiefly distinguished from ancient science by the prominence given to the idea of force.

Phenomena having been explained in terms of insensible motion, molar and molecular, these latter are in their turn explained in terms of force. Dynamism would be more appropriate than Materialism as a designation of the modern scientific movement, the idea of inertia having given place to that of an equilibrium of forces.

Of force the empirical logic knows not what to make: it is difficult to extract from its exponents any consistent doctrine on the subject. As, however, Mill illustrates his chapter on the composition of causes by examples of the composition of forces, it is safe to assume that he at least regarded force as synonymous with cause, and therefore reducible to uniformity of antecedence. This view, however, is radically unscientific. Science does not regard force as antecedent to its results at all: it regards them as strictly synchronous. Gravitation, cohesion, chemical affinity, do not antecede the various phenomena which they condition, but are exhibited in them. The force of which Mr. Spencer writes with such impressiveness is neither an event nor any number of events, but the condition of all events happening. The conception is indispensable to science, which, as I have said, cannot rest in mere events; but it is not empirically verifiable. Even if we take the step which Mr. Spencer declines to take, and identify physical forces with that force which we know immediately as exerted by ourselves in volition, still the projection of such force into the universe at large remains a mere analogy wholly "unsusceptible of being ultimately brought to the test of actual induction".

If the foregoing account of causation and scientific method is true, a law of causation will be definable as an hypothesis by which several events or sequences of events are deduced from one and the same cause or combination of causes. As such it will be a relation not of sequence, but of coexistence and community. There will thus be no radical distinction between cause and law; rather every cause so soon as defined will be itself a law, provided only the phenomena are deducible from it. A law, in fact, is simply the definition of a given cause. This point is so clear that illustration

may seem superfluous. I need therefore only remind the reader that the essence of the law of gravitation consists in its connecting facts so disparate as the fall of a stone and the persistence of the planets in their orbits as effects of one and the same cause; that the molecular theory of heat identifies the cause of the sensation, no matter what the sensible antecedent may have been, with an agitation of the insensible particles of matter; that Lavoisier's theory of combustion traces processes so apparently diverse as calcination, combustion and acidification to the operation of the same force, *viz.*, oxygen.

To sum up the matter: physical science presupposes the determination of phenomena by objective conditions or causes; observation establishes empirical rules to the effect that certain phenomena have hitherto, so far as experience has extended, coexisted with or ensued upon certain other phenomena; experiment purifies these rules from all unessential elements by the process of elimination analysed by Mill; the result is the formulation of conditional uniformities of coexistence, antecedence and sequence; then begins the process of induction in the strict sense, which consists in framing an hypothesis based on analogy concerning the nature of the causes which determine the uniformity in question; the hypothesis, when framed, is tested by attempting to deduce the phenomena from it with the help, if necessary, of experiments to test the adaptability of the theory to special cases. This last process is sometimes called verification, and no doubt in a certain relative sense it is so. Verification, in the sense of strict proof, it certainly is not, since even if the hypothesis stands the test its absolute truth is not thereby established.

The principal use of the process commonly known as verification is not to verify, but to disprove. It is a criterion rather of error than of truth. If we were to adopt the view that mere deduction of the phenomena from the hypothesis, together with the fact that the theory has been so articulated as to meet all special cases as yet given by nature or invented by experiment and to serve as an instrument of prediction, amounted to absolute proof, we should land ourselves in the absurd position that what is proved to-day may be disproved to-morrow, since hypotheses which explain certain facts perfectly well are sometimes superseded by others which explain them not a whit better, but happen to have a wider range of applicability; it is very difficult, in any given instance, to say that the resources of analogy have been exhausted. That some limit to the powers of the human

mind in explaining phenomena there must be, is clear ; the difficulty is in proving that it has been reached. Nor, if we suppose that in certain fields it has been reached, does that fact warrant us in assuming the absolute truth of the hypothesis in question. It may not be possible for us to attain absolute truth by means of hypotheses.

Mill distinguished between hypotheses which rest on mere analogy and such as are capable of " being ultimately brought to the test of actual induction," claiming for the latter a verifiability which he denied to the former (*Logic*, 8th ed., vol. ii., pp. 15, 94). And on this ground he refused the title of " positive truths " to both the emission and the undulatory theory of light. In fact, however, neither of these theories stands in a worse position than the commonplace hypothesis of an atmosphere. The atmosphere is just as little capable of being brought to the test of actual induction as is the luminiferous ether. Its existence is assumed upon the analogy of watery fluid for the purpose of accounting for certain phenomena, just as is the existence of the ether. We are so familiar with the hypotheses of an atmospheric medium that we have ceased to regard it as an hypothesis, and it requires a certain effort of thought to realise that it is so ; yet nothing can be more certain than that if we had no experience of visible fluid, however we might explain the motions of the clouds and the pressure of the air upon us, it could not be by means of an atmosphere. We should have no experience capable of yielding the notion. It is clear therefore that the atmosphere, as distinct from the perceptions for which it accounts, is not, strictly speaking, a " real phenomenon " ; is not, in fact, a sensible, but a mere hypothesis. It is possible that at some future time the idea of a luminiferous ether may be so familiar that its hypothetical character may be forgotten. In any case it is impossible to draw any valid distinction between the two hypotheses, except so far as the hypothesis of an ethereal medium may not have been so thoroughly tested as the hypothesis of an atmospheric medium has been. The latter hypothesis is, however, accepted for no other reason than that it explains certain facts, and enables us to interpret them as the result of undulations of the said medium ; and until men shall have acquired the porcine faculty of seeing the wind it will remain entirely " unsusceptible of being brought to the test of actual induction ".

It follows that the theory of sound, which is based upon the hypothesis of an aerial medium, is not empirically verifiable. Nay the entire molecular theory is incapable of em-

pirical verification, and so is the Copernican theory : they explain phenomena, but not by means of causes which are themselves phenomena.

If then, it will be asked, laws of causation are hypotheses, and as such liable to supersession, what becomes of the immutability which is commonly supposed to be essential to a law of nature? The answer seems to be that the idea which underlies our ascription of immutability to laws of nature does not stand or fall with the truth or otherwise of laws of causation, but is really identical with the uniformity of nature. The uniformity of nature involves five principles, which are : (1) that every event is the index of a cause or combination of causes ; (2) that no event is the index of more than one cause or combination of causes ; (3) that, given the proper cause or combination of causes, in the absence of counteracting causes, the effect always occurs ; (4) that the sum total of ultimate causes is a fixed quantity ; (5) that the causes existing in the universe are so related, *inter se*, that in virtue of their mutual modification change, though incessant, is on the whole gradual.

The first of these principles we have already discussed at length ; the second is clearly required by the uniformity of nature. If the same effect might indicate any one of several alternative causes, there would manifestly be a breach of uniformity. In the case *e.g.*, of heat, where if anywhere there might seem to be a plurality of causes (as friction, percussion, electrical and chemical action), the uniformity of nature requires and science has established that there is but one cause, *viz.*, molecular motion.¹

The third principle, *viz.*, that, given the cause and no counteracting causes, the effect always happens, is equally necessary to the uniformity of nature. If it were not true physics would be impossible. The fourth principle, that the

¹ The application of the molecular theory to heat is one of the most interesting examples of the true inductive method. By the liquefaction of two pieces of ice by mutual friction Davy proved that the accepted theory, which identified the objective correlative of the sensation with a subtle fluid (caloric), permeating bodies and forced out of them by friction, could not possibly be true, since a body which had parted with a certain quantity of caloric by friction, as according to the theory the melted ice had done, must require the application of more caloric to raise it to a certain degree of temperature than it required before, and this was not true of the water into which the ice had been converted by the friction, its temperature being higher than that of the ice. And as the effect of friction was already conceived to be an agitation of the molecules constituting the rubbed body, Davy inferred that such an agitation was the objective correlative of the sensation of heat.

sum total of ultimate causes is a fixed quantity, follows from the first; for the coming into being of a new ultimate cause, or the passing out of existence of an old one, would be an uncaused event. The fifth principle, that of the reciprocal action of causes, is essential to the idea of an universe. If any cause or set of causes was unrelated to the rest, there would be, strictly speaking, no universe. If, *e.g.*, the forces which make for rest and those which make for motion were unrelated, there would be two worlds but no universe—a world in which everything was at rest and a world in which change was perpetual and universal, but no transition from motion to rest and rest to motion. There would thus be no universe, no organic unity of things. It is clear therefore that the existence of the universe implies that the statical and dynamical forces do interact. It is further implied that such forces are equal in amount. If, *e.g.*, there were a preponderance of the statical over the dynamical forces, the universe must eventually pass into a state of complete quiescence. This would, however, involve the impossible supposition of a last event. It follows that the statical and dynamical forces must be regarded as equivalent. If, however, they were also equally distributed, it is plain that no change could ever occur. Hence we must regard them as equivalent indeed but unequally distributed. It is further clear that, if this inequality of distribution were very great, the universe would present the aspect of a series of cataclysmic changes, instead of the incessant but gradual change which we know and which is implied in the uniformity of nature. Nor is it possible to conceive that the universe will ever enter upon a period of general and continuous cataclysmic change. The violence of change is proportionate to the resistance offered; hence every cataclysm presupposes a period during which the statical forces have been largely predominant, during which the dynamical forces have slowly accumulated. No rapid and general succession of cataclysmic changes is therefore possible.

These principles constitute, in my opinion, what we mean by the uniformity of nature and the immutability of law. They are indispensable to physics, but they are not empirically verifiable; the only verification of which they are susceptible is just their indispensability. They are principles of the possibility of physical science. Laws of causation, on the other hand, are figurative schemata whereby the essential unity of the universe is bodied forth to the eye of imagination. As such they are not devoid of truth, and indeed of positive truth. An idealist who holds that matter and force

have no existence apart from consciousness is not bound to deny the positive truth of any scientific theory which furnishes the best available explanation of the phenomena it purports to explain. In fact, it is just positive truth which he will allow to such an hypothesis ; *i.e.*, he regards it as a necessary moment in the process by which the human mind comes by the knowledge of the essential unity of the cosmos. Its verification consists in this necessity. Such verification may be called transcendental as distinguished from empirical verification. At the same time he will certainly deny the absolute truth of all such hypotheses. The scientific Pegasus is a noble animal, and his rider extremely bold ; but “ *post equitem sedet atra cura* ” ; Nemesis rides on the croup in the shape of the metaphysician, and will not be shaken off no matter how rough the pace may be. Metaphysics, in fact, is related to physics, as physics is related to experience. The ideal world of physics satisfies the metaphysician as little as the world of sense-perception satisfies the physicist. His procedure is of the simplest kind, and consists in merely pointing out that an idea is an idea and therefore relative to consciousness, and that by consequence the entire body of scientific hypothesis has only a relative validity. Those who think otherwise he shows to be still in the bondage of Scholasticism—to be in fact no better than those mediæval thinkers who mistook the connotation of a common term for a real essence residing in things. In short, he pushes the principle of conceptualism to its logical issue. By degrees a dim perception of his meaning dawns upon the scientific mind, and therewith the half-suspicion that he may be right. Accordingly an attempt is made to meet him half-way. Hence arise two schools of scientific thinkers. Both agree in admitting the relativity of scientific theory ; but, while one denies the power of the human mind to reach absolute truth, the other joins hands with those who are only just emerging from the realism of common sense in order to find the absolute in that of which nothing in particular can be said. The first school virtually admits that science is an illusion, and erects its own speculative incapacity into a standard of human faculty. The second differs but by a hair's-breadth from the first, and that difference is not on the side of logic. This poor *caput mortuum*, this absolute, this unconditioned, which it is sought to purge of all subjective elements, after all, it must be admitted, exists, and on this admission the metaphysician pounces with avidity. ‘ You tell me,’ he says, ‘ that the absolute and the relative are wholly heterogeneous, yet you predicate of both this same attribute, exist-

ence. It is idle to assert that existence as absolute is totally different from existence as relative; for that is merely to admit that your system is based upon an equivocation. Existence, like every other term, denotes an idea, and as such its import is relative to consciousness. You will not assert that the existence of a cognition is anything more than its being known. To assert then that that which is neither known nor knowable exists is simply to contradict yourself. You claim, in fact, to transcend consciousness, and though experience may, consciousness cannot, be transcended. If we could know that the unknowable exists, we should as surely transcend consciousness as if we knew exactly how it exists. You have no logical alternative but either to deny existence to consciousness or to admit that the unknowable does not exist. As matters stand, you are cheating yourself with an abstraction.'

In truth it is not the metaphysician who seeks to transcend consciousness, but the scientific or quasi-scientific thinkers, who cannot see that a term is none the less relative that it is abstract, who surrender to the idealist the concrete world of perception and imagination only to mistake for things in themselves those ideas which of all others are the most attenuated, the nearest to insignificance.

The metaphysician, however, in holding by the doctrine that existence is limited by consciousness, does not mean to deny the existence of the objective universe which science postulates. It is his proper problem to reconcile physics and idealism—a problem which I must content myself on this occasion with thus barely indicating.