VI.—CRITICAL NOTICES.

Logic, Part II. (Demonstrative Inference). By W. E. JOHNSON. Cambridge University Press, 1922. Pp. xx, 258.

THE second volume of Mr. Johnson's great work on *Logic* deals with demonstrative inference, deductive and inductive. It is perhaps even more interesting than the first volume, on account of the extreme practical importance of its main subject, and also on account of the digressions on such matters as Magnitude and Symbolism. It covers the whole range of mathematical reasoning, and it also deals with those types of argument which Mill tried, not too successfully, to classify in his Inductive Methods. Incidentally it contains almost the only good criticism that has yet appeared on a number of fundamental, but rather technical, points in Russell's *Principles of Mathematics*.

The work opens with an Introduction, which clears up certain points in vol. i., and restates Mill's criticisms on the syllogism in terms of the distinction between Epistemic and Constitutive Conditions, which was drawn in the first part.

Chapter i. discusses the general nature of inference, and its connexion with implication. Mr. Johnson says that implication is "potential inference," and holds that, although implication and inference are distinct, neither of them can be understood except in terms of the other. He thinks that it follows from this that we ought rather to say "*p would* imply *q*" than "*p implies q*," and he actually adopts this mode of statement throughout the book. It seems to me that this is not true, and that it does not follow from the identification of implication with potential inference. If implication is potential inference, we ought no doubt to say "*p would* justify you in inferring *q* (if you knew that *p* was true)," whenever *p* does imply *q*. But I cannot see why this should make us introduce the potentiality into the statement about implication, and say that *p would* imply *q* rather than that *p does* imply *q*. To take an analogy. We might say that "threatening" is "potential injuring". But this does not mean that we ought to confine ourselves to statements like "A would threaten B". On the contrary we say that "A does threaten B" whenever it is true that "A would injure B (if he could)". This criticism is not merely verbal, as may be seen from the following examples. I should say: (1) M*a*P and S*a*M *do* imply S*a*P; (2) M*a*P *would* imply S*a*P (if the premise S*a*M were added); and (3) M*a*P *would not* imply S*e*P under any circumstances (because there would be illicit process of P). Now I do not see how (1) and (2), which are clearly different, could be distinguished in Mr. Johnson's terminology, since he would have to put (1) in the form "MaP and SaM *would* imply SaP". The phrase "would imply" seems only to be appropriate to cases like (2); and, if it be used for cases like (1), we are left with no appropriate expression for the former.

It is implied in ch. i. and definitely asserted in ch. ii, p. 30, that "there is no single relation properly called *the* relation of implication". What Mr. Johnson means is that to say that p implies q is simply to say that q could be inferred from p, and that this is true when and only when one or other of several specific types of formal relation hold between p and q. In ch. i., § 4, Mr. Johnson says that there are two fundamental relations between p and q, which justify inference from the former to the latter. These relations are formulated by him in two Principles of Inference, the *Applicative* and the *Implicative*. The Applicative Principle states that, if p be of the form *All S is P*, and q be of the form *The given S is P*, then q can be inferred from p. The Implicative Principle states that if p be a compound proposition of the form (x)-*and*-(x *implies* y) whilst q is of the form y, then q can be inferred from p. All deductive inference rests on these two principles; and, therefore, I take it, all implication depends on one or other of the two types of relation mentioned in these principles.

There is no particular difficulty about the Applicative Principle, but there is a question to be raised about the Implicative Principle. This professes to state one of the types of relation which must hold between p and q if p is to imply q. But it presupposes that p itself already contains two propositions x and y, of which the former implies the latter. The question at once arises: Of what nature is the relation between x and y, in virtue of which x implies y? If there be just two relations—the Applicative and the Implicative—which give rise to implication, it would seem that the implication which is involved in the very statement of the Implicative Principle must itself rest on either the Applicative or the Implicative relation. If this be so, I do not see how the Implicative Principle can be taken as expressing one of the two fundamental types of relation on which implication depends. It would seem that the implication which is involved in the Implicative Principle must at last rest on the Applicative Relation, on pain of an infinite regress. If so, the Applicative Principle is more fundamental than the Implicative Principle.

In ch. ii., § 3, p. 30, Mr. Johnson attempts a more accurate statement of the Implicative Principle, but it does not meet the difficulty that I have just pointed out. He there reformulates the principle as follows: "There are certain specifiable relations such that, when one or other of these subsists between two propositions, we may validly infer the one from the other". It is, I think, perfectly clear that this is *not* a reformulation of the Implicative

Principle, as originally stated on p. 11, and that it is not consistent with the statement on p. 10, that the Implicative Principle expresses one of the "two fundamental relations which will render the inference from p to q . . . formally valid." To say that *there are some* relations which will render the inference valid is not to state *one of the two* fundamental relations which justify inference. In fact it is obvious that the Implicative Principle in its second formulation would be true if implication rested on no other relation beside that mentioned by the Applicative Principle.

There is thus certainly some inconsistency in Mr. Johnson's language, and I am a good deal puzzled both as to what he really means and as to what is the truth of the matter. I would tentatively offer the following suggestions. (1) The Applicative Principle really does state one fundamental type of relation between two propositions, such that, whenever it holds, the former implies the latter. (2) If you grant the Applicative Principle, the Implicative Principle, in Mr. Johnson's second formulation, immediately follows. This is sufficient to show that the second formulation does not adequately express what Mr. Johnson means by the Implicative Principle; for he certainly understands by it something which is parallel to and independent of the Applicative Principle. (3) The Implicative Principle, as originally formulated, does express one of the relations on which implication rests. But it only applies to the special case where one of the propositions is a complex, one of whose parts itself involves an implication. And this implication must in the end presumably rest on some other type of formal relation than that which is formulated in the Implicative Principle. *If* it were true that there are only two fundamental types of relation which generate implications, it would seem that the Applicative relation must be more fundamental than the Implicative, in the sense that the implication which is involved in the premise to which the Implicative Principle is applicable must ultimately rest on the Applicative relation. But I do not know why there should not be *many* different formal relations which give rise to implications. And, although Mr. Johnson seems to hold on p. 10 that there are only two, he seems to make no such restriction on p. 30. (4) In fact I take it that there are many independent formal relations which generate implications. *E.g.*, if p has the form P, and q has the form (P or Q) they are so related that p implies q. And this depends neither on the Applicative nor on the Implicative Principle. What then is the *special* importance of the Applicative and Implicative Principles? (5) It seems to me that their great importance is as *generative* principles. It is by them, and by them alone, that we can deduce chains of new truths from a few suitable primitive propositions. The primitive propositions state certain independent and immediately obvious formal implications, like p implies (p or q). These give us premises of the kind to which the Implicative Principle can be applied. Again the Applicative Principle allows us to substitute what Mr.

Johnson calls "connected complexes" for the simple terms in the primitive propositions, and thus to reach new truths by what he calls "functional deduction". If we read a work like *Principia Mathematica*, we see that, once the primitive propositions have been laid down, all further progress is made by repeated use of these two principles. Mr. Johnson is therefore justified in the importance which he ascribes to them, though, as I have said, some of the statements which he makes about them seem to me puzzling, and at least verbally inconsistent.

Mr. Johnson next makes some very interesting observations on the applicational and the implicational forms of inference. As an example of a purely applicational argument he takes such an example as: "All propositions have predicates, therefore *Matter exists* has a predicate". Now the question arises: Is there not a suppressed premise, *viz.*, "*Matter exists* is a proposition"; and is not the argument therefore a syllogism, which Mr. Johnson regards as involving both the applicative and the implicative principles? To this he answers that the supposed premise is really, from the very nature of the case, superfluous. We cannot attach any meaning to the phrase *Matter exists* unless we know that it is a proposition, and it is therefore superfluous to state that it is a proposition. Mr. Johnson makes two statements about such propositions which are verbally inconsistent. On p. 14 he says that they are not genuine propositions. At the foot of the same page he says that they are propositions of a peculiar kind, which he proposes to call *structural*. A structural proposition is not simply verbal, for it is not about words. What it does is to assert a general category of the subject. But it does not add to our knowledge, because the subject has to be given to us under this general category before we can specify it at all in a judgment. A category is a determinable or set of determinables, and all judgment consists of specifying the determinate forms in which a subject exhibits those determinables under which it must be given to us if we are to be able to think of it at all. When a superfluous premise is added to convert a purely applicative argument into a syllogism, Mr. Johnson calls the premise a *sub-minor*.

We can now understand Mr. Johnson's analysis of the ordinary subsumptive syllogism. Take the syllogism: "All equilateral triangles are equiangular, the triangle ABC is equilateral, therefore it is equiangular". Mr. Johnson would analyse this somewhat as follows:—

Everything with sides and angles (M, P) is equiangular (*p*) if equilateral (*m*).

Therefore the triangle ABC is equiangular (*p*) if equilateral (*m*). (Applicative Principle.)

The triangle ABC is equilateral (*m*).

Therefore the triangle ABC is equiangular. (Implicative Principle.)

Here M and P are the determinables under which the object in question is given. The major states an universal connexion between one determinate under M and one determinate under P. No premise of the form "The triangle ABC has sides and angles" is needed, for such a proposition is merely structural.

Mr. Johnson points out that it is possible to make a really implicative argument look applicative by introducing a superfluous major, just as it is possible to make a really applicative argument look implicative by introducing a superfluous minor. This would happen if you were to take the formal Barbara (MaP . SaM implies SaP) as a *premiss* in some particular argument in Barbara. There is a positive inconsistency in doing this, for the principle of the syllogism in Barbara states that the premises of Barbara are by themselves *sufficient* to justify the conclusion, and you stultify this if you introduce the principle itself as a *further* premise.

The remaining point to notice in this chapter is Mr. Johnson's clear distinction between the constitutive and the epistemic conditions of valid inference. The constitutive condition is that the premises shall *be* true and shall *imply* the conclusion. The epistemic condition is that you shall be able to *know* that the premises are true and that they imply the conclusion without having to *know* beforehand that the conclusion is true. It is clear that in a great many cases, *e.g.*, where the major is proved by induction, or is self-evident, or is accepted on authority, and where the formal connexion between it and the conclusion can be intuited, these conditions are fulfilled.

We may take chapters ii. and viii. together, for they introduce us to the unusually extended sense in which Mr. Johnson uses the term *induction*. The Applicative and Implicative Principles assume that we have already got a number of universal premises to work with. How do we get these? Always by something of the nature of induction, according to Mr. Johnson. Now this might at first make the reader think that Mr. Johnson is an empiricist; but this is far from being so. We do not start by seeing axioms in their generality, we get to know them by reflecting on particular instances. The process by which this happens is called *Intuitive Induction*. Mr. Johnson defines Induction in ch. viii., as a process by which we start from certain instantial premises and reach a conclusion which is a generalisation *of these premises*. (It would not be enough to say that the conclusion is wider than the least wide of the premises, for, as we shall see, Mr. Johnson holds that many purely deductive arguments have this characteristic.)

Now I think that this definition of induction would generally be accepted. And it is certain that the process of seeing an axiom by reflecting on particular instances of it answers to this definition, if it be a process of inference at all. Hence Mr. Johnson is quite consistent in saying that all principles and major premises are ultimately reached by some kind of induction. And it does not make him an empiricist, for an empiricist would hold that they are all reached by that particular kind of induction which Mr. Johnson calls *Problematic*. Problematic induction leads only to probable conclusions, needs special axioms or postulates, and is left to be treated in the next volume. But there are three processes of inference which answer to the definition of induction, lead to conclusions which are as certain as their premises, and are treated in the present volume. These are Intuitive, Summary, and Demonstrative Induction; and it is the first of these which establishes the fundamental principles of inference themselves, and the self-evident axioms which form the major premises of pure logic, mathematics, etc.

Mr. Johnson distinguishes two principles of Intuitive Induction which he calls the *Counter-applicative* and the *Counter-implicative* Principles. The first may be stated as follows: "Sometimes we can see that what is true of this instance is true of any other instance, and then we can be sure that it is true of all instances". The second can be stated as follows: "Sometimes when we have made a particular inference which is valid we can see that its validity is due to a certain type of formal relation which holds between premise and conclusion". I can then conclude by the Counter-Applicative Principle that any argument of this form will be valid. These principles cannot be formulated so that we can safely use them blindly, as we often can the direct Applicative and Implicative Principles. Insight into the special subject matter which forms our instances is necessary.

In ch. ii. we are given a very useful division of propositions into a hierarchy, which I will now exemplify. We have (1) Supreme principles of inference, such as the Applicative, Counter-applicative, etc. (2) (*a*) Formal axioms, such as *p implies q-or-p*. (*b*) Formal propositions deduced from these axioms by the deductive principles in (1), *e.g., if q implies r then p-implies-q implies p-implies-r*. (3) (*a*) Particular instances of (2*a*), from which (2*a*) are derived by principles of intuitive induction contained in (1), *e.g., Jones is a knave implies* (*Brown is a fool*)-*or*-(*Jones is a knave*). (3) (*b*) Particular instances of (2*b*), *e.g.*, the particular syllogism in Barbara to prove that George V is mortal. (3*b*)-propositions follow from the corresponding (3*a*)-propositions by the Applicative Principle. The dividing line between (2*a*) and (2*b*) is not of course perfectly sharp, since different propositions are taken as axioms in different systems. (4) (*a*) Experientially certified propositions, like *This patch is red*. (4) (*b*) Deductions from these made in accordance with the axioms and principles of the higher levels. The distinction between the two sub-groups here is again not sharp, because no two people are agreed as to precisely what is certified by mere sense-experience and what is inferred from it.

Chapter iii. deals with Symbolism and Functions, and is far the best account that I know of these subjects. It contains a severe criticism on the inconsistencies of Mr. Russell's account of propositional functions. Mr. Johnson begins by dividing symbols into *shorthand* and *illustrative*. The former are simply abbreviations for words like *and*, *or*, *implies*, etc. They stand for formal or logical entities and may be called *formal constants*. This means that they have precisely the same significance wherever they occur, and that this significance is part of the subject matter of pure logic. The word *white*, or any shorthand symbol that we might use for it, is a *material constant*. That is, it is the name of a certain definite entity which does not belong to the subject matter of pure logic. Certain shorthand symbols might, for all we know at the outset, be either material or formal. The figure 2 would be an example. We might reasonably think that it was a material constant, like *white*, but it turns out to be formal if we accept Russell's and Whitehead's proof that arithmetic contains no fundamental concepts which do not belong to pure logic.

Illustrative symbols are the P's and Q's, x's and y's, of formal logic and algebra. Mr. Johnson calls such symbols *variables*. It will be noticed that he confines the names *constant* and *variable* to words and symbols, and does not apply them to what these denote. According to him, illustrative symbols are singular names of a peculiar kind. Their peculiarity is that they "stand for" any one of a whole set of ordinary singular names. Thus in "x is mortal" the symbol x stands indifferently for the names "Socrates," "Plato," "The Man in the Iron Mask," and all other names (say) of persons. There seems to me to be a verbal inconsistency in Mr. Johnson's statements on this point. After saying that s in "s is p" stands for any substantive-*name*, he goes on to say (p. 60) that "p stands for any one indifferently assignable adjective comprised (say) in the class *colour*". It is clear that he here means *adjective* and not *adjective-name*; for the adjective-name "red" is not comprised in the class *colour*, whilst the adjective red, which it denotes, is. Now it is clearly inconsistent to make the variable s stand for substantive-*names* and not substantives, whilst you make the variable p stand for *adjectives* and not adjective-names. I think the verbal confusion arises through the ambiguity of "standing-for," which sometimes means "acting as representative for" and sometimes means "denoting". S stands for the names "Socrates," etc., in the sense that it equally represents any one of them. P stands for the colours red, etc., in so far as it *represents* equally any one of a set of names each of which *denotes* a certain colour.

Variables are closely connected with functions, and functions according to Mr. Johnson are bound up with constructs. A function is the identity of form which can pervade many constructs constructed out of different terms. Thus p-or-q and r-or-s are two constructs out of r and s, p and q, respectively. And both exemplify the alternative function. The terms in a construct, for which substitutions may be made without changing the nature of the construct, are called *variants* by Mr. Johnson; and the illustrative symbols for variants are of course variables.

This definition of *function* is consistent with the sense in which it is used in mathematics. Mr. Johnson has no difficulty in showing that Russell's various uses of the term *propositional function* are consistent neither with each other nor with the common usage. The whole of what Mr. Johnson says on this subject is well worth reading, and seems to me perfectly conclusive.

One other very interesting point in this chapter is Mr. Johnson's view that verbal phrases like *Smith-and-Brown* or *Smith-or-Brown* do not denote genuine logical constructs, whilst phrases like *white-and-hard* or *white-or-hard* do. The only apparent exception that I can think of would be propositions like "Smith and Brown are a couple," which clearly cannot be analysed into "Smith is a couple and Brown is a couple". But Mr. Johnson would no doubt meet this by his distinction between the conjunctive and the enumerative *and*.

Chapter iv. deals with the ordinary formal development of the syllogism. I need scarcely say that this is done as well as it could be done. There are just three points worth special mention. (1) Mr. Johnson criticises the ordinary method of reaching the valid moods by laying down rules and striking out the moods that conflict with them. He justly points out that this will not suffice to guarantee the validity of those that are left. For this a positive set of dicta is needed. These Mr. Johnson supplies. (2) In place of the by no means obvious rule that a negative conclusion needs a negative premise Mr. Johnson substitutes the proposition that three classes S, M, and P, can be co-extensive. As a matter of fact the rule in question is only needed to cut out the mood *PaM . MaS implies SoP*. To deny the validity of this is equivalent to saying that SaP, MaS, and PaM are consistent; and this is equivalent to Mr. Johnson's rule, as the reader can easily see for himself. (4) Mr. Johnson makes a practical remark which all who have to teach elementary logic will do well to bear in mind. In giving examples of syllogisms we should take care that our premises and our conclusions are neither obviously true nor obviously absurd. The former error will make our students confuse formal validity with material truth, the latter will make them think that the syllogism is a mere game. Mr. Johnson recommends examples from casuistry, economics, and politics, and supplies some amusing examples about the veracity of my Lord Grey, which he apparently regards as neither axiomatic nor obviously incredible.

Chapter v. deals with what he calls the *Functional Extension of the Syllogism*. Here the major is a numerical law of the form $P = f(M)$, *e.g.*, the gas law. The minor is of the form: "In this case M has the value m". The conclusion is: "In this case P has the value p, which $= f(m)$". (Where Mr. Johnson got his extraordinary expression for the gas law—$T = 239PV$—is more than I can imagine.)

The rest of the chapter is mainly taken up with cases where we are given (say) P as a function of A, B, C, D, and we try to get (say) A as a function of P, B, C, D.

Chapter vi. is extremely important; for it deals, under the heading of *Functional Deduction*, with all the reasoning of pure mathematics, except that of Euclidean geometry, which Mr. Johnson considers to have certain peculiarities of its own. The premises of functional deductions are equations of the form $f(A,B,C) = \phi(A,B,C)$ for all values of the variables. The argument is applicative, and takes place by substituting *connected* complexes for simple variants in these functions. If for A you substitute $(x + y)$ and for B $(x - y)$, for instance, the two expressions would be connected constructs because of the common terms x and y in both. To take a very simple example; from the formula $(a + b)(a - b) = a^2 - b^2$ we derive the formula $4xy = (x + y)^2 - (x - y)^2$ by substituting for a and b respectively the connected complexes $(x + y)$ and $(x - y)$.

Mr. Johnson points out two important characteristics of this type of reasoning. (1) It is demonstrative, and yet can lead to conclusions which apply more widely than the premises, and (2) it is impossible to reduce it to syllogistic reasoning. As regards the first point his meaning is the following. Suppose you start with a premise that involves two distinct variants, A and B. Then, if A be susceptible of n values and B of m, it is clear that the formula covers mn cases. Now substitute for A and B respectively the two connected complexes $f_1(A,B,C)$ and $f_2(A,B,C)$, and suppose that C is susceptible of p values. We shall derive a general formula about A, B, and C which will cover mnp cases. If we are dealing with ordinary algebraical formulæ all our variables are supposed to be capable of representing any number, and so $m = n = p = 2^{\aleph_0}$, the number of the arithmetical continuum. In this case the actual *number* of cases to which the conclusion applies is the same as the number to which the premise applies; for $mnp = mn = m$, when we are dealing with transfinite cardinals. Nevertheless, it remains true that the cases covered by the conclusion contain all and more than all the cases covered by the premise; just as Space contains all and more than all the points on any straight line, although the cardinal number of points in a line is the same as that of the points in the whole of Space.

There is a point here which Mr. Johnson does not bring out explicitly. Suppose that your premise was a formula whose variants were definitely confined within a certain range of values, could you be sure that all substitutions of connected complexes would be valid? It seems to me that you could not. Suppose, *e.g.*, that your premise was a formula about X and Y, and that the values of X were restricted to integers between 0 and 3, and the values of Y were restricted to integers between 2 and 5. Then any attempted argument which proposed to substitute (X + Y) for X would break down. For the only possible values of (X + Y) would be 4, 5, and 6, all of which lie outside the range of possible values for X. Thus the fact that the range of variation of all the variables in an algebraical formula is the whole number-continuum seems to be an important condition of the general validity of this type of deduction. The second peculiarity of functional deduction may be illustrated as follows. By purely syllogistic reasoning we could not prove anything about the numbers which are divisible by both 2 and 3, which is not also true of all numbers divisible by 2 and of all numbers divisible by 3. But by functional deduction we can prove properties which are true of this particular species of numbers and are not true of either of the genera to which it belongs.

The last point to notice in this chapter is the very severe criticism of Russell's *Principle of Abstraction*. Mr. Johnson agrees that Mr. Russell proves the proposition which goes under this name, provided we grant the reality of classes, which Russell himself afterwards attempts to deny. But he holds that the proposition which is proved is so tame as to be of no philosophical interest whatever. Mr. Johnson is no doubt right on both counts. But, as regards the first, I should think it would be quite easy for Russell to restate the Principle in terms of his "no-class" theory, for he does not get rid of classes and substitute *nothing whatever* for them. As regards the second, the criticism is perfectly valid against some applications which Russell made of the Principle in his hot youth. (I think I am doing him no injustice when I say that at one time he thought he had proved the absolute theory of time by the Principle of Abstraction.) But I presume that these were *péchés de jeunesse*, over which Mr. Russell would wish now to draw a veil.

Chapter vii. is a long and interesting one on the *Different Kinds of Magnitude*. I can only briefly indicate some of the more interesting points in it. The best previous treatment of the subject is of course in the *Principles of Mathematics*. (Mr. Johnson does not seem to be acquainted with the very difficult later theory of the *Principia*, which, so far as I know, no philosopher has yet dared to criticise or even mention.) Mr. Johnson differs a good deal from Mr. Russell. (1) He counts numbers as magnitudes. (2) He distinguishes them as *abstract* from *Concrete Magnitudes*, like lengths and temperatures. (3) He calls the latter *quantities*, whereas Russell confines this name to substances having magnitude, such as foot-rules. (4) He distinguishes between *extensional* wholes (classes), whose magnitudes are numbers, and *extensive* wholes, like areas and stretches of time. He brings out in a most admirable way the points of analogy and difference between the two. (5) He distinguishes between *distensive* and *intensive* magnitudes. The former seem to be degrees of difference, and their zero is identity. The zero of intensive magnitude is non-existence. (6) He holds a characteristic, and to my mind very doubtful, view that magnitudes of different kinds can be multiplied and divided by each other to give new kinds of magnitude, such as area and velocity. The more usual view of course is that it is only the numerical measures of the magnitudes that can be multiplied and divided. It seems to me that the following is an objection to Mr. Johnson's view. He admits that only homogeneous magnitudes can be added. But multiplication is primarily repeated addition. It is therefore difficult to see that he can consistently hold that non-homogeneous magnitudes can literally be multiplied when they cannot literally be added.

The chapter contains a short, but most illuminating, discussion on the absolute and relative views of Space and Time. Mr. Johnson holds that two different controversies have been confused under this head. One is the question whether there are substantival entities of a peculiar kind (points and instants) between which spatial and temporal relations ultimately hold, or whether such relations hold directly between what would commonly be said to "occupy" points and instants. This might be called the *Substantival v. the Adjectival Theory* of Space and Time. Mr. Johnson inclines to the adjectival view, and dismisses points and instants as "substantival myths". The other question is whether position in space or time can only be defined in terms of relations. This is a question that could arise just as much on the substantival as on the adjectival view. I gather that Mr. Johnson inclines to the non-relational form of the adjectival theory. There is a third view, *viz.*, that points and instants are certain classes of events or objects. This has of course been greatly developed in recent times by Whitehead. I suppose we might say that this makes points and instants "adjectival" as well as "substantival myths". This view Mr. Johnson rejects with scorn, but I am not altogether persuaded by his arguments against it.

The rest of the book deals with all forms of Induction except the problematic kind. We have already seen the wide sense in which Mr. Johnson uses the term *Induction*, and have described Intuitive Induction. Chapter ix. treats of what he calls *Summary Induction*. This starts with the familiar "Perfect Induction," which, Mr. Johnson points out, can be reduced to syllogism. The remainder of the chapter deals with the establishment of Euclidean propositions by the use of figures. Purely analytical geometry proceeds wholly by functional deduction, but its axioms and therefore its conclusions are wholly hypothetical. In Euclidean geometry, according to Mr. Johnson, the axioms and propositions are asserted to be true of things in nature. We might have established enough axioms by summary induction from figures, and then have used nothing but functional deduction in our proofs. But this has not in fact been done; the explicit axioms of Euclid are not adequate to guarantee deductively all his conclusions, and that is why figures have to be used in geometrical proofs. At certain stages in the proofs summary inductions have to be made, and so a bad figure may lead to false conclusions. Mr. Johnson illustrates this last point very happily by a pleasing fallacious proof that all triangles are isosceles.

It remains to explain how Mr. Johnson supposes that summary induction establishes geometrical propositions from figures. The example that he gives is the establishment of the axiom that two

Euclidean straight lines cannot cut in more than one point. So far as I can understand, the process is supposed to be as follows: We image one fixed line AB and another cutting it at A. We then image this other line AX as continuously rotating about A. We see that in each of its positions it does not cut AB again, and we sum this up in the perfect induction that it never cuts it again. There are three points to notice: (*a*) Mr. Johnson holds that we succeed in imaging an actual infinity of positions. I should have thought it was just as impossible to image this as to sense it. (*b*) He insists that the process must be done by imaging, and not by perception, because "It is only through imagery that we can represent a line starting from a certain point and extending indefinitely in a certain direction" (p. 202). If Mr. Johnson can have indefinitely extended images he is more fortunate than I. (*c*) I understand Mr. Johnson to hold that the axioms of Euclidean geometry are supposed to be true of the physical objects in the external world. I should have thought it was extremely rash to extend the geometrical properties of our images to physical objects.

The last two chapters are devoted to what Mr. Johnson calls *Demonstrative Induction*. His treatment falls into two parts; (1) certain types of hypothetical syllogism in which an instantial premise leads to an universal conclusion, and (2) his substitute for Mill's Methods. The typical example of hypothetical argument which Mr. Johnson gives is of the form: "If some S is P then all T is U; but this S is P; therefore all T is U". It is thus an argument whose major is a hypothetical proposition with a particular antecedent and an universal consequent. The other premise is the assertion of a certain instance in accordance with the antecedent. The conclusion is of course the assertion of the universal consequent. Now no one would deny the validity of such arguments; the only question is whether they can be called inductive, even in the wide sense in which induction is defined by Mr. Johnson. In their most general form they hardly can be called inductive, for the conclusion is not a generalisation of the instantial minor. Mr. Johnson next quotes examples in which he alleges that the conclusion really is a generalisation of the instantial minor. One example is: "If some boy in the school sends up a good answer, then all the boys will have been well taught; the boy Smith has sent up a good answer; therefore all the boys have been well taught". I cannot myself see that the conclusion of this is a generalisation of the instantial minor. I should have thought that it was obvious that "All the boys have been well taught" could only be a generalisation of such an instantial proposition as "The boy Smith has been *well taught*," whereas the actual minor is "The boy Smith has *sent up a good answer*". I therefore see no ground for counting even this argument as inductive. In fact the only argument of this type which would be genuinely inductive, in Mr. Johnson's sense, would be of the form: "If some boys in the house have measles, all will have measles; the boy Smith has measles;

therefore all the boys in the house will have measles". This is demonstrative and inductive, and not altogether remote from the real facts of life, as housemasters know to their cost.

Mr. Johnson points out that arguments of this kind really are common in science. From what we know of the atomic theory we can say with great probability that "If one sample of Argon has a certain atomic weight, then all samples of Argon will have the same atomic weight". We then find that the atomic weight of a certain particular specimen is 40. And we are justified in concluding that all specimens of Argon will have atomic weight 40, provided our major is correct.

I will end with an account of Mr. Johnson's substitute for Mill's Methods. He sees clearly that Mill was confused as to the nature of the methods. Really they should be purely demonstrative, leading to conclusions which are as certain as their premises. And their premises have to be borrowed from the results of problematic induction. Now Mill hardly distinguished the Method of Agreement from Induction by Simple Enumeration, which is a form of problematic induction. Again, he thought that the ultimate majors of these arguments were very wide general principles, like the Law of Causation. Mr. Johnson points out that they need much more definite and concrete majors before they can be rendered genuinely demonstrative. These majors have to be established by problematic induction, and they take the following form in the simplest case. Certain sets of generic characteristics ("determinables," as Mr. Johnson calls them) determine a certain other generic characteristic. Each determinable is susceptible of a number (finite or transfinite) of specific modifications. *E.g.*, "colour" is a determinable, and a certain definite shade of red is a determinate under it. And of course each determinate is capable of being exhibited in an infinite number of particular instances. With these preliminaries we can state the kind of major premise which will serve for a demonstrative induction. We need—if I understand Mr. Johnson rightly—in the simplest case, to establish a proposition of the following kind as a premise. (1) In all cases where all the determinables ABCD are present the determinable P is present; and no other determinable (say Q) is present in all these cases. (2) In all cases where the determinable P is present all the determinables ABCD will be found; and there will be no other determinable (say E) common to all these cases. When such a premise has been established the demonstrative induction rests on certain axioms about adjectival determination. Let us see how much freedom this premise allows us. If I interpret Mr. Johnson rightly it is quite possible (1) that we should have *abcdp* and *a′b′cdp*, for instance. (2) It is even possible that we should have *abcdp* and *a′bcdp*. But (3), if this be so, we cannot have *a″bcdp″*. In fact we may here conclude A*bcdp*, *i.e.*, that, although the *presence* of A in some form is necessary to the *production* of *p* yet its *variations* are irrelevant to the *variations* of *p*, so long as BCD have the specific values *bcd*. (4) Even if we have *Abcdp*, we must not conclude that variations of A will be irrelevant to variations of *p* when BCD are not confined to the specific values *bcd*. We may perfectly well have *ab'cdp'* in spite of *Abcdp*. (5) Lastly, if we find that *abcdp* and *a'bcdp'*, then we cannot have *a"bcdp* or *a"bcdp'*; we must have *a"bcdp"*. *I.e.*, if *any* variation of A is relevant to variations of P, while BCD have the specific values *bcd*, *all* variations of A will entail variations of P under the same conditions. But (6), even if this be so, we must not conclude that, when the specific values of BCD are no longer confined to *bcd*, we cannot have such a case as *a"b'cdp*.

In all these arguments it is assumed that the determinables under discussion are "simplex," *i.e.*, that A, for example, is not really a complex of two or more determinables, say A_1A_2. It is also assumed that ABCD are all independently variable. Taking such a major as this, and supplying it with different sorts of minor from our observations, it is clear that we can arrive at four different types of conclusion, according to the nature of the factual minor supplied. (1) If all are simplex, and *abcdp* and *a'bcdp* then *Abcdp*. (2) If all are simplex, and *abcdp* and *a'bcdp'*, then *a"bcdp"*, where *p"* differs from both *p* and from *p'*. (3) If all be simplex, and *abcdp* and *a'bcdp'* then *a"cdp* must be *b"*, where *b"* differs from *b*. (4) If *abcdp* and *a'bcdp'* and *a"bcdp* then A cannot be simplex but must be of the form A_1A_2.

These four types of argument Mr. Johnson calls respectively the figures of *Agreement*, *Difference*, *Composition*, and *Resolution*. The reasons for the first two names are obvious. In the third, after a variation in A has produced a variation in P we find that a further variation in A does not produce the expected further variation in P. We therefore conclude that this variation in A has been *compounded* with and neutralised by a variation in some other factor such as B. In the fourth we have the same sort of facts to explain; but we know that there has been no variation in the other factors, whilst we are not sure that all the factors are simplex. We are therefore forced to *resolve* the factor about whose simplicity we were doubtful into two or more factors.

Mr. Johnson illustrates his Figures and then deals with the more complex and actual case of a determined result involving several determinables PQRS, say. The general principles involved are the same, and will be clear to anyone who has understood the argument in the simpler cases.

I think there can be no doubt whatever that Mr. Johnson's Figures are a great improvement on Mill's Methods, both in logical rigour and in approximation to the actual procedure of scientists. There is, however, one criticism which strikes me. Surely the axioms on which Mr. Johnson bases his Figures wholly ignore the possibility of the laws of adjectival determination sometimes taking a *periodic* form. Suppose it happened that P was so connected with ABCD that—

$$P = A \sin (BC + D).$$

Then we should have $p = a \sin(bc + d)$ and $p' = a \sin(b'c + d)$ and yet $p = a \sin(b''c + d)$, provided that b'' is and b' is not equal to $b + 2\pi n/c$. Nor is this an outrageous supposition, since electromagnetism mainly rests on laws of this kind.

I have perhaps said enough to show that Mr. Johnson's book is one which no one interested in Logic and Scientific Method can afford to neglect. It contains many controversial points, as any thorough treatment of such difficult subjects must do; but I have no hesitation in saying that it is the best book that has appeared, or is likely to appear for a long time, on the absolutely fundamental questions with which it deals.

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De l'Explication dans les Sciences. Par ÉMILE MEYERSON. Paris: Payot & Cie, 1921. 2 vols. Pp. xiv, 338 and 469. Price 40 fr.

I.

M. MEYERSON here deals from a different point of view with the problem which he handled with so much distinction in *Identité et Réalité* (1st ed., 1908, 2nd ed., much enlarged, 1912: Paris, Felix Alcan). These two books deserve to be widely known in this country, both to philosophers and to scientists. M. Meyerson's style is a model of concreteness and lucidity; his argument is wonderfully continuous, in spite of the wealth of illustration drawn from the history of science with which he enforces it.

The problem is one of theory of knowledge: to discover "the essential principles of thought." The method is to examine the processes of scientific reasoning as actually exhibited in the history of science (ix.). His work is not metaphysics, but, he hopes, "prolegomena to any future metaphysics" (xii.).

In this examination he does not trust the scientist's own accounts of his processes, but studies the scientist at work, so as to see how he acts. M. Meyerson's study then can be described as a study of scientific reasoning from a behaviourist standpoint (*e.g.*, *Identité et Réalité*, 432-433).

In *Identité et Réalité* this investigation was pursued empirically. In the present book an attempt is made to justify the same results by a more deductive consideration of the conditions of scientific explanation as such.

II.

It is assumed throughout that man's reason is an instrument which has to be applied to the original data of experience (sensations) in order that a world may be experienced at all. This instrument, reason, has a structure, a form, which has remained without evolution at least during historic times, although there has been a steady evolution in the products of reasoning as applied to