

II. MR. JOHNSON ON THE LOGICAL FOUNDATIONS OF SCIENCE (II).

BY C. D. BROAD.

(B) CAUSATION.—So much has necessarily been said about causation in dealing with the Continuant that we can afford to be reasonably brief. The notion of Causation is introduced in the chapter on *Fact and Law* with which the book begins. Mr. Johnson at once rejects two fairly popular opinions. The first is that the assertion "*p* determines *q*" is really a statement about our minds, *viz.*, that whenever we characterise anything as "*p*" our minds are determined to characterise it further as "*q*". This Mr. Johnson calls a purely "epistemic" view of causation. He himself holds what he calls a "constitutive" view, *viz.*, that there is a relation of a peculiar kind between the fact of being characterisable as *p* and the fact of being characterisable as *q*, and that this holds regardless of minds. It seems to me that Mr. Johnson is here hardly using "epistemic" and "constitutive" in his usual senses. On the theory which he rejects there is a real relation of causal determination, only it is supposed to be confined to certain states of mind. On the theory which he accepts this same relation holds also between events which are not states of mind. I should hardly have thought that a mere difference of opinion about the *range of application* of a relation whose *existence* is apparently admitted by both parties could be accurately described as a difference between a constitutive and an epistemic view of causation.

The second view which Mr. Johnson rejects is roughly the theory that causal laws are just statements of *de facto* regularities. He distinguishes between such propositions as "Anything that was *p* would be *q*" and "Everything that is *p* is *q*". The former he calls "Universals of Law," and says that they express "nomic necessity". The latter he calls "Universals of Fact". The former imply the latter, but are not equivalent to them. A "nomically contingent" proposition is of the form "A thing might be *p* without being *q*," and this must be distinguished from the particular factual proposition "Some *p* is not *q*". Now causal laws are

universals of law and are nomically necessary. The need for this distinction is perhaps most clearly brought out in I. § 5. In the first place, the belief that if p were to happen q would happen is often the reason why p never does happen, and therefore why the factual universal "No p is non- q " is true. Thus, it is commonly believed that "if a person were to go to a Royal garden-party in bathing-drawers he would be turned out"; and it is for this reason (among others) that no-one *does* go in that costume; whence it is true that "no-one goes so attired and fails to be turned out," which is the corresponding factual universal. Now, if the general belief in x is what causes y to be true it can hardly be maintained that x and y are the same proposition. Secondly, we assert such propositions as "if the molecules of a gas had no extension it would accurately obey Boyle's Law". And we know that there are no gases of which this is true. Now the corresponding factual universal would be (when stated in negative terms) "No gas both has unextended molecules and fails to obey Boyle's Law". This is of course true, since it is implied by the proposition "No gas has unextended molecules". But the latter proposition equally implies the factual universal "No gas both has unextended molecules and fails to disobey Boyle's Law". There is no inconsistency between these two factual universals, and they are both true. But there certainly is an inconsistency between the two propositions "If a gas had unextended molecules it would obey Boyle's Law" and "If a gas had unextended molecules it would disobey Boyle's Law". Hence it seems necessary to distinguish between the universal of law and the corresponding universal of fact.

What Mr. Johnson does not seem to bring out very clearly is the connexion or lack of connexion between nomic necessity and logical necessity, *e.g.*, between the kind of necessity which belongs to the proposition that "if a billiard-ball were hit it would move" and the kind of necessity which belongs to the proposition that "if all S were P all non-P would be non-S". Dr. McTaggart apparently identifies the two, and a discussion of the subject by Mr. Johnson would have been interesting. I can find only two passages which seem to throw light on his view of this question. In I. § 4 he says that a nomic proposition "expresses a relation between the characters p and q indicative of the nature of the world of reality". In VI. § 3 he draws a distinction between causal laws and "Formal Universals". Under the latter head he includes the laws of kinematics and the whole of geometry. Formal Universals are laws which apply to space, time, and motion as such, apart from any

question of their concrete filling. They do not apply to existents, if by an "existent" you mean "whatever is actually or potentially manifested in space and time". On the other hand, causal laws apply to the concrete filling of space and time; they presuppose formal universals, but the converse does not hold. It is clear from these passages that Mr. Johnson means to restrict the *subject-matter* of nomic propositions to occurrents and continuants. But this still leaves it uncertain whether he supposes the *necessity* which characterises the nomic, the formally, and the logically universal to be the same or different.

To understand the details of Mr. Johnson's treatment of causation it is necessary to notice his distinction between "cause-factors" and "a completed cause" and "effect-factors" and "a completed effect". He holds that the occurrence of any characteristic is causally determined by a *finite* number of other characteristics. *E.g.*, suppose that something in fact has the character e . Then it is certain that there is a finite set of characters, say $c_1 \dots c_n$, possessed by this thing, and such that anything which had this set of characters would also have e . But it may be that a thing which has $c_1 \dots c_m$ without $c_{m+1} \dots c_n$ need not have e , and it may be that the former sub-set can occur without the second. A cause-factor or an effect-factor is apparently a single characteristic. A completed cause of a given effect-factor e is a set of characters $c_1 \dots c_n$ such that anything that had this set would have e also. A completed effect of a given cause-factor c is a set of characters $e_1 \dots e_n$ such that anything that had this set would have c also. (I. § 4 and V. § 1.)

With these definitions it is obvious that there can be plurality of completed causes relative to a given effect-factor and plurality of completed effects relative to a given cause-factor, for this merely amounts to saying that we cannot simply convert an A-proposition. (V. § 3.) It does not in the least follow from this that causal laws in terms of *completed* causes and *completed* effects cannot be stated in a reciprocal form. By a process of gradual modification at both ends we may get from an irreversible law, such as "Anything that was $c_1 c_2 c_3$ would be e_1 ", to a law of the form "Anything that was $c_1 \dots c_n$ would be $e_1 \dots e_m$ and anything that was $e_1 \dots e_m$ would be $c_1 \dots c_n$ ". (V. § 6.) In fact Mr. Johnson holds that we have not got a causal law properly stated until the following conditions are fulfilled:— (1) All the cause-factors are independently definable and variable; (2) All the effect-factors are so too; (3) None of

the effect-factors can be inferred from any selection less than the whole of the cause-factors, and none of the cause-factors can be inferred from any selection less than the whole of the effect-factors. When these conditions are fulfilled the causal law is reversible.

It will be noticed that in this discussion on Plurality in V. Mr. Johnson does not give any rule for distinguishing a cause-factor from an effect-factor. If the characteristic with which you start is to be called a *cause*-factor then the set of characteristics which together imply it is to be called a completed *effect*; if it is to be called an *effect*-factor then the set of characteristics which together imply it is to be called a completed *cause*; but why it is to be called a cause-factor in some cases and an effect-factor in others Mr. Johnson does not here explain. The discussion of this point is carried a little further in VI., and runs as follows. Some philosophers have made the cause a property of a continuant and the effect an occurrent. An example would be if we said that gravitation caused the fall of the Campanile. On this interpretation, of course, there is complete lack of homogeneity between cause and effect. (VI. § 2.) But Mr. Johnson holds, quite rightly, that the causal relation is primarily between occurrents (*ibid.*), though he maintains that these occurrents must be located in certain specified continuants and that the properties of these continuants must be mentioned in any complete statement of causation. (VI. § 4.) Since the completed cause and the completed effect *both* involve the three factors of occurrent, the continuant to which it belongs, and some property of this continuant, it is clear that they cannot be distinguished by the different nature of their constituents. Moreover, Mr. Johnson explicitly says that cause and effect are not *epistemically* distinguishable. The cause can be inferred from an adequate knowledge of the effect just as well as the effect can be inferred from an adequate knowledge of the cause. And he admits that it remains a serious question whether there is anything left by which cause and effect can be *ontologically* distinguished. (VI. § 2.)

The question has to be dealt with separately for transeunt and for immanent causation. For the former I think that Mr. Johnson's solution is as follows. Transeunt causation always requires two continuants C_1 and C_2 in some specific relation R to each other. When this relation has been established between C_1 and C_2 , the state of C_2 (say) which immediately follows differs in some assignable way from what it would have been if C_2 had been left to itself. We then

call the establishment of R between C_1 and C_2 a *cause-factor* in this transaction; and we count the divergence of C_2 's immediately subsequent state from what it would otherwise have been as the *effect* of this. Of course at the same time C_1 's state may be modified by the establishment of R between C_2 and C_1 . But this introduces no difficulty; it is simply a case of reciprocal transeunt causality. (*Introd.* § 7.)

Now, so far as I can see, in all cases where there is a difficulty in distinguishing between cause and effect Mr. Johnson has to appeal in the end to transeunt causality. Two cases arise over immanent causality. (1) We may have simultaneous immanent causality. Mr. Johnson illustrates this from the gas-law, $pv = Rt$, where p , v , and t , stand respectively for the pressure, volume, and temperature of a given mass of gas, and R is a constant. Which of these are you to call the *cause* of the rest? "So long as we are concerned only with immanent causality there is absolutely nothing to determine which . . . is to be called cause and which effect." (IX. § 4.) Mr. Johnson's solution is as follows. The whole process must be analysed into three stages, of which two are transeunt and one is immanent. He begins by distinguishing between the *external* pressure, volume, and temperature— p_e , v_e , and t_e —and the corresponding *internal* variables— p_i , v_i , and t_i . The former are the weight on the piston, the volume of the container, and the temperature of its walls. The latter are the reaction of the gas on the walls, and its volume and temperature. Suppose now that the experimenter arbitrarily modifies p_e and v_e by pressing down the piston. Then the whole causal process must be analysed as follows. (a) An inward transeunt process in which the internal pressure and volume are modified according to the laws $p_i = p_e$ and $v_i = v_e$. (b) An immanent process in which the internal temperature is modified in accordance with the law $t_i = p_i v_i / R$. (c) An outward transeunt process in which the external temperature is modified in accordance with the law $t_e = t_i$. Now in the transeunt processes there is no difficulty in saying which is cause and which is effect. The arbitrary change of external pressure and volume is the cause in the first, and the change of internal temperature is the cause in the second. Mr. Johnson's rule is that in the immanent process those factors must be taken as *causes* which are *effects* in the previous transeunt process, and those must be taken as *effects* which are *causes* in the subsequent transeunt process.

(2) In XI. § 5 Mr. Johnson goes further and asserts that it would be impossible to draw a distinction between cause

and effect in purely immanent causation, even when it is successive, and not simultaneous as in the case just considered. We all believe that ontologically the earlier parts of the history of a continuant determine the later parts and not conversely. Yet there would have been nothing to suggest this to us if all causation had been immanent; for, with an adequate knowledge of the nature of a continuant we can infer backwards just as certainly as forwards in its history, so long as it is left to itself. As it is, however, "an immanent process of causality may be broken in upon from without by an influence which modifies the succeeding manifestations. . . . After the interruption the relation of the succeeding to the preceding is objectively differentiated from that of the preceding to the succeeding." (XI. 6.) I must confess that I do not clearly understand this. Take, *e.g.*, a moving billiard-ball which hits a cushion and rebounds. Consider two successive stages x and x' in its course before the impact, and two successive stages y and y' in its course after the impact. Then (a) x' can be inferred by a purely immanent law from x , and conversely. (b) y' can be inferred from y by a purely immanent law, and conversely. (c) Neither x nor x' can be inferred from y or y' , nor conversely, by a purely immanent law. But (d) y or y' can be inferred from x or x' together with a knowledge of the impact; and equally x or x' can be inferred from y or y' together with a knowledge of the impact. I really cannot see where the "objective differentiation" comes in in all this; everything seems to be perfectly symmetrical.

The only other points that I need mention in Mr. Johnson's doctrine of causation are the following. (1) In successive causation we must not suppose the cause and the effect to be momentary events. They are events of finite duration which are adjoined at a common temporal boundary. Thus the typical statement would be: "The change from A to B causes the change from B to C". (VI § 6.) (2) In transeunt causation the cause and the effect are always simultaneous. (XI. § 5.) *E.g.*, I suppose, that a weight does not have to stand on a table for any finite time before the upward reaction of the table upon it begins. Immanent causation may be either simultaneous or successive. (3) In transeunt causation the cause and the effect always have to stand in a certain peculiar relation which is not temporal. In physical affairs it is a spatial relation, such as contact. What precisely it is in psycho-physiological causation Mr. Johnson does not distinctly tell us.

(C) THE LOGIC OF PROBLEMATIC INDUCTION.—The validity

of all science, according to Mr. Johnson, rests on certain "postulates". These are propositions which are accepted assertorically and not merely hypothetically, which are nevertheless not self-evident nor capable of inductive proof, and which involve concepts (such as cause and substance) which are "not given in experience". These postulates enter even into the singular perceptual judgments which form the materials that science generalises. (*Introd.* § 3.) Whether Mr. Johnson thinks that any further postulates are needed for generalisation, which do not enter into singular perceptual judgments, and, if so, what they are, is not clear to me.

In inductive generalisation we start by observing certain instances which have some determinate value of P and some determinate value of Q. We then want to know whether All P is Q or All Q is P. The question which of these two generalisations we shall seek to make is determined by whether we have already found that Some P is not Q or that Some Q is not P. Let us suppose that we have found the latter and not the former, so that we are seeking to establish that All P is Q. The kind of evidence that we need is the following. In the first place we confine ourselves to a single determinate p_1 under the determinable P, and we try to observe as variable a collection of instances having p_1 as possible. Suppose we find that they all have a certain determinate value q_1 under the determinable Q. We next examine in turn sets of instances having the determinates $p_2, p_3 \dots p_n$. We will suppose that all the members of each such set are found to have a certain determinate value of Q, and that these values differ for each set. They might be $q_2, q_3 \dots q_n$. As we have said, within each set we shall try to vary the instances as much as possible. On the other hand, as between any two sets we want as little variation as possible in all other respects except the values of P which distinguish them. Our observational data are now of the form All observed p_1 was q_1 and All observed p_2 was $q_2 \dots$ and All observed p_n was q_n . The corresponding generalisation will be of the form All p_1 will be q_1 and All p_2 will be $q_2 \dots$ and All p_n will be q_n . Now the vitally important point to notice is that the *whole* of the first set of facts is the evidence for *each separate constituent* of the generalisation. Our evidence for believing that all water will boil at 100° C. at normal pressure is not simply that all observed *water* has boiled at *this* temperature, but that all samples of each chemical compound which have been observed (*e.g.*, alcohol, ether, chloroform, etc.), have been found to have a characteristic boiling-point under a given pressure. I do not think that

this point has ever been brought out so clearly as it is by Mr. Johnson's notation. A second feature to notice is that the evidence falls into two parts, *viz.* (a) that constancy in P is accompanied by constancy in Q in spite of variations in other factors, and (b) that variation in P is accompanied by variation in Q in spite of constancy in the other factors.

On the logical relevance of number and variety of instances Mr. Johnson takes very much the same view as Mr. Keynes. We have to remember that P may be a complex group of determinables ABCD. The great danger of inductive generalisation is the following. All the observed instances that were q_1 may have had *abcd* in common. We may omit to notice *d*, or may notice it and treat it as irrelevant. In that case we are likely to put our generalisation in the form All that is *abc* is q_1 . And this may be too sweeping; the proper form being All that is *abcd* is q_1 . Strictly, we shall only be safe if we put into our subject *all* that is common to the observed instances. But this is not practicable, and we have to distinguish as best we can between relevant and irrelevant common features. The object of choosing as variable instances within a set as possible is to reduce the common features which we do not include in our subject as much as we can. And the only object of multiplying instances within a set is the *hope* that we shall thereby reduce the common features, even where we cannot be positively *sure* that we are doing so. Mr. Johnson points out that the fact that all the observed instances have fallen into a certain restricted region of time or space may be relevant; not because absolute position in space and time is relevant in any causal law, but because all the instances that fall into such a region may agree in their close spatio-temporal proximity to some particular causal agent, and this fact may be highly relevant.

The following remarks may be made here. (1) This shows the practical futility of Mr. Keynes's form of the "Uniformity of Nature". (2) In *all* inductions our observations have in fact been confined to a comparatively small region of time and space. Indeed the temporal limits are ridiculously small, since they are determined by the length of human tradition up to date. This should make us very doubtful of inferences about the remote past or future based on inductive generalisations. (3) We always start an enquiry with pretty definite views about what is likely to be relevant to what. These are based on past experience. Where we lack this basis, as *e.g.*, in Psychical Research, we can attach very little weight to our inductions. (4) Relevance is a matter of degree. The more

minute the phenomena that we are investigating and the more accurate we want to make our results the less we can afford to treat as irrelevant. (5) There have been certain features, such as the fixed stars and the present constitution of the solar system, which have been present in all human observations and cannot be varied by us. We therefore cannot tell whether they may not be relevant to all our laws. This does not matter very much when we are predicting only a little way ahead; but it makes predictions to very remote periods, when the structure of the solar system and the positions of the fixed stars may be very different, highly precarious. (6) Mr. Johnson does not apparently discuss another source of weakness which Mr. Keynes notices, *viz.*, that we may put more than we ought into the predicate of our generalisation, and conclude, *e.g.*, that All P is XYZ when we ought only to conclude that All P is X.

It remains to consider the detailed theory of "Eduction" which is put forward in IV. and in the Appendix. From such a premise as "Certain things which are *m* are *p*" the first step that one can take is to "A certain further thing which is *m* (*e.g.*, the next one that I meet) will also be *p*". This is *Eduction*. The next step is *Induction*; but it branches into two forms according to the nature of the original premise. If *all* the observed things which were *m* were *p*, we may proceed to the *Pure Generalisation* that all things which are *m* will be *p*; but, if only a certain proportion of the observed things which were *m* were *p*, we can only proceed to the *Statistical Generalisation* that such and such a proportion of things which are *m* will be *p*. (I have invented the last two terms, because something has evidently gone wrong with Mr. Johnson's nomenclature. What he calls "Appendix on *Eduction*" is not mainly on *Eduction*, as defined by him in IV. § 1, but is rather on the special kind "inductive inference whose conclusion is class-fractional" (IV. § 3), which I have called "statistical generalisation".)

In IV. he considers eduction from premises all of which are favourable. The typical argument has three premises and two middle terms, one substantival and the other adjectival. It may be put as follows:—

s is characterised by $p_1 \dots p_m$
 $p_1 \dots p_m$ characterise $s_1 \dots s_n$
 $s_1 \dots s_n$ are characterised by *p*
 Therefore *s* is characterised by *p*.

Mr. Johnson argues, rightly I think, that there is no "pure induction" and no "pure analogy". One can only say that

some arguments are more of one type and some more of the other type. The most purely analogical argument must introduce at least two substances which are known to agree in a number of characters; and the most purely inductive argument must introduce at least two characters which are known to belong to all the instances examined in the premise. Mr. Johnson also works out in elaborate detail the three kinds of negative evidence that are favourable to an eduction. There is no need to excite ourselves over these, for a little reflexion and manipulation will show the reader that they can all be reduced to the form already given, provided we are allowed to introduce negative characteristics and to substitute for "*s* is not characterised by *p*" the equivalent "*s* is characterised by non-*p*".

Suppose now that we have a set of premises of the kind mentioned above. Then the addition of a further substantive s_{n+1} which has all the m predicates strengthens the conclusion if and only if it has some characteristic which all the others lack (or lacks some characteristic which all the others have). If we allow negative characteristics it does not matter which alternative we adopt; Mr. Johnson adopts the latter. (It is evident that we shall have to define what we are going to mean by a "characteristic," or this condition will become trivial. I think we shall have to exclude disjunctive characteristics, for instance.) Again, the addition of a further characteristic p_{m+1} which belongs to all the n substantives strengthens the conclusion if and only if it be nomically possible for all the rest to characterise a substantive without this one. (Here I am again departing from Mr. Johnson's actual statements. He says that it is necessary that there shall be a substantive which has all the other characteristics and lacks this one. I cannot see that this *factual particular* needs to be true; provided that the corresponding *nomical contingency* holds. If I am right Mr. Johnson has here overlooked his own distinction. But, as his rule is more rigid than mine, no practical harm would come from following it.)

A set of characteristics such that no selection from it causally determines the rest may be called an "Independency". (I am again modifying, as above.) A set of substantives, such that any one of them has (or lacks) some characteristic which is lacked (or had) by all the rest may be called a "Variancy". We may say then that an eductive argument is not in its correct form unless the substantival middle term is a variancy and the adjectival middle term is an independency. It will not be positively fallacious if these conditions be not fulfilled, but it will appear to be stronger than it really is.

About the Appendix all I can do is, with the utmost respect to Mr. Johnson, to parody Mr. Hobbes's remark about the treatises of Milton and Salmasius: "Very good mathematics; I have rarely seen better. And very bad probability; I have rarely seen worse". The subject is too technical for discussion here, and so many friendships have been wrecked on Bernoulli's Theorem and its Converse that I will content myself with saying that I am bound to reject Postulate I. on page 183, where Mr. Johnson assumes equi-probability of *ratios* instead of equi-probability of *constitutions*. I know that he has done this with his eyes open, and I must leave him to his colleague at King's. I fear that the High Table there will be rent with dissension over this business. Granted Mr. Johnson's postulate his conclusions follow, and the bit of mathematical reasoning which leads to them is extremely beautiful.¹

(D) APPLICATION OF THE NOTION OF CAUSE TO MINDS.—Mr. Johnson accepts what he calls "impartial dualism," which he considers to be the view of unsophisticated common-sense. He thinks that it cannot be proved, but he is sure that all the objections that have been brought against it are fallacious, and that no satisfactory alternative theory has been propounded. Here I wholly agree with him. This view consists in holding that certain mental events have purely mental causes; that certain physical events have purely physical causes; and that there are certain "critical points" at which we have physico-psychical or psycho-physical causation, *viz.*, when a stimulus produces a sensation or a volition produces a movement. He points out that the word "parallelism" is ambiguous. In its scientific sense it *asserts* that every mental event has a neural accompaniment, and conversely. In its philosophic sense it *denies* that there can be any causal relation between the two orders. The grounds for these two forms of parallelism are quite different; and denial of interaction is the very last thing that would be suggested to an unbiassed mind by the assertion of one-one correlation.

The average scientist does not really accept philosophical parallelism, but he also does not accept common-sense dualism. What he really holds is one-sided interaction of body on mind. He does not believe that one state of mind

¹ Mr. Johnson has lately given up this postulate; substituted a much more plausible one for it; and deduced from the new postulate, by an admirable piece of mathematical reasoning, substantially the same results as he reaches in the Appendix. Unfortunately I cannot accept the new postulate on reflection, though it looks harmless enough at first sight.

directly causes another; or that a state of mind directly causes a bodily change; neural processes are supposed to determine each other, and each link in this neural chain is supposed to produce a causally isolated mental state. The *appearance* of causal connexion between the mental states of such a series is supposed by the average scientist to be a mere result of the *real* causal connexion between their neural counterparts.

Now Mr. Johnson does not attempt directly to refute this view. He contents himself with arguing (1) that it is inconsistent with a genuinely *impartial* scientific parallelism; (2) that certain facts which are alleged in favour of epiphenomenalism are consistent with (and perhaps require for their full explanation) the action of mind on body; and (3) that scientific parallelism is probably false except in a very restricted sense. The first point is this. The immediate causal conditions of the stimulus which produces a sensation are purely physical in many cases. Impartial parallelism would suggest that the immediate causal conditions of the volition which produces a bodily movement are purely psychical. (*Introd.* § 10.) We should then have to suppose that in sensation the neural events determine each other and also a series of causally disconnected sensations, whilst in a process of deliberation the psychical events determine each other and also a series of causally disconnected neural changes. This view is not in fact held by Mr. Johnson, because he thinks that "a volition is caused . . . by such purely psychical processes as feeling, desire, knowledge, and thought, to which there are no neural or physiological correspondents". But it is the view which he would take if he were a complete scientific parallelist. (*Introd.* § 10.)

The second point seems to come to this. Suppose we consider a series of events which starts with a physical stimulus and ends with a bodily movement. Common-sense would say that the stimulus causes a sensation, the sensation a volition, and the volition a movement. The scientist would be inclined to say that the really effective links are the neural correlates of these psychical states. Mr. Johnson argues (*Introd.* § 11), so far as I can understand, that this might be admitted and yet it might be necessary to hold that mind acted on body. Different people react differently to the same physical stimulus, and they react differently at different times in their lives according to the experiences that they have had. We may say, no doubt, that this is due to the presence of *traces*, and we may hold that these traces are *now* purely physiological. Nevertheless, it may well be that they would

not have been formed at all if mind had not acted on body in the past. Thus, Mr. Johnson's position appears to be that, whilst the *proximate* causal explanation of such "behaviour-cycles" may require nothing but physical and physiological states and laws, the *existence* of some of these physiological states requires for its explanation the past action of mind on body. It is perhaps worth while to point out that this defends the popular view, that mind acts on body, only by radically transforming it. The popular view is that mind acts on body in the course of *each* voluntary action and determines *this particular movement*; the amended view is that mind *has acted* on body in the past, and has determined not a particular movement, but a *relatively permanent disposition*.

The third point is discussed at length in Chapter VIII., particularly in § 6. Mr. Johnson uses the word "sensation" in much the same way as I use the word "sensum" and he assumes without discussion that "sensations" are mental occurments. I will continue to use the word "sensum"; in stating his position. His view is that there is probably parallelism between *sensa* and neural processes, in the sense that the latter "can be described in terms of the same number of distinct determinables" as the former. But "there are other psychical phases to which no changes of neural process correspond". These are cognition and feeling, which "may be provisionally defined as . . . variable relations or attitudes towards sense-experiences". (VIII. § 2.) The argument for this view is roughly the following. Everything that can be said about the qualities or relations of neural events is needed to account for the qualities and relations of *sensa*; and so nothing is left in the neural events to correspond to the various cognitive or affective attitudes which we may take towards *sensa*. Take, *e.g.*, two *sensa* in the visual field. Presumably each has some perfectly determinate shade of some colour, and they stand in some perfectly determinate spatio-temporal relation to each other. Now we can well believe that there is some specific difference (*e.g.*, in rate of vibration) between the neural correlates of these two *sensa*, and that this is correlated with their difference of shade. We can also believe that the spatio-temporal relation between the two *sensa* is correlated with that between the neural disturbances. But we must remember (*a*) that we may cognise the actual shades of colour and the actual spatio-temporal relations of these *sensa* with any degree of determinateness, and (*b*) that we may take up towards them feelings of various kinds and of any degree of intensity.

What is there left in the neural correlates to the *sensa* to account for the particular one of these attitudes and the particular degree or intensity of it which we in fact take up? (VIII. § 6.)

This argument does not seem to me very impressive. No doubt it shows that the neural correlate of a *sensum* having such and such qualities and relations cannot *also* be the neural correlate to our more or less determinate beliefs about this *sensum* and our more or less intense feelings towards it. But it leaves it perfectly possible that *elsewhere* in the whole neural situation at the time there is a correlate to these mental processes also. Mr. Johnson seems to see this perfectly obvious objection at the beginning of VIII. § 3. But he at once diverges into an elaborate discussion of "the experience of effort or strain," which, however interesting and important it may be, seems to have no direct bearing on this question. And, so far as I can see, he never gets back to this point. I agree with Mr. Johnson that there is not the faintest reason to suppose that there is a detailed neural correlate to all our mental states, and that the only place where there is decent evidence for it is sensation; but I do not think that his argument for this conclusion would persuade many people. Mr. Johnson holds that the belief that all mental processes have a neural correlate is largely due to the common confusion between thinking and having images. People hold, probably correctly, that there is a neural correlate to imagery and to association. No doubt the laws of association will account for the fact that the word "*red*" rises to my lips or its image to my mind when I see a red thing. But there is a difference between merely ejaculating "*Red!*" and thinking of it as the name of a quality which characterises a certain object. Neural process may suffice to explain why certain words, sensible or silly, come to be spoken or imaged on certain occasions; but what neural process is correlated with the act of understanding the meaning of a word or sentence?

It remains to say something about Mr. Johnson's analysis of the psycho-physical causation which is involved in voluntary action. In the first place he draws a very interesting logical distinction between two different ways in which we may get knowledge of causal determination. In some cases, he thinks, we start by observing a great deal of regularity, and end by inferring causal connexion from it in each individual case of the kind observed. In other cases we start by observing causal connexion in the individual instance, and precariously argue from this to regularity in the class.

Now he holds that the determination of our voluntary decisions is of the latter kind. "By direct introspection I feel assured that I can assign the cause of any one of my acts of will; but it is only with considerable doubt that I should venture to formulate rules in accordance with which I invariably act. In virtue of this assurance I maintain that in willing I am . . . *determined*, because my volition is not uncaused, *free*, because the immediate determinants of my volition are within my own consciousness." (*Introd.* § 12.)

This seems to me an extremely bold claim to make for introspection, in view of the possibility of mental states which cannot be introspected and which may influence my decision. It is interesting to compare it with the alleged immediate evidence of indeterminism, which Sidgwick and others have asserted. I should have thought that the latter *claim* was much more plausible than the former, though I do not think that it is irreconcilable with the *fact* of determinism, if it be a fact. Mr. Johnson carries the analysis of voluntary decision further in VIII § 9. His view is that in ordinary non-moral decision there is a conflict between desires which we simply watch till it is settled. In other cases we actively interfere by modifying the strength of the attraction and repulsion of various factors in the alternatives. This is always done by a process of selective attention; the more determinately we know any alternative the more strongly the originally attractive features attract and the more strongly the originally repulsive features repel us. People claim to be free in this special sense that they can modify the strength of their primary desires by selective attention. This claim is perfectly just, but it is quite compatible with determinism, since the direction of their attention is also determined by desires. The only criticism that I have to make on this is that I should have thought that it often happened that the more determinate knowledge of an originally attractive alternative *weakened* its attraction or changed it into repulsion, and conversely for an originally repulsive alternative.

So far we have been considering the purely psychical causation which leads up to voluntary decision. There remains the transeunt process by which decision passes into action. According to Mr. Johnson, the transition from one order to the other is marked by the experience of *effort*. This is a genuine sensation with a specific intensity and character and a definite neural correlate. Its neural correlate is the immediate effect in the brain of the decision in the mind. Thus it differs from all other sensations in that its

"stimulus" is mental and not physical. And its other peculiarity is that it leads, by ordinary physiological and physical causation, to effects which were intended by the agent. Thus we have two causal orders to consider:— (a) Foresight of external change determines voluntary decision which determines neural change. (b) Neural change determines (a) by physio-psychological causation a sensation of effort, and (β) by physiological causation the foreseen and desired result. The external result is remote, but foreseen by the agent. The internal result is immediate and appropriate, but unknown to the agent.

It is needless for me to praise a book which will obviously become a classic. To the professional logician and metaphysician Mr. Johnson's work is of course indispensable. To the psychologist it offers certain passages which he will do well to read and ponder. And the intelligent scientist who wants to see the best statement which has ever been made of the concepts and postulates which he uses daily, and who is willing to give to a pretty stiff book the same attention which he would bestow on a masterpiece in his own subject, may be most strongly recommended to devote his leisure to this work.