

V.—CRITICAL NOTICES.

Implication and Linear Inference. By BERNARD BOSANQUET.
Macmillan. Pp. ix, 180.

THIS little book, whose value is altogether out of proportion to its size, contains the clearest and most plausible account that Prof. Bosanquet has yet given of his views on logic. The author has made a careful study of recent writers whose general position differs considerably from his own, such as Dr. Mercier, Husserl, and Mr. Leonard Russell; and much light is thrown on his own system by his discussion of theirs. In particular it is pleasant to see that at least one English philosopher of eminence recognises the importance of Husserl's work, which has been strangely neglected here, possibly on account of its extreme prolixity and its barbed-wire entanglement of new technical terms.

Prof. Bosanquet is concerned to maintain that inference is everywhere of the same general type, and that it is not subsumptive or syllogistic. The true type is explained under the name of *implication*; subsumption he calls *linear inference*, and condemns as a 'second-hand' process of argument. The book falls into three closely connected parts. Chapters I., IV., V., and VIII. explain the nature of implication; and exhibit its connexion with induction, judgment, supposition, and the contrast between the necessary and the contingent. Chapters II. and III. deal with the linear view of inference, and claim to show that most of the critics of the syllogism have never freed themselves from its domination. Chapters VI. and VII. deal with points that are somewhat less vital to Prof. Bosanquet's argument, *viz.*, the constant use of sets of three terms or propositions in inference, and the question whether logic has any special connexion with the study of minds and their processes.

The essence of this theory of inference seem to be the following. We start with some complex of related terms. This may either be actual or merely supposed. The relating relation that characterises this complex will be such that each term in the complex is relevant to all (or, at any rate, to many of) the other terms. Such complexes are what Prof. Bosanquet means by universals. I may remark, in passing, that this explains, as I had long suspected, why Prof. Bosanquet asserts many propositions about universals which seem to people brought up on a different nomenclature to be patently false. What he says about universals is both true and important when the name is understood in his sense, and false

when it is understood in the sense of *abstractum*. The only ground of quarrel that remains is that he seems to deny that universals, in the latter sense, are also real and important; but this is a matter that concerns his large *Logic* rather than the present work. Still, even when it is understood that universals are to mean complexes, it seems to me that Prof. Bosanquet's theory requires universals in the sense of *abstracta*. For I imagine that what is important is, not some one particular complex, but the characteristic type of structure of all the possible complexes of a class. This, I think, is implied by the fact that what we should commonly call the same complex varies its terms and their relations, within limits, in determinate and interconnected ways. This is assumed by Prof. Bosanquet, and seems to imply a contrast between the permanent general type of structure—an universal in the sense of an *abstractum*—and the determinate complex as it is at a given moment (if it be in time) or distinct instances of it (if it be timeless, as in geometry), which are universals in Prof. Bosanquet's sense.

Implication is defined as the relation which subsists between one term or relation in such a complex and the rest, in so far as their respective modifications afford a clue to one another. The position then is that if one term or relation in a complex of a certain general structure varies (presumably within the limits required for the complex to remain of the same structure), there will be correlated variations in some or all of the other terms and relations. It appears from the definition that this state of affairs is not itself implication, but is only a precondition for it. For implication it is not enough that modifications in different parts of the complex should in fact be correlated, they must further be so correlated that one 'affords a clue to' the other. Prof. Bosanquet thus agrees so far with logicians of the Russell-Whitehead type as to regard implication as a relation between terms which subsist whether a mind recognises it or not. He differs in so far as they make implication a very special relation that holds only between *propositions*. It is doubtful whether this difference is very important. I take it that the connexion between the two senses of implication is this. The proposition that asserts that such and such a term or relation in a certain complex is modified in a certain way is connected by 'implication,' in the Russell-Whitehead sense, with a proposition asserting that some other term or relation undergoes a correlated variation. The connexions of the actual terms or relations in the complex, in virtue of which the two propositions imply each other in this sense, are 'implications' in Prof. Bosanquet's sense. Thus the connexion would seem to be that Prof. Bosanquet's implication is that relation within a factual complex which is the factual correlate of implication, in the Russell-Whitehead sense, between propositions about terms or relations within this complex.

We next come to Prof. Bosanquet's use of the word *inference*. This seems to be bound up with a special theory as to the precise way in which inferences are made. His view is the following, if I

have rightly understood him. It is a contradiction in terms to hold that no proposition is true, or even to doubt all propositions. But it is perfectly possible to deny or doubt this or that proposition. Inference consists in transferring the certainty which we have that there are *some* true propositions to the truth of *this or that* proposition. All arguments thus finally come down to the form: Either p is true or nothing is true. We start from the idea of some definite restricted complex, which may or may not be actual. Within this conclusions emerge whose rejection would 'shatter' the experienced world. Thus two wholes are involved in any inference:—the restricted complex with whose terms and relations you are explicitly concerned, and the total character of reality. Your conclusion is *based on* the restricted complex, but you can only *draw* the conclusion by 'applying the complex . . . to the reality which survives and transcends any modification introduced by the complex'. Another way of putting it is that we make a joint system of the restricted complex and the rest of reality and read off the implications from it.

All this is highly figurative, and it is necessary to discuss its precise cash value. (i) If Prof. Bosanquet's view of inference is to be taken literally he has replaced the categorical syllogism as the type of all inference by a certain mixed disjunctive syllogism. This syllogism has, in all arguments, ultimately the same proposition for the second alternative in the disjunction, *viz.*, the proposition: No propositions are true. The categorical premise is ultimately the same in all arguments, *viz.*, the denial of this alternative. (ii) This would seem to involve the view that one formal principle at least is recognised on its own merits as absolutely true, *viz.*, the proposition: If p or q , and not $\neg q$, then p . For, otherwise, a person might admit both that if p is false, everything is false, and that the latter is impossible, and yet refuse to admit that this implies p . (iii) It is also necessary to assume the non-formal principle of inference, first explicitly recognised by Frege. Otherwise we cannot pass from recognising that the premises imply the conclusion, and asserting the premises, to asserting the conclusion by itself. (It is no answer to this to remark that in fact p is connected with q , etc.; that therefore it is a fiction to say that we finally assert it by itself; and that there is thus no need of the non-formal principle to justify this procedure. This answer would rest on the fallacy of confusing the two statements: I assert that (p is true in isolation from the premises) and I assert p , in addition to asserting the premises and the implication. I do not assert the former; but, whenever I infer p , I do pass from merely asserting the premises and the implication to asserting p itself in addition. And this needs justification. I seem to have met traces of this confusion in Prof. Bosanquet's large *Logic*.) (iv) Prof. Bosanquet might urge that he does not make use of the *general* formal principle: If p or q , and not $\neg q$, then p . He might say that he only needs the more restricted principle, in which

q is, not *any* proposition, but the particular one that no propositions are true. And he might say that the restricted principle is justified on exactly the same grounds as justify the denial that no propositions are true. If so, a person would be inconsistent in accepting the categorical minor and denying the implication between the two premises and the conclusion. Let us consider this position. Prof. Bosanquet holds that to deny all truth is not merely practically barren but that it is logically refutable. Such scepticism contradicts itself. I do not see why this should move the sceptic, unless he voluntarily adopts the silly attitude of combining a belief in the law of contradiction with a denial that any proposition is true. Thus I conclude that Prof. Bosanquet must accept the law of contradiction on its own merits; for clearly it is circular to accept it on the ground that the rejection of it would be self-contradictory. But, if the ground for denying that all propositions are false be the law of contradiction, it cannot be maintained that we have the *same* ground for believing 'If p or q , and not $-q$, then p ' and for believing 'not $-q$,' where q is the proposition that there are no true propositions. For the law of contradiction alone will certainly not guarantee the former principle; since the law is only about any proposition and its contradictory, whilst the principle is concerned with pairs of propositions, of which the first may be about anything, and the second, being the statement that all propositions are false, is not in general the contradictory of the first.

It thus appears that, even if we accept Prof. Bosanquet's view that all inference in the end comes down to showing that unless p be true nothing can be true, a number of principles must be assumed simply on their own merits and not because of their coherence or lack of coherence with anything else. These include at least the law of contradiction, which is needed to guarantee the minor; the principle of the mixed disjunctive syllogism; and the non-formal principle of assertion. I do not really know whether Prof. Bosanquet needs to deny this. It is verbally indeed at variance with the general spirit of his writings; but it seems to me that he has only to say that such principles can be 'read off' from the system of reality as a whole, or from *any* subordinate system in it, whilst other kinds of implication are bound up, in part at least, with the particular structure of particular subordinate complexes. Possibly this is what he does mean, but I do not think he has made his position very clear on this matter.

But is it really true that all inference ultimately involves the disjunction: Either p is true or nothing is true? I find this very difficult to believe. I do not think Prof. Bosanquet can mean to assert that we could show in detail, *e.g.*, that unless the space of ordinary life had three dimensions Julius Cæsar would not have been bald. Thus he must be able to know in some *general* way that the falsity of a given proposition implies that of all propositions, without going into detail. What he appears to mean is that

we can show that if p were false some fundamental principle, such as the law of contradiction, which pervades all reality, would be false. We must remember that he is dealing with implications within subordinate systems, and so his view would be most fairly represented by putting it in the form: If the part B of the system S did not have the modification μ_b when the part A has the modification μ_a some all-pervasive and fundamental proposition about reality as a whole would be false, and, if this were so, nothing would be true. If this be the right interpretation his theory of inference contains two apparently separable steps: (i) the argument that if so and so were not true some fundamental proposition about reality would be false, and (ii) the argument that in this case nothing would be true. The two steps are not separately stated, and I cannot help suspecting that this covers one or both of two possible confusions. The first is this. It is possible to confuse (a) all propositions about reality, with (b) propositions about all reality. The laws of logic seem to be propositions of the latter kind, in the sense that they apply to and are true of reality as a whole and also any part of it. If (b) be confused with (a) the second step of the argument follows automatically from the first. I strongly suspect the presence of this confusion in several places. The other possible fallacy is this. The law of contradiction is supposed to guarantee in a special way that not all propositions are false. It is easy but fallacious to pass from this to the view that, if the law of contradiction were false, all propositions would be false. Now the law of contradiction is a fundamental and pervasive proposition of the kind mentioned in the first step of the argument. Hence a logical fallacy of the sort just mentioned would lead naturally to Prof. Bosanquet's result. The argument would be: 'If p is not true then the law of contradiction (e.g.) is not true, but if the law of contradiction is not true nothing is true. Hence if p is false nothing is true.' But at most what we know is that if the law of contradiction is true something is true; and it does not follow that if the law of contradiction is false nothing is true; all that would follow is that two contradictory propositions might both be true.¹

Thus I cannot see any reason to believe that all inference ultimately rests on the disjunctive: Either p is true or nothing is true. And this distresses me the less because it is gravely doubtful whether the statement, *Nothing is true*, is either true or false. In actual fact this set of noises or marks does not stand for any proposition at all; for the theory of logical types condemns such expressions as meaningless. Thus it would be unfortunate if all inference really did depend on a disjunction, the second member of which is not a proposition at all, but a mere noise like *Jabberwocky*.

I am strongly inclined to think, therefore, that all that Prof.

¹ Doubtless if the law of contradiction were false we could not know, of any other proposition, whether it were true or false. But this is very different from knowing that all other propositions are false.

Bosanquet really means is that all inference involves in the end an argument of the form: If p were false then some proposition which is true of the whole of reality would be false. I am compelled to regard his actual statement that, if p were false, then no proposition would be true, as either a rhetorical expression of this or a mistaken inference from it.

I am encouraged in this belief by the fact that nothing stronger than my statement of his position seems to emerge from the interesting discussion in Chapter VIII. on supposition and the views of Mr. Leonard Russell. Prof. Bosanquet's argument here is that, however much you may explicitly suppose, you can do nothing with your suppositions in the way of drawing conclusions unless you *assert* (and not merely *suppose*) the law of contradiction (and presumably other principles of pure logic). And these are laws pervading all reality. He also argues that when the conditions of a judgment are once made explicit 'it is absolute in its challenge to reality'. 'Its truth then depends on the absence of hidden obstructions in the universe of unknown reality.' Thus, here at any rate, the connexion with reality that is demanded is not: 'This true, or *nothing* true,' but: 'This true, granted the absence of obstacles, or *the laws of logic* would be false'.

Prof. Bosanquet seems to me to be right in his main contention about supposition. Stated in terms of Mr. Bradley's distinction between premises and principles, the first part of his view might be expressed as follows: Your *premises* may be merely supposed for the sake of argument, but, if you are going to make any use of them, your *principles* of inference must be not merely supposed but asserted. This seems to me to be true; and it is not in any way altered by the fact that all the conclusions reached in systems of supposals are hypothetical. No doubt they are; *e.g.*, the propositions of any system of pure geometry are hypotheticals with the postulates as antecedents. Nevertheless there is assertion; for these hypotheticals themselves are not merely supposed but are positively asserted. It is their antecedents and consequents, taken separately, that are only supposed and not asserted. And you clearly cannot get assertion (even of hypotheticals) out at the end, if nothing but supposition is put in at the beginning. This I take to be what Prof. Bosanquet has in mind when he says that a judgment, whose conditions are once made explicit, is absolute in its challenge to reality. His second contention about supposition is less clear to me. If, in a certain system of suppositions (say, the postulates of Euclidean geometry), I can prove by the laws of logic that the postulates imply a certain proposition, *e.g.*, I. 47, what 'hidden obstacle' can there be in the rest of reality to prevent the hypothetical: 'The postulates imply I. 47,' from being true? No doubt there might be 'hidden obstacles' (*e.g.*, the non-Euclidean character of physical space, if this has any definite meaning and were a fact) to the truth of the postulates or of I. 47, taken separately. But, so long as I recognise that the postulates are only supposed, I

am not attempting to assert them or to assert I. 47. And such obstacles are perfectly irrelevant to the truth of all that I do assert, viz., that the postulates imply I. 47. No doubt Prof. Bosanquet is here thinking of much more concrete systems of supposals, as his example about driving to Hampstead shows. It is obviously true that much more has to be supposed in such cases than is ever made explicit, *e.g.*, the constancy, in the main, of the ordinary laws of the material and mental worlds. The result is that your hypotheticals *are* only true subject to conditions that are not contained in their explicit antecedents. But I do not see why Mr. Leonard Russell's remedy, of supposing more, will not work here; though I do see that it will not do as a substitute for actually asserting the principles of inference.

We have now discussed Prof. Bosanquet's views about supposition, and have tried to understand what he means by implication and by inference. And we have argued that his statements about the latter must be interpreted in a much restricted sense if they are to be plausible. We have not yet seen clearly how he supposes inference—defined as the process of conferring the certainty that there is *some* truth on to some definite proposition—to be connected with implication in his sense of the word. The chief source of information on this point is Chapter IV., where Prof. Bosanquet gives a large number of examples, ranging from mathematical proofs, through inferences about social and physiological matters, to judgments of value. The principle, he says, is the same throughout as that which is involved when we argue that $2 \times 2 = 4$ or that equilateral triangles are equiangular. It is that, 'within any complex of terms and relations which is distinctly before our apprehension, connexions can be seen as between antecedents and consequents, which are necessary and relatively *a priori* so long as that complex is assumed'. In Chapter I. it is said that, even if there be no process of argument (as where we convince ourselves that two straight lines cannot enclose a space) there is something like inference. But more usually we have to 'build up the system' and then 'read off' the implications. Apparently inference is the act of reading off the implications after scrutinising the system that we are dealing with and viewing it in relation to the wider system of reality as a whole. If we can 'read off' straight away the inference is immediate; if we have first to build up the system before being able to read off the implications it will be mediate. *All* implication, we are told, is logically *a priori*. The question of our actual degree of certainty in any case depends on the distinctness of the structure of the subordinate system in itself and in its relations with the ultimate whole. Those propositions to which the name *a priori* is commonly confined are propositions in which the simplicity and abstractness of the relation concerned makes it specially easy to read off the connexion. But we are told that, in other cases,—notably religious and moral matters—what is lost in simplicity and abstractness may be gained in depth.

I must confess that I have considerable difficulty in seeing the precise connexion between the various parts of this theory. Let us begin with the statement that all implication is logically *a priori*. So far as I can see *a priori* is never defined. But evidently propositions like $2 \times 2 = 4$ are supposed to be examples where it is so clearly present that it is falsely thought by most people to belong only to them. Now I suppose that the peculiarity of such propositions is that, when you understand and reflect upon the terms and their relation, you see in the end that those terms must be related by that relation. And we do not seem to see anything of the kind when we reflect upon humanity and mortality, or on silver, increase of temperature, and increase of length. Now I take it that *part*, at any rate, of what Prof. Bosanquet wishes to assert is that this is only a difference *quoad nos*, and not a difference in logical character of the propositions $2 \times 2 = 4$ and Silver expands when heated. The subjects and predicates of propositions which are only proved by induction are bound together in reality in just the same way as those of 'self-evident' propositions. It is only a difference in the complexity of the subject-matter or some peculiarity of our place in the world that makes the apparent difference between them. This, I think, is certainly part of Prof. Bosanquet's contention; and this seems to be borne out by his rejection (in Chapter IV.) of the Leibnitzian distinction between laws that hold in all possible worlds and laws of the actual world.

Now this may perfectly well be true. I am quite sure that we always *mean* by a law of nature, such as All S is P, something more than the merely numerical proposition that 100 per cent. of S's are P's. It seems to me possible that laws of nature assert connexions between certain *bundles* of universals (in my sense, not Prof. Bosanquet's) and other universals or bundles of them; whilst laws that appear to be specially *a priori* assert connexions of precisely the same logical kind between single universals or very small bundles of them. On this view the contingency of the laws of nature does not depend on anything peculiar in the *connexion* asserted, but on the fact that it is contingent that there should be instances of just these bundles. Man and silver are highly complex bundles of universals, and it is contingent that there should be instances in great numbers of just these bundles; but it may be that the connexion between such bundles and mortality or expansion on rise of temperature is as necessary as that between 2×2 and 4. If Prof. Bosanquet means something of this kind I am not indeed sure that he is right, for I find the whole subject excessively difficult to make up my mind about; but I think he very well may be right.

But clearly, even if this be part of what he means to assert, he means much more than this. His theory is not only or mainly about the nature of the propositions which we finally reach, but about the processes by which we reach them and the attitude that we finally take towards them. And here I do feel difficulties. We

do 'read off' laws from certain systems after 'applying to them' the rest of reality and viewing them in their relation to the latter. I can ascribe meanings to this both in the case of mathematical reasoning, and in the case of induction; but unfortunately they do not seem to be the same meaning. In what is ordinarily called a purely deductive argument the system that we start with is defined by the postulates. A system of pure geometry, treated by the method of Veblen or of Whitehead, in his *Mathematical Concepts of the Material World*, seems to bear a very close resemblance to Prof. Bosanquet's partial system. We start with one fundamental relation (e.g., between) and lay down its properties in our fundamental postulates. Then we 'apply' the rest of reality to it, in the definite sense that we argue from these postulates by using the laws of logic, which are laws of all reality. And there is a perfectly clear sense in which we may be said to 'read off' our results from the joint system composed of our partial system and the rest of reality. We do see the connexion between the postulates and the propositions of the system of geometry by viewing the two in their logical interrelations under the guidance of the laws of logic; and this vision in the end is a kind of immediate insight, however much mediation may have been needed to put us into a position to see these connexions.

Now let us consider inductive arguments. With nearly all that Prof. Bosanquet says in his many excellent examples I agree heartily. Induction by simple enumeration, eked out to the maximum possible extent by the laws of probability, is, I agree, worthless. And, in the hypothetical method, we do not put forward hypotheses *in vacuo*, but start with very definite views as to what types of hypotheses are admissible in a given subject-matter. This restriction is based on what we believe ourselves to know about the general 'make up' of the physical world and of the special peculiarities of the region of phenomena under discussion. Anyone can see this for himself, e.g., who compares Mill's Methods, as offered, with the actual processes of argument that one uses in a chemical or physical laboratory. And the case is strengthened by negative instances. Why is Psychical Research so excessively difficult and unsatisfactory? Because we have at present no idea what hypotheses are reasonable and what are not, as Mr. Bradley pointed out in his classical article on the subject many years ago. Psychical Research illustrates another important contention of Prof. Bosanquet's extremely well. He says that in most inductions you cannot make a sharp separation between the particular facts of observation and the general structure of the system under investigation. Now this is exactly illustrated by Psychical Research. Everybody accepts observations by competent physicists, even if they seem to be startling and revolutionary. We feel sure that they will fit into the system somehow and that the methods used are appropriate to the investigation of physical systems. But people view even the alleged *facts* of Psychical Research with a

scepticism that they never think of applying to ordinary physical phenomena. No one makes it an objection to photography that, at a certain stage, the operator goes and muddles about in a highly suspicious way in a dark room into which nothing but red light, and not much of that, is admitted. But we feel that precisely the same circumstance makes very much against Dr. Crawford's experiments with Miss Golligher on telekinesis. The reason is obvious. Photography does and telekinesis does not fit in with our view as to the general 'make up' of nature; we understand why a plate must be developed in red light and semi-darkness; we do not see why Miss Golligher could not move tables without contact under an arc lamp. [I ought perhaps to add that I use these illustrations simply to bring out Prof. Bosanquet's points, and not to condemn Psychical Research. Its special difficulties ought to act as a challenge to scientists; and I have not the slightest sympathy with the ignorant pontifications of biologists of the Ray Lankester type, or with the more respectable but to my mind equally mistaken objections of the Dean of St. Paul's, who seems to think it a sufficient reason for not pursuing the subject that mediums are often of doubtful moral character, that the results do not point to a particularly cheerful or desirable kind of future life, and that psychical research (in common, I may remark, with alcohol, tobacco, politics, and religion) is liable to have bad effects on persons of weak nerves and intellects.]

The sense then in which we view our partial system in its relations with the whole and read off our implications from the joint system is, in inductive matters, the following. We believe ourselves to know a good deal about the 'ground-plan' of the region of phenomena,—electricity, life, etc.—of which we are investigating a particular instance. We also believe ourselves to have a sound general knowledge of the ground-plan of the physical world as a whole, of which this special region forms a part. This knowledge suggests hypotheses to us and limits very greatly the hypotheses that seem worth consideration. And, unless this were so, induction would be impossible. Now Prof. Bosanquet evidently regards *this* process of forming a joint system and reading off implications as being essentially the same as that which we have already illustrated in purely deductive sciences like pure geometry. I should very much like to believe that he is right, but I do not feel at all certain that he is. Most of the supposed knowledge, that is always at the back of our minds, about the general ground-plan of our particular region and of nature as a whole, has been handed down to us from earlier investigators. No doubt each generation has added something to it, and we may hope to pass it on to our scientific descendants in a slightly purer and deeper form. But I cannot see that ultimately it rests upon anything but induction by simple enumeration, or that now or at any past stage it has had a trace of the self-evidence of the laws of logic or of pure mathematics. I grant at once that at no stage have men in fact

argued explicitly from simple enumeration alone or put forward hypotheses freely *in vacuo*. But I should suppose that the cause of this is that, long before scientific investigation started, and probably in a pre-human stage, fixed ideas (though very crude ones) as to the general ground-plan of nature were formed in us by the actual preference which nature has so far shown for a comparatively few kinds of substances and for a comparatively regular behaviour. I am not making the mistake of arguing that because our beliefs started in this way they are false. All that I assert is (a) that this seems sufficient to explain how we have come to be in a position to make inductions, and (b) that when we come to scrutinise the results from a logical point of view we can see no self-evidence in them, and no evidence for them except induction by simple enumeration, concerning the weakness of which I entirely agree with Prof. Bosanquet.

I suppose that Prof. Bosanquet would answer that you cannot expect these more concrete laws to get the same clear self-evidence as the highly abstract principles of logic and mathematics, but that what they lack in clearness they gain in depth. I am not at all sure that I understand this doctrine; which also appears in the earlier chapters of Mr. Bradley's *Essays on Truth and Reality*. In some places Prof. Bosanquet seems to argue that mathematical propositions gain their clearness from the fact that to deny them involves almost at once a denial of the law of contradiction. But obviously this cannot account for the kind of certainty possessed by the law of contradiction itself, or by those other principles of formal logic which have to be used in proving that the denial of any given proposition *p* (other than the law of contradiction) would lead to a denial of the law of contradiction. Yet this is precisely the kind of certainty that mathematical axioms themselves seem to possess. Since then there is a whole class of propositions which are certain, and whose certainty *cannot* be due simply to the fact that the denial of them leads in a few steps to a contradiction; and since mathematical axioms seem to possess exactly the same kind of certainty as these propositions; it seems doubtful whether the certainty even of the latter can be explained as Prof. Bosanquet claims to explain it. As to the other propositions, whose denial does not lead immediately to contradictions, but to the rejection of something 'deeply rooted' in reality, I am not quite sure what is to be said, because I do not clearly understand the phrase 'deeply rooted,' which is of course metaphorical. Does it simply mean that we should have to reject a great many interconnected propositions that we do not think of doubting? If so, the question will be: Do these propositions, however numerous and closely connected, seem to be so obviously true that it is impossible to reject them? After all, there are wide systems of mutually confirmatory errors; it would be highly uncomfortable to have to give them up, but this is not a logical ground in their favour. Or do 'depth' and 'rootedness' have some other meaning? I suspect the moralistic flavour

of these phrases, and cannot help wondering whether it may not lead to the fallacy of supposing that, because certain propositions have ethical value, they must *ipso facto* have truth-value too.

It may be said that all the criticisms that we have passed on Prof. Bosanquet's theory of inference are really refuted by him in Chapters II. and III., where he deals with the linear conception of inference. Let us now turn to this subject. Prof. Bosanquet holds that the people who think the syllogism the only or the main type of deduction, and the people who base induction on simple enumeration, commit two forms of a single fallacy.

(i) *Syllogism as the type of all deductive reasoning.* Prof. Bosanquet makes two objections. (a) The syllogism will not allow you to particularise your predicate as you learn more about the details of the subject. Yet we constantly do this in deductive arguments. He instances the deduction of the moon's motions from the law of gravitation. Here, I think, he is certainly right. But he does not tell us in detail what the line of argument is here. It is therefore possible that he is right in denying that it is syllogistic and wrong in holding it to be an instance of inference in his sense. It will be worth while then to consider what we really do when we deduce the moon's motions from the law of gravitation. In the first place, I suppose that the argument does have one characteristic that Prof. Bosanquet objects to in the syllogism; it does use a 'borrowed premise'. A person working out the moon's motions does now take over the law of gravitation as an otherwise established fact, and does not 'read it off' from the moon's motions. We also 'borrow' the laws of motion, the laws about the composition and resolution of forces, and the rules of arithmetic, algebra, and the calculus. The peculiarity of the reasoning is that one premise, the law of gravitation, states a quantitative relation between *any* values of three sorts of variables, viz., any pair of masses, any distance, and the mutual accelerations. The 'predicate' is able to be 'particularised' in accordance with the special character of the 'subject' because the premise is about the *correlated values* of sets of variables; whilst, in the ordinary syllogism with a major like All M is P, the major is simply a statement about a *conjunction* of attributes, with no information about their correlated values.

The principle of reasoning used is that what is true for *any* values of a set of variables is true for definite given values, such as the masses of the moon and earth, and their distance. The detailed deduction is carried out by using the laws of motion, themselves propositions of the same peculiar kind as the law of gravitation; and arguing in accordance with the laws of pure mathematics. Certainly this is not syllogism, but it seems equally unlike inference in Prof. Bosanquet's sense. It uses borrowed premises; it argues in accordance with self-evident formal principles; and—in so far as one can talk about subjects and predicates at all—the principle according to which the predicate is varied to fit the variations of the subject is part of the content of the premises.

How is this process related to syllogism? Prof. Bosanquet quite truly says that it is a mistake to suppose that the formal principle of the syllogism is used as a premise in particular syllogisms. He is also quite right in saying that in any particular syllogism we can see the connexion between premises and conclusion directly without appeal to such a formula as $MaP . SaM . \therefore SaP$. Thus the formal syllogism in Barbara is in general used neither as a premise nor as a principle for any particular syllogism in Barbara. Yet there is a certain analogy and a certain difference between the process of argument in the syllogism and in the determination of the moon's motions from the law of gravitation. The analogy is this. In the argument about the moon's motion an essential step is to substitute definite particular values for the variable masses and distances whose mode of correlation is stated in the law of gravitation. In the syllogism to prove that Socrates is mortal from the fact that all men are so it is indeed neither necessary nor as a rule desirable to appeal to the general formula $MaP . SaM . \therefore SaP$. It is not necessary, because the particular case is as evident as the general formula; it is not desirable, because to most men the general formula is less evident than the special case. Nevertheless, if we were asked: Why is the argument about Socrates valid? our natural reply would be that it is so because the argument is of the form $MaP . SaM . \therefore SaP$. And by this we mean that it is got by substituting the particular values man, mortal, and Socrates for the variables M, P, and S respectively in the general law. So far there is analogy. But, if we look more closely, we shall also find difference. The law of gravitation is just a premise, not a logical principle of reasoning. Substitution of constant values for the variables in it is a step in the reasoning. But the complete formal Barbara is not a premise for the conclusion that Socrates is mortal; it is a principle exemplified by the particular syllogism about Socrates. Thus the function of substituting constants for variables is quite different in the two cases. In the argument about the moon's motion it is a step that actually has to be performed in the course of the proof if the conclusion is to be reached. In the syllogism about Socrates it is not a step in the proof but an additional statement, which may or may not be made, *about* the proof.

(b) Prof. Bosanquet's other objection to the syllogism is that it borrows its premise by assumption or from a previous argument. We have just seen that this is not a *special* objection to the syllogism. Surely every argument of any complexity has to borrow premises from many sources. It seems to me, *e.g.*, that every particular application of induction borrows a whole view of the general make-up of the physical world. Surely Prof. Bosanquet does not maintain that, whenever we make an induction, we see with direct insight for ourselves that general ground-plan of nature, which he rightly insists that we use, but which has certainly been handed down to us from the reflexions and inductions of previous scientists.

(ii) *Induction and the Syllogism.* Prof. Bosanquet's reason for holding that the current theory of induction involves the same fallacy of linear inference as the current theory of deduction seems to be as follows. This theory of induction starts by observing conjunctions between particulars A_1 and B_1 , A_2 and B_2 , . . . A_n and B_n . It then formulates a general law connecting A with B . And it then professes that all further anticipation about future A 's and B 's or inference about past or distant ones is made by subsumption under this law. The law, supposed to be established by induction by simple enumeration, is thus simply 'borrowed' in dealing with any subsequent particular case, and the particular case is subsumed under it. Prof. Bosanquet's alternative is well summed up in the following quotation: 'The difference is that between going from a presupposed connexion to a new case taken to fall under it, and determining a conclusion from a system of relations which, *in the moment of determination*, is apprehended as making it inevitable' [p. 24. My italics]. So far, the connexion between traditional induction and linear inference seems to consist, not in an analogy between these processes themselves, but in the fact that the results of induction are used as major premises for syllogisms. Any view of induction that makes it establish general laws, by whatever means, might lead to this result over the subsequent application of the laws. But Prof. Bosanquet means to assert more than this; he thinks that the *special* theory that induction proceeds by analogy or simple enumeration reduces induction itself to linear inference. Moreover, the quotation just given strongly suggests that he does not think that the object of induction is to establish general laws, but rather to exhibit directly the connexion of particular cases. If this be so the subsequent linear application of inductive results *may* fairly be laid at the door of the traditional theory of induction itself; for, if that did not (erroneously, as Prof. Bosanquet would hold) claim primarily to establish general laws, it would be under no temptation to be lured afterwards into syllogism and linear inference.

Now I must confess that it seems to me perfectly clear that induction—and not any special theory about it—does claim to establish general laws and not to offer direct insight into special cases. And it seems to me equally clear that we do afterwards 'borrow' these laws and use them deductively for dealing with special cases. The sciences of physics and chemistry, *e.g.*, are full of general laws such as Maxwell's Equations, Ampère's Rule, Lenz's Law, the Laws of Reflexion and Refraction, and so on *ad nauseam*. And in dealing with special cases we do 'borrow' these laws and argue deductively from them and the special values of the relevant variables which determine our special cases. The example discussed above about the law of gravitation and the moon's motions seems adequately to illustrate this. I admit that no theory of induction that I am acquainted with seems to me to explain satisfactorily how induction justifies our strong belief in such laws.

But it does seem to me (a) that if they be not somehow established by induction they are not established at all, and (b) that if induction does not somehow establish such laws it does nothing.

Having said so much I shall be asked how I propose to meet Prof. Bosanquet's criticisms. I meet them largely by agreement and partly by drawing certain distinctions. (a) The laws are not used as major premises of syllogisms, but in the way illustrated in the discussion of the moon's motions. Thus the objection that syllogism will not modify 'predicates' to fit the special peculiarities of special 'subjects' becomes irrelevant. (b) This peculiar *use* of the laws is rendered possible by their special *nature*. They assert, not mere conjunction or disjunction of attributes, but correlations between the values of certain sets of variables. (c) Such laws evidently cannot be established by the mere noticing of likeness and difference. Thus all that Prof. Bosanquet has to say against this as the fundamental process of induction has my hearty agreement. They are established by something like the Method of Concomitant Variations. This method, the only one of Mill's which really is inductive and does not simply consist of mixed hypothetical syllogisms with the definition of cause and effect as the hypothetical major, is *the* method of all advanced sciences. (Mill's account of it, as a weakened form of Difference is of course preposterous; if the two are to be compared at all Difference should be regarded as a very special case of Concomitant Variation.) There is a genuine connexion between the induction that only argues by analogy and the linear inference that can only use syllogism. The connexion is that induction which only proceeds by likeness and difference can at most establish laws of the mere conjunction or disjunction of attributes, and no use can be made of such laws except as majors for syllogisms. But there *are* other kinds of laws, and these are reached by another kind of induction, and can be used as premises for another kind of deduction. Lastly (d) there no doubt is such a thing as that immediate insight into special cases and special regions of which Prof. Bosanquet makes so much. The argument about $B > C, A > B, \therefore A > C$ is seen to be true with the same certainty as $MaP, SaM, \therefore SaP$. It is therefore futile, even if it be possible, to throw such arguments into syllogistic form. The difference between such arguments and, *e.g.*, syllogisms can be put quite simply and clearly. The relations which occur in the premises and conclusion of a syllogism are *logical* relations, *e.g.*, that of class-inclusion. The relations that occur in the argument about A, B, and C are not logical relations, because the entities that they relate are of a more special kind, *viz.*, magnitudes. The distinction between these relations which are and those which are not logical relations depends simply on the abstractness and generality of their fields; classes are abstract enough to form part of the subject-matter of pure logic, magnitudes are too special to do this. But *all* relations, whether themselves logical relations or not, have logical *properties*.

And the relation $>$ has the same sort of logical properties as that of class-inclusion. Wherever the logical properties of a relation or set of relations can be clearly recognised, and seem to justify an inference, we can make the inference; and it is a matter of perfect indifference whether the relation itself be or be not a logical relation. The desire to reduce every argument to a syllogism depends on two equally baseless superstitions: (a) that only logical relations have logical properties, and (b) that no logical relation except that of class-inclusion has the logical properties needed for inference. But, granting all this, I believe that the cases where we can 'determine a conclusion from a system of' (non-logical) relations which, in the moment of determination, is apprehended as making it inevitable' are comparatively few and simple. Prof. Bosanquet admits and asserts that we do not, as a rule, 'read off' the connexions simply from the partial system under investigation. We have to view it in the light of our knowledge of the make-up of nature as a whole. But exactly how that knowledge arose and exactly how it operates in a given case he does not in detail tell us. To me it seems clear that it is not 'apprehended in the moment of determination,' but is 'borrowed' from the past researches of ourselves and our scientific ancestors; and that we do not 'read off' our results by merely gazing at it and our partial system, but reach them by definite processes of deductive reasoning, which, though not syllogistic, rest upon formal principles that can be elicited and stated.

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The Intuitive Basis of Knowledge. By N. O. LOSSKY. Authorised translation by NATHALIE A. DUDDINGTON, M.A. Preface by Prof. G. DAWES HICKS. Macmillan. Pp. xxix, 420.

THE translation of this important work of a distinguished Russian realist has been ably performed by Mrs. Duddington, and Prof. Dawes Hicks supplies an appreciative, though critical, introduction. The sole faults that can be found with the translation are in connexion with certain chemical terms. On pp. 74 and 297, where Prof. Lossky is made to speak of *chlorate*, I think it is pretty certain that *chloride* is meant. And on the latter page the expression *sulphurate of calcium* is used for what an English chemist would call *calcium sulphate*.

In the Introduction it is pointed out that, whilst we are most of us realists (at least as regards the material world) in ordinary life, philosophic study in most cases leads to something very much like subjective idealism or pure agnosticism. It is suggested that this is because philosophers, in studying knowledge, have usually taken over in an uncritical way categories like substance, cause, etc., which they daily use successfully in dealing with the material world, and have tried to force the relation between minds and their objects into these moulds. This accusation is made more detailed in the first