

VII.—NEW BOOKS.

Encyclopædia of the Philosophical Sciences. Vol. I., *Logic.* Pp. x, 269. Macmillan.

THIS volume is the first of a series to be issued under the editorship of Sir Henry Jones and Arnold Ruge. The translation of the articles has been well done by B. Ethel Meyer. In the absence of the originals the only criticism that I have to make on her work is that 'Natrium' in Loskij's article is not an English word, but is the German for Sodium. There is also either a misprint or a bad grammatical mistake on page 61.

The book opens with an introduction by Ruge who contrasts and compares the scheme of the series with that of Hegel's *Encyclopædia*. In the existing state of knowledge, he says, we can only expect contributions from various thinkers based on the present condition of the particular sciences, not a complete account of the nature of Reality from a single philosopher. The contributors to this volume are Windelband, Royce, Couturat, Croce, Enríques, and Loskij. All the articles except Croce's have merit, but I do not think that any greatly advances the subject; and the scheme seems to me to suffer from the defect that no writer has space to offer as full an account of his own point of view as he could give (and often has given) in his own works. Much the most interesting contribution seems to me to be Royce's, who alone ventures to say much about induction.

Windelband begins by tracing the relation of Logic to the special sciences, to psychology—descriptive and genetic—and to language. His conclusion is that Logic must take the results and methods of the sciences as in the main sound, but must criticise and compare them. The only connexion with psychology is that unless we have a definite psychological terminology we cannot state unambiguously what kind of mental states are capable of truth or falsehood. The connexion with language is that truth claims to be valid for all men, that this introduces a social reference and so necessitates a definite view about the possibility of unambiguous communication of judgments. He seems to hold that the coherence theory of truth is the one that we must actually use as our test, but that at every stage there lurks behind it a notion of correspondence. I would prefer to say that we all know that coherence is not what we *mean* by truth, but also know that with certain presuppositions it is a good test for it. As to the question of correspondence Windelband says that the relation between the content of valid thought and what exists need not be the same in all sciences. He adopts Lotze's expression of *valid* to describe the mode of being of relations and universals, and holds that these do not exist but are 'the form and order under which what exists is determined'. He then adds that if you insist on ascribing being to such an order you will have to conceive it either as an unknowable thing-in-itself or as psychical. He offers no reasons that I can see for the first alternative. I suppose that he must base his opinion here on some such argument as that of the *Parmenides*; his argument in support of the view that you will have to take relations and universals as psychical seems to be that they only become actual in one sense when actually

thought about. But since he admits that in another sense they are entirely independent of any one's opinions, and that the mind that would have to be assumed is utterly different from ours, I do not see why he should think that people must come to this conclusion which he himself rejects. With regard to the truth of the sciences as a whole his view is that, though we are not directly acquainted in perception with the real world, yet the special sciences do give us genuine knowledge, as far as they go, about fragments of it.

Windelband argues that the Laws of Thought are actual laws of the real world, and that they only have their sense of 'ought' as regards fallible thinkers. This seems to me true, but I cannot follow him in some of his applications of the view. He says, for instance, that in Probability we go against the Law of Sufficient Reason because we there assert without a sufficient ground. But what we really do is not to assert something without sufficient ground, but to assert with sufficient ground that this something has such and such a probability. How otherwise could we talk of justifiable and unjustifiable assertions about probability? There are many other points in the article which might be criticised if space allowed.

Couturat's article on symbolic logic is, I think, rather disappointing. A modern treatment of the subject should certainly tell us more of the doctrine of types, and his definition of the identity of two individuals sins against this doctrine by introducing the notion of all functions. I also seem to detect some confusions. We are told that a judgment is an assertion of a fact; it is true if the fact is real, false if it does not exist. But neither Couturat nor any of the other contributors enter into the difficult question of what false judgments are really about, which is as old as Plato and has been the subject of valuable work in recent years by Meinong, Stout, Russell, and others. On page 149 Couturat suddenly introduces the notions of *the true* and *the false*, and talks of their implications. But he has previously been talking of propositions and their implications; now the true and the false are not propositions but values of them, and he ought surely to give a new definition of implication here or some justification for still using the old one. On the same page there seems to be a confusion between the senses of value. He says that propositions can only have two values (true or false) whilst functions can have an indefinite number. Surely there is no analogy between the truth of a proposition and a constant value of a function.

Couturat connects probability with functions, as distinct from propositions, and defines it as the ratio of the number of values for which the function is true to the number for which it is significant. But surely this cannot be the whole meaning of probability, since the definition is only plausible if you add that all the values are equally probable, and so the definition itself involves the notion to be defined. Neither do Couturat's grounds for denying that probability can apply to propositions (*viz.*, the fact that every proposition is either true or false) seem to me at all conclusive. On page 161 I must note the bad misprint of $< \phi$ for $< \phi$.

Royce's article is an attempt to exhibit Logic as a science of order. It begins with what seems to me a very excellent account of inductive reasoning. Inductive generalisation cannot depend on such principles as the Uniformity of Nature or the Principle of Sufficient Reason; because these are *general* laws, whilst we know that in particular cases we can generalise and in others not, and the question how far generalisation in a given sphere can be trusted has to be left to the experts in that sphere. I agree with Royce's conclusion here, but I am doubtful as to the validity of his argument. If it be possible to give a general account

of induction at all it must rest on a general principle: his own account does this, though his principle is a law of logic not of the empirical world. His own theory assumes (1) a finite and determinate range of objects, and (2) the notion of 'a fair sample'; but (3) it does not assume laws of nature. If we define a fair sample as one chosen with no special motive it can be proved that more of such samples will closely resemble the whole in composition than not. Hence if you judge the whole from the sample you will be much more often nearly right than not. And the advantage of the expert is that he knows what is a fair sample in his field of work.

It is to be noted that here the definition of a fair sample must have shifted; it was originally defined as one chosen with no ulterior motive, but increasing knowledge of a given sphere will not make you less likely to have ulterior motives in your choice of samples. Royce then applies this principle to the justification of hypothetico-deductive theories. Their advantages are (a) that the innumerable mathematical results offer a vast field of samples, and (b) the complete definiteness of the concepts used makes the agreement or disagreement of an empirical sample with a predicted result absolutely determinate. I do not think that Royce sees one difficulty that seems to me serious. It is this. The number of results deducible from a mathematical theory is infinite. The number of observable samples is finite. But his original argument rested on the assumption of a limited region to choose from. Does the observed agreement with the results of theory, however far-reaching, really then add appreciably to the probability of the theory without some further assumption?

So far Royce's results have only been connected with order in that it is the order and law of the system of mathematical concepts that make the hypothetico-deductive method so valuable. He next goes into the question of conceptual order more thoroughly for its own sake. There is much here that I should like to criticise if I had space. His difficulty seems to be that, whilst some logical concepts, *e.g.* class, are necessary, in that they are asserted in the act of trying to deny them, others are only suggested by experience. He wants to be able to found all logic and mathematics (*plus* the innumerable non-quantitative sciences of order that he foresees) on purely necessary concepts. And he thinks that this can be done by the development of Kempe's Theory which he made in a paper some years ago. Here he hardly has space to make his theory plausible; I certainly cannot see how logical concepts can be put in terms of acts of rational choice, which I should have thought presupposed them. Those who are interested in Kempe's own theory which is purely logical may be referred to the last volume of Schröder where it is fully stated and discussed.

There are only two points that I need mention about Enriques' contribution. (1) He objects to Peano's distinction between the two kinds of syllogism in Barbara. He says that in the syllogism 'The apostles are 12, Peter and Paul are apostles, ∴ Peter and Paul are 12,' what alters is not the copula but the middle term, which is the class in the major premise and the abstractum of the class in the minor. But, even so, I should have thought that the relation between a sub-class and a class that contains it would probably be different from that between an individual and an abstractum of a class, which would be all that Peano would need. (2) He seems to think that the applicability of the laws of logic to the existent changing world is not absolute, but depends on the fact that many things change very slowly. Surely this is absolutely irrelevant. If nothing in the empirical world were the same at any two moments of time the laws of logic would equally apply to it.

Loskij's article is a plea for Realism. It seems singularly alive to us, since he has evidently not heard of the English and American movement in this direction that has been going on for so long now. It is more curious that he does not seem to know of Meinong and his school. He says that the relation of subject and predicate is one of ground and consequent, and is always necessary. In judgments of preception like 'This rose is red,' based on analysing a perceived complex, we do not see the necessity because we fail to see all the intermediate links which are apparently infinite in number. As this makes *all* propositions necessary and as he does not tell us what he means by that word, these results need not greatly disturb us. We are also told that, since logical laws are laws of the object, and since thought merely recognises them, thought cannot go wrong. It is only the substitution of 'fancy' for it that leads to error. Unfortunately no explanation is offered of why we fancy that fancy is thought in such cases.

Finally it is my unpleasant duty to express surprise that an article so offensive in tone as Croce's was included in this book without emendation. No one is under any obligation to read or understand symbolic logic, but, if he cannot do so, he should speak with modesty of distinguished workers in another sphere. To present in a patronising way a travesty of the methods and results of such men as Frege, Peano, and Russell; to refer to them *de haut en bas* as 'deserving authors'; and to congratulate oneself on the habit of a 'decent and comprehensible' mode of expression;—these impertinences can only cover a writer with deserved ridicule, and are singularly tactless in view of the logical leanings of at least three of the other contributors.

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Proceedings of the Aristotelian Society, 1912-13. Williams & Norgate.
Pp. 375.

The thirteenth volume of the new series of *Proceedings* of this Society opens with a paper on the "Notion of Cause," by Mr. Russell. Bergson comes in for a full share of discussion, points in his philosophy being treated by Miss Costelloe ("What Bergson Means by 'Interpenetration'"), Miss Stebbing ("The Notion of Truth in Bergson's *Theory of Knowledge*"), and Prof. Robinson ("Memory and Consciousness"). There are two papers on volition: "The Nature of Willing," by Dr. Dawes Hicks, and "The Analysis of Volition," by Prof. Hoernlé. Prof. Hoernlé also contributes to a symposium together with Prof. Stout and Mr. Barker on the question: Can there be anything Obscure or Implicit in a Mental State? Miss Jones deals with Dr. Mercier's Logic, Dr. Wolf with the Philosophy of Probability; and there are papers on "Purpose and Evolution," by Mr. Lynch, on "Intuitionist Thinking," by Prof. Granger, and on "Kant's Transcendental Aesthetic," by Mr. Carlile. There is also a short abstract of a paper by Prof. Jacks on "Does Consciousness Evolve?"

Mr. Russell's paper severely criticises the current notions held by philosophers as to what scientists mean by the Law of Causation. He points out that necessity has a special reference to propositions considered as values of propositional function which are true for all permissible values of some variable. He then discusses the difficulties introduced into ordinary notions of causation by recognising (*a*) that there are no 'next' events, and (*b*) that to recur an event must be more or less abstract; and points out the many errors that have sprung from assimilating causation to human volition. What the advanced sciences