

V.—SOME CONTROVERTED POINTS IN SYMBOLIC LOGIC.

By A. T. SHEARMAN.

By any person commencing the study of Symbolic Logic it is not unnaturally soon concluded that there exist several "systems," marked off from one another by fundamental differences. Such systems he is inclined to describe according to the character of the view that the founder entertained as to the import of the proposition. Thus there is the compartmental view, the predication view, the mutual exclusion view, and so on. But subsequent study enables the reader to perceive that, in adhering to such a conception, he is hiding the points of likeness and magnifying the points of difference between the proposed methods of treating the subject, and he is thus led to look rather at the net result of the different efforts. That is to say, instead of continuing to speak of several isolated systems, he proceeds to study the calculus that is now available, and to the construction of which most symbolists are seen to have contributed.

The interest of the subject then gathers round such questions as to whom we are most indebted for those rules of procedure that may be said now to constitute the calculus, what important differences of opinion have arisen as the subject has been gradually thought out, and which of the conflicting views do we find it correct to adopt. Our business in this paper is with the second and third of these questions. In other words, we shall be occupied not so much with an historical sketch of the progress of the subject as with a critical account of certain points that have arisen as the work has proceeded.

SYMBOLS AS REPRESENTING CLASSES AND PROPOSITIONS.

We cannot do better than to commence with the question as to whether the symbols operated upon in the calculus should refer to classes or to propositions. There are here three considerations that must be kept quite distinct if the subject is to be profitably discussed. In the first place, it is possible to affirm that symbols may under one set of conditions represent terms, and under another set of conditions represent propositions, and then it has to be decided which of the two available uses it is expedient primarily to adopt. Secondly, it may be held that it is a matter of indifference whether symbols stand for terms or for propositions. And, in the third place, the opinion may be maintained that only one of the two should be symbolized—on this view it is generally to designate propositions that symbols are exclusively utilised.

As regards the question of expediency, it has been affirmed that we should commence with the symbolization of propositions, for then, firstly, our procedure throughout will be analytical; and, secondly, we shall avoid the “confusion” that is introduced through the identification of the “physical” combination of propositions into a system with the “chemical” combination of subject and predication into a proposition.*

The former of these reasons is undoubtedly a strong one, but I am inclined to think that the common method of beginning with the consideration of classes, and the operations that may be performed upon them, is the better one to employ. For one thing, the latter procedure is of a simpler character than the other. But a stronger reason than this is that during the process of considering the manner in which the analysis of propositions modifies the form of the synthesis, it is necessary to point out that the letters representing predications obey the simple laws of propositional synthesis;† it is, therefore,

* *Mind*, vol. i, N.S., p. 6.

† *Ibid.*, p. 352.

desirable to be able to refer to an earlier discussion of terms and the operations that may be performed upon them.

With respect to the confusion that it is alleged is likely to arise from our allowing letters originally to represent terms, it is, I think, apt to be exaggerated; indeed, a careful analysis of what really happens during the employment of literal symbols in the two spheres will show that there is no good reason for confusion in any degree. The fact that contradictories are not the same in both regions has been declared to be a likely source of error. Now it is certainly true that the contradictories in the two cases are different, but this should not involve any uncertainty in the application of the old formulæ to the new use. All that is necessary is that we make allowance for the change in the character of the contradictory, *i.e.*, we must not admit that propositions are sometimes true and sometimes false.

Again, it has been said that those who utilise the old rules for the new subject-matter will be led actually to confuse a class with a proposition, inasmuch as on the class view the contradictory of x is the class \bar{x} , but on the propositional theory the contradictory of the proposition x is the affirmation " \bar{x} is true."* But this criticism loses its force if the distinction is drawn between the truth of a proposition and the statement that the proposition is true. When the old formulæ are applied to the new case, the correct procedure is to make the letter symbol represent the truth of a proposition, while such an expression as $x = 1$ is used to denote that such a proposition is true. Hence the contradictory of the truth of x does not leave us with a proposition, but simply with the truth of \bar{x} . There is thus a perfect analogy between this case and the case where the letters represent classes. And, just as the class \bar{x} may be declared to exhaust the universe, so it is possible to state that the truth of the proposition \bar{x} is the only possibility. In other words, in both cases we may say that $\bar{x} = 1$.

* *Mind*, vol. i, N.S., No. 1, p. 17.

When writers, who start by making letters stand for classes, come to make such letters stand for the truth of propositions, there is no serious alteration involved, except the one already noticed, in the logical rules that have been established: there is merely another method of interpretation put upon the literal symbols. Such logicians argue that the logical machinery may be put to uses other than those for which it was originally intended. For instance, the symbol 1 from meaning the totality of compartments comes to denote the only possibility, and 0 receives the meaning of no possibility.

Where the symbolic framework, as elaborated from the point of view of the class, does not apply to the new case, the fact is due, as Venn shows, to the circumstance that we have no longer any place in the contradictory for the word "some." In dealing with classes, when it is said that $x + \bar{x} = 1$, it is meant that both x and \bar{x} contribute to the total, but on the proposition interpretation, the admission of x excludes absolutely the admission of \bar{x} . Hence, if xy is declared false, we can only say that one of the three $x\bar{y}$, $\bar{x}y$, $\bar{x}\bar{y}$ is true, while, if xy is declared true, then $x\bar{y}$, $\bar{x}y$, $\bar{x}\bar{y}$ must all be false. That is to say, of the formally possible propositional alternants only one can be true.

But there are some writers who maintain that it makes no difference whether symbols represent terms or propositions. These logicians have to attempt to show that the characters of the contradictories do not vary in the fundamental way that I have just mentioned. Mrs. Ladd-Franklin, for instance, endeavours to deal with this question by asserting that a proposition may be true at one time while it is false at another;* but, as Mr. Johnson remarks, propositions that relate to different times are different propositions. Mrs. Ladd-Franklin asks, "Why exclude from an Algebra which is intended to cover all possible instances of (non-relative) reasoning such propositions as 'sometimes when it rains I am pleased and some-

* *Mind*, vol. i, N.S., No. 1, p. 129.

times when it rains I am indifferent ?'” But I am not aware that any symbolist wishes to exclude such propositions. Supposing we regard this statement as consisting of two propositions—in contradistinction for the moment to the way in which Mr. Johnson argues, namely, that the particle “and” implies that we have really only one—then the symbolist will, of course, say, “Let x equal the proposition ‘sometimes when it rains I am pleased,’ and y equal the proposition, ‘sometimes when it rains I am indifferent.’” Here, if these two propositions are true, we shall have $x = 1$ and $y = 1$ respectively; while if x is not true, *i.e.*, if $x = 0$, the verbal rendering will be “It is not true that sometimes when it rains I am pleased,” and similarly with the rendering of $y = 0$. Mrs. Ladd-Franklin argues as though x were made by the symbolist to stand only for such a proposition as “I am always pleased,” but, of course, the symbol may stand for any proposition (or rather, truth of any proposition) whatever.

But though the symbolist can deal with such propositions he will not in consequence proceed along the lines that Mrs. Ladd-Franklin thinks Schröder should have followed. She argues that it is not justifiable to regard $x < y + z$ as requiring fundamentally different treatment according as x , y and z stand for terms or for propositions. Schröder had maintained that, when the letters represent propositions, it is not possible, as it is on the view that we are dealing with classes, for x to be divided up between y and z . To this his critic says that in material consequences, such as “if it rains, either I stay in or else I take an umbrella,” the proposition is satisfied if there are some instances in which I stay in and some in which I take my umbrella. She fails to observe that the introduction of the word “instances” does away with the special character of the sequence, and reduces the problem to one of class implication. So long as propositions as such are retained, Schröder is undoubtedly correct in saying that x cannot be divided up between y and z .

Again, Mrs. Ladd-Franklin points out that there is a close resemblance between what she terms logical sequence and the case where the left-hand member of the subsumption stands for a singular subject. But this is not any reason for regarding the question whether we allow literal symbols to stand for terms or for propositions as one of indifference. Such a statement as "she is either a queen or a fairy" is one of those limiting cases for whose investigation in general we are so much indebted to Dr. Venn. It is quite correct to say that "there seems, in fact, to be a close relationship between the logical sequence between propositions, and the sequence between terms when the subject is singular," but Schröder's general argument is not thereby invalidated: the original formulæ must be modified to suit the case of the proposition with singular subject and disjunctive predicate, just as there must be modification to meet the case where terms stand for propositions. Mrs. Ladd-Franklin's answer to Schröder, when he asks what can possibly be meant by $(a - < b) = \bar{a} + b$ on the supposition that the letters stand for terms instead of for propositions, appears to me to be quite sound. She says that the verbal rendering will naturally be as follows:—"All a is b " is-the-same-thing-as "everything is either non- a or else b ." But all that is hereby demonstrated is that the letters in a certain equation may have two different readings: there is here no argument to prove that it is a matter of indifference, so far as rules of application are concerned, whether our letters in a problem stand for terms or for propositions.

In stating the facts of the case we have, therefore, to avoid two extremes. On the one hand it is incorrect to say that all the rules apply equally well for both classes and propositions, and on the other hand we need not go so far as to state that the rules are different in the two regions. The former statement is erroneous, the latter suggests more disparity between the two procedures than actually exists.

It has been mentioned also that writers sometimes maintain that symbols should be employed exclusively to represent propositions. Mr. MacColl takes this view of the case. But I can see no valid reason why symbols may not designate now classes and now propositions. The only thing to be remembered is that the rules of procedure are not quite the same in the two cases. If we are to be restricted to one only of the two uses, then I think that Venn is justified in saying that symbols should stand for classes rather than for propositions. As regards the question of economy of space in the solution of problems, the evidence seems to show that the class interpretation is to be preferred. Certainly this is the case so far as the representation of the syllogism—after all an important form of reasoning—is concerned. I do not lay much stress upon the argument based on space-economy. At any rate, we ought not to judge systems by the amount of working that has been offered when the exponents were dealing with certain well-known problems, because as a rule the symbolist could, if he had so chosen, have made his solution much more compact than he did. Still, seeing that symbolism is an aid to thought, we need not despise brevity, if thought is thereby rendered the greater assistance.

SYMBOLIC LOGIC AND MODALS.

In close connection with the subject as to whether Symbolic Logic deals primarily or exclusively with propositions, is the question as to the kind of propositions to which in any case it must confine itself. The symbolist can deal with assertorics only. It has, however, sometimes been held that certain other propositions fall within the scope of his treatment. For instance, he is said to be able to manipulate propositions that are "probably true." I think he has nothing to do with such material, for the simple reason that it does not exist. Mrs. Bryant, in her suggestive paper on "The Relation of

Mathematics to General Formal Logic,"* still holds to the view that it is a legitimate subject of inquiry when we ask concerning a proposition "how often is it true relative to the total number of cases its occurrence in every one of which would constitute its unconditional truth?" Two considerations show that this question is not an intelligible one. In the first place, it is a mistake to speak of a proposition as being *often* true, for on each supposed occasion of its truth there would be a new proposition. In the second place, though unconditional truth may well be established from certain true propositions, this establishment is due simply to the fact that such propositions are true, and not to the fact that they are *always* true. Mrs. Bryant escapes the mistake of speaking of degrees of truth, but she falls into an equally serious error in holding that a proposition may more or less frequently be true. She is quite correct in saying that "a proposition is the assertion of a joint event," but when this assertion is once made it is either true or it is false: it cannot be probably true. It may be more or less probable that the events ought to be joined in the way asserted by the proposition, but such probability is a matter to be taken into consideration before the assertion is made. The error in question arises apparently through the confusion of proposition with event. The probability of an event is certainly measured "by the ratio of the number of cases in which it occurs to the whole number of cases considered," but the probability of the truth of a proposition has no meaning. We may not, as she would allow us, write "proposition" for "event" and "is true" for "occurs."

Nor can the symbolist manipulate propositions respecting probabilities, unless he recognises that he is dealing with an affirmation of the relation in which a thinker stands to a certain statement. That is to say, the symbolist will still be engaged upon assertoric propositions. Mr. Johnson has made

* *Proc. Arist. Soc.*, vol. ii, N.S., p. 121.

this quite clear. As he expresses it, these assertions about the probability that a predicate is to be attached to a subject relate to a different plane from the one with which pure Logic is concerned. They refer to the obligation under which the thinker finds himself to accept a statement of an assertoric kind, but the propositions that engage the attention of the logician are these assertories themselves.

And in the same way that the symbolist cannot without the use of new terms deal with propositions asserting probabilities, so, unless the same procedure is adopted, he must consider as outside his province many of the kinds of propositions that are mentioned in the very ingenious system that has been elaborated by Mr. MacColl. This logician holds that the symbolist, besides classifying propositions into true and false, may make other classifications according to the necessities of the problem. Thus, in addition to the probable, improbable and even propositions already mentioned, there are those that are certain, impossible or variable, those that are known to be true, known to be false, and neither known to be true nor known to be false, and so on. The objection to this procedure is based on the fact that the considerations according to which such classifications are reached all refer to the relation in which the thinker stands to the proposition, and not to the proposition itself. All such facts as Mr. MacColl has in view can be dealt with in Symbolic Logic, but it is in their case necessary to introduce new terms. Thus, take the case of a proposition A , which we will suppose to be false. We have then symbolically $A = 0$. Now, suppose we introduce the conception involved in the words "it is known," the proposition that we shall have to deal with will be "that it is known that A is false is true." It will still be a case of truth or falsehood, but the propositions that are declared true are not the same. Mr. MacColl is, therefore, incorrect in stating that his $A : B$ is *stronger* than $A \text{ ---} < B$: it is not a matter of strength, it is a matter of an entirely different proposition.

It will be seen from these considerations why it is that the same writer's recent explanations of his views are unsatisfactory.* He maintains, for instance, that his formula $(A : x) + (B : x) : (AB : x)$ is true, but that he could not use the formula $(A : x) + (B : x) = (AB : x)$. He grants that the latter is true when $A : x$ means $(\bar{A} + x)$, but not when we have the meaning that he assigns to $A : x$, viz., $(\bar{A} + x)^e$, i.e., "it is certain that A implies x ." In unfolding his view, Mr. MacColl takes an illustration, in which the chances that A is x are 3 to 5, that B is x are 3 to 5, and that AB is x are 1, and his demonstration that under these circumstances the former of the above formulæ alone holds good is doubtless sound. But he is not justified in constructing formulæ upon this plane. At any rate, those that he here constructs form no part of pure Logic, for in this the force of the proposition consists in the definite erasure of certain compartments. If Mr. MacColl wishes to deal with the data he mentions he should introduce new terms. Pure Logic can take account of the uncertainties that such data occasion, but the propositions dealt with will then denote not the relation of the respective letters to x , but the relation of the thinker to each implication.

And here I may perhaps in passing notice the argument advanced by Mr. MacColl in his criticism of the ordinary employment of 1 and 0 in propositional Logic.† His object is to show that such usage leads to absurdity. To do this he commences by affirming that since 1 and 0 denote true and false propositions respectively, these symbols represent two mutually exclusive *classes* of propositions. Hence the definition $0 \text{ ---} < 1$ should assert that every false proposition is a true proposition, which is absurd. My reply to this is that it rests on a misunderstanding. For 1 and 0 never do represent true and false propositions, and consequently two mutually exclusive

* *Mind*, N.S., No. 47, p. 355.

† *Ibid.*, p. 357.

classes of propositions. The symbols denote respectively the only possibility and no possibility: we do not refer to a class at all. The introduction here of the definition $0 < 1$ is, therefore, altogether unjustifiable.

SYMBOLS OF OPERATION.

Next as regards the method of connecting the term-symbols. For a long time it was thought to be absolutely necessary to use symbols of operation, but Dr. Keynes has shown that the most complicated problems may be solved with the greatest ease without such use. The words "and" and "or" are amply sufficient in his hands for the connection of the term-symbols, while to connect the subject-group with the predicate-group he needs not to depart from the customary "is." Still, as Mr. Johnson points out, Keynes has hardly developed a logical calculus, for this is characterized by the mechanical application of a few logical rules.

But I may say that there is a difference of opinion among logicians as to the best manner in which to describe the advanced work that has been done by Dr. Keynes. On the one hand it is said that he has hardly developed a calculus, and on the other hand the question is asked whether his methods can fairly claim to belong to the Common Logic.* Venn thinks that these methods would never have been reached without a training in the earlier symbolic systems, for "the spirit of the methods is throughout of the mathematical type." And Venn, in the second edition of his *Symbolic Logic*, which appeared after the publication of Keynes' work, repeats the statement made in the first edition to the effect that the want of symmetry in the predication view of the proposition forbids its extension and generalisation.† Thus, if

* See Venn, in *Mind*, vol. ix, p. 304.

† *Symbolic Logic*, 2nd ed., p. 29.

Keynes' work is not a calculus and does not belong to the Common Logic, it is a little difficult to know how to classify it. My own view is that it is what he claims it to be, a generalisation of (common) logical processes. There are no symbols that are suggestive of Mathematics except the bracket, and none suggestive of earlier symbolic work except x for not- X . The distinction between subject and predicate is observed, and the use of the copula is retained. There is generalisation of the various forms of immediate inference commonly recognised, as well as of mediate arguments involving three or more terms. Whether the processes can be readily described as a calculus is perhaps doubtful. Certainly Keynes does not reach his conclusions from the mechanical application of a very few fundamental laws, but the rules that he does employ are after all not very numerous, and with a little practice can be applied with almost mechanical facility. I agree with Venn that it is difficult to suppose that such methods would have been reached without study of existing symbolic systems, and there is a distinct resemblance between certain parts of Keynes' treatment of the subject and that given in Schröder's *Operationskreis*, to which work frequent reference is made in the notes of the *Formal Logic*. Still, whatever may have been the history of the growth of the subject in the writer's mind, now that the methods are thus presented I think that they should be regarded as a generalisation of the common logical processes.

Most writers on the subject of Symbolic Logic have undoubtedly introduced symbols of operation, and the four following, as is well known, have frequently been used:— $+$, $-$, \times , \div , to denote respectively aggregation, subduction, restriction, and the discovery of a class which on restriction by a denominator yields the corresponding numerator. Of course, other symbols might have been used to designate precisely these operations, and it may be well to ask whether, seeing that these symbols are employed in a special region of

thought, it is well to have them employed in both regions. If they had first been used by the class logician, would the thinker who deals with numbers have done wisely in adopting them in his science? There is no reason, of course, in the nature of things why they should not have been employed in Logic first of all, but they were in use long before the logician began to look around him for some symbols suitable for the operations he had to perform. Did Boole, therefore, act wisely in making use of these symbols in his solutions? In some respects he did wisely, and in some he did not. He did wisely because there is some analogy between certain processes of Mathematics and those of Logic; for instance, the commutative and associative laws are applicable in both regions. And, even in cases where most of all it may be said that the adoption of mathematical symbols is likely to mislead, there is little risk of error if we regard the symbols as "representing the operation, and merely denoting the result." *

Thus, $\frac{0}{a}$, which in Mathematics denotes zero, might, regarded solely as a result, be taken in Logic to stand for "nothing"; but, when we remember that the symbol also points to an operation, no confusion need arise. It becomes obvious, that is to say, that we here have the result of finding a class which upon restriction by a gives 0, which class is immediately seen to be \bar{a} .

Boole did wisely also—though perhaps somewhat unconsciously—in that by employing these symbols he directed, as Mr. Johnson has remarked, far more attention to the study of Symbolic Logic than the subject would otherwise have received.

On the other hand, it may be doubted whether the analogy between the two sets of processes is sufficient to justify the application of the same symbols. The law that $xx = x$, for instance, is largely operative in the logical region while being

* Mrs. Bryant, *loc. cit.*, p. 108.

almost entirely inapplicable in Mathematics. Moreover, had Boole not adopted these symbols there would have been avoided the many disputes concerning the propriety of using them. Without doubt, out of all the controversy on the subject some truth has emerged, but it is probable that, had the relations of classes or of propositions received the attention that the disputants gave to a comparison of the mathematical and logical processes, Symbolic Logic would have made more rapid strides than it has done. The wonderful mathematical structure was erected without reference to what the logician was doing, or whether he was doing anything, and it may be that the logical structure would have been more imposing if the builder had concentrated his thought upon his own work instead of casting side glances to see what was occupying the attention of the mathematician.

Much discussion has arisen concerning three of these four symbols of operation, and it is stimulating to thought to weigh the arguments that have been advanced in connection with them. First, with regard to the sign $+$. Boole always used this sign on the understanding that the terms so joined are exclusives. It was his special merit, so it has been affirmed, to improve on the common vagueness. That is to say, if "or" on the popular view means anything from absolute exclusion to identity, then the logician is called upon to improve on the ordinary view when he states his premises in symbolic language. It has also been maintained that there is a very great advantage in adopting the exclusive notation, inasmuch as there is then rendered possible the introduction of inverse operations. That is, before ab can be subtracted from an aggregate of terms, it must be known that the aggregate contains ab —if the matter were left open there could be no subtraction. Similarly with division. If a class is to be found which on restriction by a denominator is to yield the numerator, then there must be no indefiniteness as to what this numerator is.

On the other hand, it is maintained that, for the purpose of expressing the premises in symbolic form, much economy of space and time is effected if the non-exclusive method is adopted. Further, on this plan it is possible to arrive at the contradictory by a very simple process. The demonstration of this is one of the most original parts of Schröder's work.* He showed in the *Operationskreis* that the contradictory of $(ab)_1$ is $(a_1 + b_1)$, and that of $(a + b)_1$ is $a_1 b_1$ —in the *Vorlesungen* the proposition appears as No. 36. Of course, Jevons had previously argued that the individual does often think in the non-exclusive fashion, but this is no reason why such notation should be adopted in the logical calculus. It was for Schröder to point out that by the adoption of the method in the calculus problems could be solved more easily than on the Boolean plan; and not only would the process be easier, but, what Schröder thinks to be still more important, each step would be intuitively obvious, and justifiable on purely logical grounds. As a result of the long debate, the non-exclusive notation has undoubtedly found favour, and Venn in his second edition adopts it, having come, as he says, to recognise its "brevity and symmetry," but still holding to the view that the question is one of method rather than of principle. Having thus changed his opinion, Venn has, of course, either to reject all inverse processes, or else to revert to the exclusive notation when dealing with them.

The confusion which has been stirred up by many of those who have discussed this question is greater, perhaps, than is to be found in any other part of Logic. It is very common to find no distinction made between (1) what actually takes place in disjunctive thinking, (2) what is the treatment of the disjunctive judgment in the text-books that discuss the elementary rules of formal logic, and (3) what way of dealing with the disjunctive is the most serviceable for a generalised logic.

* See Adamson's excellent critical notice in *Mind*, vol. x, p. 252.

These three points of view were made clear by Dr. Venn long ago, but they are quite neglected even now in some discussions. For instance, Mr. Ross set out recently * "to try to determine the import of the disjunctive judgment, and to find out the exact place which it occupies in the connected whole of logical thought." He then proceeds to criticize Mr. Bradley and Mr. Bosanquet (who are, let it be observed, talking about the manner in which we are thinking when we are thinking disjunctively) by appealing to considerations based on common logical usages. But obviously the practices of the logician can never define the actual form of the judgment. Somewhat later, when Mr. Ross advances "other considerations which go to show how inexpedient it is to treat the disjunctive judgment as necessarily exclusive," it becomes particularly noticeable that he fails to distinguish between two entirely different questions, one of fact and one of convenience. He actually proposes to show how *inexpedient* it is that alternatives *are* (in Bradley's view) exclusive of each other!

To put the matter in the simplest possible form, when Boole meets with some premises involving alternatives, he asks whether he is to regard the alternatives as exclusives or not. Then, if the answer is in the negative, Boole will write down $xy + \bar{x}y + x\bar{y}$, where x and y were the original non-exclusive alternatives. If Schröder meets with the same premises, he will, of course, also want to know if the alternatives are exclusives, and when informed that they are not, he will write down $x + y$. Then each symbolist may go to work with his special rules, and each may obtain the correct solution. Thus it is the person supplying the problem who places the symbolist in a position to commence the solution. I should not have put the matter in such an elementary form as this were not the many confusions that still exist a sufficient justification. The word "should" has misled Mr. Ross. It

* *Mind*, N.S., No. 48, p. 489.

may mean, "How ought I to describe the actual facts in the mind of the individual who is thinking a disjunctive judgment?" Or it may mean, "How ought I to put down in words or other symbols the facts that constitute the disjunctive thought?"

It is relevant here to notice also Mr. Bradley's treatment of the subject of alternatives. He wishes to show that alternatives are exclusives, and his procedure is to refer to the state of things when they are *not* exclusives.* Evidently, therefore, alternatives can as a matter of fact be either. To put the same thing in other words, Mr. Bradley says that when alternatives are not exclusives we are thinking slovenly. But slovenly thinking is still thinking, though we may readily grant that it is not "always safe." Mr. Bradley seems to have been led to this argument through a confusion of the kind we have just mentioned. He sees difficulties in the way of reasoning if we state the premises symbolically in the non-exclusive manner, and so he argues that those premises must have been given in the exclusive manner. But obviously they may have been given in either form, though we must know which before we can put them down in symbols. When information upon the subject is forthcoming, we can adopt either the exclusive or the non-exclusive method of representation. It has been pointed out that Mr. Ross attempts to show the inexpediency of the fact that alternatives are (in Mr. Bradley's view) exclusives. We now see that Mr. Bradley was led to regard alternatives as exclusives by reflecting how inexpedient it would be if they are not.

Concerning the employment of the sign (—) some difference of opinion has also arisen. In the first place, it has been pointed out that the sign is not absolutely necessary, since subduction may always be expressed symbolically as restriction. But, though this is true, the reply has reasonably been made

* *The Principles of Logic*, p. 124.

that it is frequently more convenient to employ the minus sign, and that no logical considerations render such employment illegitimate. But it is to be noted that only as denoting subduction is the use of the sign appropriate. If the attempt is made to designate negative terms by prefixing $(-)$ to the positive, only error can result. For, as Venn points out, the tendency then becomes almost irresistible to transfer a term with changed sign to the other side of the equation, and this will mean that a statement is made concerning a class about which the premises give no information.

So far all is clear concerning the use of the minus. But sometimes it is employed where the calculus is based on the intensive rendering of propositions, and the use in this way deserves some consideration. Castillon has carried out more consistently than any other writer the development of Symbolic Logic on intensive lines, and I shall restrict my remarks here to his treatment of the sign in question. What he means by $(-)$ becomes evident when we observe his symbolic representation of the universal negative and of its converse. This proposition appears as $S = -A + M$, by which he means that the attributes embraced under S are not co-existent with those embraced under A , but are co-existent with those embraced under M .^{*} Then he affirms that such proposition may be converted thus: $A = -S + M$. Clearly, then, what Castillon means—and he says as much—by the $(-)$ is the mental act of keeping apart, of analysis. But as he has thus far been criticized,[†] he is supposed in the original proposition to assign to S two aggregates, consisting respectively of negative and positive attributes. But this is what he distinctly avoids doing. When such infinite judgment, as he calls it, is to be designated, he employs the form $S = (-A) + M$.

^{*} *Sur un nouvel algorithme logique*, p. 10.

[†] Venn, *Symbolic Logic*, 2nd ed., p. 466.

Moreover, if he had meant what Venn thinks he did, the converse of the universal negative would, of course, have been $(-A) = S - M$. Is, then, Castillon justified in converting in the way he does? Obviously not. For to proceed from $S = -A + M$ to $A = -S + M$ is to conclude that A is co-existent with M , a statement which is at variance with the original proposition. So that on intensive lines, as these are laid down by Castillon, it is not in general allowable, any more than it is in extensive Logic, to transfer letters with changed sign to the other side of the $(=)$.

The last sign that need claim our attention is the one corresponding to the $(+)$ of the mathematics of quantity. Has this inverse process any rightful place in Symbolic Logic, or is it a survival of merely historical interest? I hold that for two reasons the process ought without hesitation to be retained. In the first place, the mental exercise involved in arriving at the comprehension of what is implied in the performance of such inverse operation is, as Venn maintains, of the greatest utility. And, in the second place, the operation is capable of yielding absolutely reliable results. It may be stated in reply to this that, in the performance of the so-called logical division, we utilise symbols that are from the logical standpoint quite meaningless, and that such a procedure is not warrantable; that, in other words, we should follow on the lines which Schröder has laid down, who makes all intermediate processes intelligible. But in answer to this it is to be noted that a calculus is a mechanical contrivance for arriving at results that cannot be intuitively reached. Having given our premises we state them in symbolic language, then manipulate this in accordance with a few simple logical laws, and so reach our conclusion. Whether or not the intermediate results are intelligible is of no importance whatever. Thus even if the intermediate processes in Logic were unintelligible, as is often affirmed, the inverse operations quite reasonably find their place in the calculus.

But, as a matter of fact, the stages between the statement of the premises and the arrival at the conclusion are not meaningless. Certainly Boole never attempted to assign them a meaning, but Venn has carefully examined all the various forms that arise as a result of "division," and he has shown that they have a perfectly intelligible logical signification. The words of explanation that are given by Mrs. Bryant as to how imaginary results arise are not therefore required in the strictly logical realm. It will be remembered that she says, "Whenever a subject is reduced to symbolic expression, imaginary results may be expected to appear, and this happens because the operations of thought which the combining symbols represent extend in application beyond the possibilities of the subject-matter." * No doubt that sentence throws light on a difficult question. But as Boole's forms have all been assigned a strictly logical explanation by Venn, it cannot be asserted that in Logic there are unintelligible expressions that call for consideration. There appeared to be such when Boole published his results, but that was only because he did not perform the task of explicitly stating the logical significance of the forms in question.

To reject inverse processes, as does Mrs. Ladd-Franklin, for instance, is deliberately to throw away useful instruments for solving problems. At the same time, she is undoubtedly correct in showing how important is that interpretation of alternatives which will allow of our reaching the contradictory with ease. The most satisfactory conclusion of the whole matter is that which Venn has formed, namely, to adopt as a rule the non-exclusive rendering, so as to profit by the simple rule for contradiction; but to change to the exclusive notation at times, in order that the advantages to be derived from the employment of inverse operations may not be lost.

* *Loc. cit.*, p. 131.

THE PROVINCE OF THE LOGIC OF RELATIVES.

Perhaps there is no term in Logic which the reader is likely to find so perplexing as the term "Logic of Relatives." He not unreasonably supposes when he comes to this part of the subject that he is going to consider all those expressions whose subject and predicate are not connected by the copula "is," but by the many other words or phrases that frequently join these fundamental portions of a proposition. Such general treatment of copulæ is undoubtedly what the term in question suggests to the mind, and this is the extension that De Morgan at any rate had in view. But in modern logical works this investigation is given up as hopeless, and instead of it we are introduced to the subject of multiple quantifications. Of course, such alteration in the subject-matter need not have involved any confusion, and some writers have made it perfectly clear to their readers that the problem investigated is no longer the wider one. But Mr. Peirce calls the new enquiry by the old name "Logic of Relatives," and such a procedure is very misleading.*

The important question at once arises whether the larger investigation is bound to be fruitless, and, if so, why such is the case. I think that a general treatment of copulæ cannot be undertaken by the logician, because we need in every case to have a piece of special information given us beyond the propositions that form the premises. Such information is necessary whether the conclusion is reached syllogistically or intuitively without the use of syllogism. That such additional proposition is required before the reasoning can be brought under the rules of syllogism is very clear. Take the case mentioned by Jevons. He says: "If I argue, for instance, that because Daniel Bernoulli was the son of John, and John the brother of James, therefore Daniel was the

* Johns Hopkins' *Studies in Logic*, p. 192; *American Jour. of Math.*, vol. iii.

nephew of James, it is not possible to prove this conclusion by any simple logical process"; we need also to be informed that the son of a brother is a nephew. Again, to take a case mentioned by Venn: "If the distance of A and of B from C is exactly a mile, that of A from B (the relation desired) may be anything not exceeding two miles"; here the additional proposition would have to contain information concerning the angular measurements of the triangle made by joining the points occupied by the three persons, and to declare in general terms what, under such circumstances, is the distance between two persons situated as are A and B. In still more indefinite circumstances of relation we should have to possess a still more complicated piece of information along with the original statements. Hence we must undoubtedly reject the doctrine that was once frequently held on this subject, viz., that such an argument as "A equals B, B equals C, therefore A equals C," is, when put in another form, an actual case of syllogistic reasoning. The opponents of such a view were quite right when they argued that this putting into another form involves a *petitio principii*. De Morgan, for instance, made this rejoinder, and Keynes is in agreement with him. Before, then, all possible premises of the kind in question can be dealt with syllogistically there will be needed an infinite number of such special pieces of information, and this amounts to saying that a general treatment of relatives is impossible. If, on the other hand, the validity of such arguments as we are considering is declared not to be established by means of syllogism, but to be as intuitively evident as the validity of *Barbara* itself, the statement means, I take it, that in each case there is involved a separate dictum, corresponding to the dictum of the syllogism. Since, however, the number of such cases is unlimited, there will be an infinite number of dicta in our Logic, which again is impossible.

The way out of the difficulty appears to be the following. It must be admitted that such propositions as the above are not susceptible of being so manipulated that they shall be put into

sylogistic form. Also it is absurd to suppose that we have at our disposal an infinite number of major premises or of dicta. Hence the general treatment of copulæ is impossible. But what we can do is to admit an arbitrary number of general propositions other than the *dictum de omni*, and the propositions thus admitted allow of our dealing with a limited number of arguments like the above. There is a special group of such statements of great importance, and they occur in the region of quantitative mathematics. I regard the axioms of Geometry as being among the assumptions that are necessary in order to allow of the application of syllogistic reasoning to propositions of that science. It is a verbal matter whether we call the additional information, that is required, by the name of dictum. I should say that it may be so called when the same information is required in a large number of instances, as is the case, for instance, with the so-called axioms in Euclid. Where such additional information is used once only, it is preferable to employ a less pretentious term.

Of course it may with good reason be here asked whether there is such fundamental difference between the *dictum de omni* and the other general propositions (say the axioms of quantitative mathematics) as to give such unique importance to the former. Are not those axioms, as De Morgan affirms,* "equally necessary, equally self-evident, equally incapable of demonstration out of more simple elements" with the *dictum*, and, if so, are not the two equally important? My view is that whatever may be the character of the two kinds of axioms as regards derivation and self-evidence, they are not of equal importance. For in all reasoning concerning quantities the *dictum de omni* is required, while in reasoning concerning qualities, where, of course, the *dictum* is also needed, the axioms of quantitative mathematics afford no assistance. De Morgan in another place† endeavours to show that questions of equality

* *Trans. Camb. Philosoph. Soc.*, vol. x, p. 338.

† *Syllabus of a Proposed System of Logic*, pp. 31, 32.

and of identity are formally on an equal footing, since "the word *equals* is a copula in thought, and not a *notion attached to a predicate*," and that "logic is an analysis of the form of thought, possible and actual, and the logician has no right to declare that other than the actual is actual." The answer to this appears to be that, though the individual does actually regard the "equals" as a copula, he does so only by a process of abbreviation: the form when fully expressed is one of identity. The logician is not bound to treat as of fundamental importance each kind of abbreviation that mankind has adopted. It is enough for him to deal with the fully expressed form, and to explain, as we have done above, that in other apparent examples of formal reasoning there is only a syllogistic process plus some material assumptions. In this discussion we have been considering cases in which only three terms are involved, and the matter has been regarded from the point of view of ordinary Formal Logic. In this narrower region the *dictum* is unique. But from such statements it is not to be concluded that we shall not when discussing the generalisation of logical processes reject the *dictum*. It will be rejected, however, not because it is not in a unique way of a formal character, but because it applies to only three terms, and we must adopt axioms that are "necessary and sufficient" for dealing with arguments of any degree of complexity.

At first sight the above statement of the case appears perhaps to agree with the view that Boole adopted. But there is really no such agreement. Boole held that general logic is quantitative mathematics with the quantity element left out, that is to say, class logic and quantitative mathematics participate in the nature of general logic, and have in addition their own special characteristics. It seems to me, on the other hand, that quantitative mathematics is a combination of the quantitative element and the principles of class or propositional logic. There are not two species of the genus general logic: there is one logic, and that is class or propositional logic, and

all that there is in mathematics is such logic, together with some material assumptions concerning quantitative objects. No argument whatever can be carried on in quantitative mathematics without the explicit or implicit application of class or propositional logic at every step. Certainly Boole appeared to establish two species of reasoning, when he applied the symbols of mathematics to the manipulation of arguments involving classes; but what he was really doing was to show how qualitative reasoning, if we employ in it symbols analogous to those that represent quantitative objects and processes, may be extended far beyond the limits of the old syllogistic arguments. To put the matter in a word, I recognise only the so-called specific logic of quality, and I regard quantitative reasoning as merely qualitative reasoning together with certain assumptions concerning the relations of quantities. As Dr. Shadworth H. Hodgson says,* formal logic "is a system wholly unrestricted in its range," or, as he adds, class Logic is "the Logic of the whole nature of any and every object of thought, of its *What*, *τί ἐστίν*, of its *Quid*, which includes both its *Quale* and its *Quantum*." That is to say, class Logic has to do with the relation of classes whether qualitatively or quantitatively determined.

It need hardly be said that though Jevons speaks of the necessity of there being additional information, before the proposition that I have quoted from him can be manipulated, he does not make any general statement on the subject. And he evidently considers that all such arguments form a class distinct from the miscellaneous selection which he brings forward in illustration of his principle of Substitution. My view is rather that his illustrations are special cases of relative reasoning, and that this is not in general possible except on the lines that I have endeavoured to indicate.

When it is stated, as was the case at the commencement of this section, that the expression "Logic of Relatives," as now

* *Proc. Arist. Soc.*, N.S., vol. ii, p. 135.

used, refers only to the operations performed upon propositions involving multiple quantifications, it is not meant to suggest that this investigation is not important. On the other hand, I think that we have here a development of the greatest interest. One problem that we have to solve in this part of the subject concerns the method of synthesizing these multiply-quantified propositions. Another problem is when we are given such a synthesis, and have to find the least determinate alternant that will explain the given synthesis, or the most determinate determinant that the synthesis implies.* An investigation of the principles, according to which these results may be reached, naturally follows the study of the subject-matter of ordinary Symbolic Logic, in which, of course, we are concerned with singly-quantified propositions.

THE UTILITY OF SYMBOLIC LOGIC.

A few words may be added as to the utility of Symbolic Logic. Of the educational advantages arising from the concentration of thought that the discipline demands, it is impossible to speak too highly. On all sides the educational utility of mathematical study is recognised, but I venture to state that Symbolic Logic takes no second place in this respect. Probably, also, everyone would allow that the generalised treatment of thought throws much light upon problems that appear in the special or syllogistic treatment. As regards the direct utility of the discipline, the question is somewhat complex. It may readily be granted that natural science cannot make any direct use of Symbolic Logic. Mathematics is absolutely necessary for an insight into Nature's laws, but natural science is not immediately furthered by the rules of the logical calculus. Jevons seems to think that the facts point in the other direction, for he held that science is advanced by means of the Substitution of Similars. But the truth is that science

* *Mind*, N.S., No. 3, p. 354.

must supply the premises upon which the symbolic logician may bring to bear his mechanical contrivances.

The position of Jevons on this subject is, I think, still at times somewhat misunderstood. Mr. E. C. Benecke, for instance, affirms that Jevons did not intend the symbolic system as developed in the *Principles of Science* to assist in the advancement of knowledge.* But surely this is not a correct view of Jevons' position, for he maintained that "the *Substitution of Similars* is a phrase which seems aptly to express the capacity of mutual replacement existing in any two objects which are like or equivalent to a sufficient degree,† and "in every act of inference or scientific method we are engaged about a certain identity, sameness, similarity, likeness, resemblance, analogy, equivalence or equality apparent between two objects."‡ Nothing could be clearer than these statements. Jevons thought and said that the principles of his symbolic calculus were applicable for the advancement of science. Mr. Benecke is apparently of opinion that Jevons in the *Principles of Science* first developed a symbolic calculus, and then proceeded to deal with scientific methods. But the whole book has to do with the methods of science (as Croom Robertson says, "the Methods, rather than the Principles, of Science, would, perhaps, be a more appropriate title for the book as it stands"), and the latter portion of the volume is engaged not upon an investigation quite distinct from that which occupies the former part, but with the work of ascertaining "when and for what purposes a degree of similarity less than complete identity is sufficient to warrant substitution." This substitution is all along held to be the fundamental process.

And here, by way of parenthesis, I may perhaps be allowed to make a few further remarks upon the logical position of Jevons. It is impossible for readers of Symbolic Logic not to

* *Proc. Arist. Soc.*, N.S., vol. ii, p. 141.

† *The Principles of Science*, p. 17.

‡ *Loc. cit.*, p. 1.

give his views frequent consideration, and it will be useful to inquire how far he has contributed to the erection of the symbolic structure. Students of Venn cannot but be impressed with the fact that many of Jevons' proposals are of little or no value. I have drawn up as full a statement of the case as I have been able to reach. Jevons' doctrine of the superiority of the equation $x = xy$ to represent the universal affirmative is erroneous, for this form is immediately reducible to $x = \frac{0}{0}y$ or $x = v.y$. It is impossible to adopt his method of denoting particular propositions, for though he avoids the difficulty apparent in the Boolean system, where $\frac{0}{0}$ is taken to denote complete indefiniteness, such escape is effected by employing the postulate that no term whatever shall be equivalent to 0. This would exclude the possibilities of a calculus, for a collection of consistent propositions may eventually be found to have established the entire destruction of a certain term.* I should agree with this criticism of Venn's, but I do not think that Jevons would have done so; he would probably have replied that if such collection of propositions resulted in such a destruction then the group was not perfectly consistent. Again, we have already seen that Jevons' argument against using the exclusive notation in Logic is not valid, though since his time this method of dealing with alternatives has been largely adopted: his point was that we do often think in the non-exclusive manner, but this is no reason why we should do so in our symbolic reasoning. He certainly drew up a table by which a type of proposition may be reached for the solution of the inverse problem in the case of three terms, but he did little more than indicate the difficulty involved in solving the inverse problem in general. Moreover, his doctrine that Induction is to be identified with this inverse method is quite

* Venn, *Symbolic Logic*, p. 156.

erroneous, for, as Mr. Johnson has most perspicuously shown, the series of propositions that Jevons desires to reach are only determinants of the data—are, that is to say, neither more general nor more conjectural than the data. Jevons' conception of Boole's idea of the scope of mathematics was, previous to the second edition of the *Principles of Science*, altogether mistaken, and hence the attempts in the earlier edition to "divest his (Boole's) system of a mathematical dress" could not result in much that is useful.* But even in the second edition the inaccurate notion has only partially disappeared. Boole's is still a quasi-mathematical system, still requires "the manipulation of mathematical symbols in a very intricate and perplexing manner." Jevons, in holding the view that the process of subtraction is useless because the same operation can be represented as one of restriction, passes over the fact that each may be useful at times. His objection that, because he admits the Law of Unity into his system, Boole must necessarily have done the same, is without force, since Boole was not guilty of any inconsistency in the omission. Jevons declared that $\frac{0}{0}$ cannot be understood without reference to the mathematics of quantity, an assertion which is refuted from the simplest logical considerations. His statement that inverse operations are impossible is contradicted by the history of Symbolic Logic. I do not profess that this list is complete, but it must be confessed that, though Jevons stimulated logical thought much more extensively than most men are enabled to do, his actual contributions to the development of Symbolic Logic were few and relatively unimportant. His great powers were, in short, less successfully occupied in the logical than in the mathematical realm. In pure economic theory and in currency investigations, where in both cases the argument is almost entirely concerning quantities, his work is of the utmost value,

* G. B. Halsted, in *Mind*, No. 9, p. 134.

and has placed him in the very first rank of thinkers upon such subjects.

To resume the main discussion of this section, we have said that Symbolic Logic does not directly lead us to any new truths in natural science. It is, however, by no means the case that no new truth at all, but only a recognition in another form of the information contained in the premises is reached by means of the calculus. For what is a new truth? It is an accurate subject-predicate combination that an individual forms, but which has never till then been formed in the history of the race. Now such a combination may be reached deductively or inductively. It was a new truth when the conclusion of *Euc. I, 47*, was for the first time reached, just as it was a new truth when Adams and Leverrier discovered the planet Neptune. In a second sense a truth may be said to be new when, though well known to science, the full force of the subject-predicate combination is for the first time grasped by the mind of a student. Here again the above-mentioned combinations may take equal rank in their claims to be designated new. And, just as in pure Mathematics the results may constitute new truths in both of the above senses, so in Symbolic Logic we may be said in the same senses to reach a new truth. For instance, the difficult problem that was first solved by Boole* gave a result that was true and altogether new, and this solution, which is well known to all symbolists, is the occasion of the experience of a new truth in the mind of each student of the subject.

Moreover, though it be correct, as we have seen, to say that Symbolic Logic cannot directly assist the individual in his scientific pursuits or in his daily affairs, the indirect help of the discipline in each of these regions is by no means insignificant. Mankind is consciously or semi-consciously much occupied with questions that turn upon the relations of classes,

* Boole, *Laws of Thought*, pp. 146-148.

so that the manner of looking at things which the logical study makes habitual cannot fail to be of service in practical concerns. Instead of confining himself to things that are seen, the logician spontaneously is led to regard the things that are not seen. It has become a custom with him to consider the \bar{x} as of equal value with the x . The truth is not that his logically-developed habits are not applicable to the affairs of ordinary life, but rather that he will so weigh the pros and cons of a question that his active forces will be apt to suffer from a certain paralysis. The man of strong will, who has a more or less vivid idea of one aspect of a practical problem, is much more likely to achieve a great deal than the man who sees accurately both sides. Hence the dilemma faces us whether it is better to act vigorously, and accomplish much that has to be revised and largely undone, or to produce only a small amount, but such as needs little alteration.

Now, if the study of Symbolic Logic is thus indirectly of use in natural science and in practical affairs, then *a fortiori* the study is of service to the philosopher. For I take it that we philosophize rather in order to know than in order to act, and therefore in philosophy there is no danger whatever arising from seeing the other side of a question. I think, moreover, that the principles of Symbolic Logic point in a striking manner to the fact that in philosophical researches we shall always be left with a duality, however far we press our investigations. Attempts to reduce the world to unity—to God, to Self, to Nature, for instance—appear to be doomed to fail. In this extreme case our 1 means the totality of the existent, the universe in the common acceptance of that term. As before, $x + \bar{x} = 1$ of necessity, and with this necessity we are obliged to stop. We cannot establish the existence of x only, for there is no premise available with the information that $\bar{x} = 0$. For instance, let x stand for "God," then \bar{x} will stand for "not-God." Now, if we attempt to demonstrate the non-existence of \bar{x} , we shall be proceeding in an absurd manner, for

we shall be assuming, if not ourselves, at any rate our reasoning, which evidently is a part of the \bar{x} . An opponent of this argument might perhaps affirm that the human proof may well be regarded as a form of Divine reasoning. God would thus be proving His own exclusive existence. But it is obvious that the circumstances under which such Divine ratiocination would be taking place would be such that a human thinker was recognising the argument as his own construction. Hence the human mind and its reasoning would still be distinct from the Divine. And, similarly, in our other efforts to reach unity, the argument is based on the assumption of an ultimate duality.

The remarks that we have made with respect to the utility of the ordinary Symbolic Logic apply also to the so-called Logic of Relatives. In this further study we do not arrive at anything more general or conjectural than the multiply-quantified propositions with which we start. There is here, therefore, no instrument by which the problems of natural science may be solved. But the educational advantage and indirect assistance of the study, and the possibility of reaching new truths, in the sense that we have just mentioned, are the same as in the case of the Symbolic Logic that deals with singly-quantified propositions.
