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V. *Implicational and Equational Logic.*
 By HUGH MCCOLL, B.A., *University of London**.

PROF. JEVONS, in his new work, 'Studies in Deductive Logic,' of which he has kindly sent me a copy, refers to my papers in 'Mind' and in the 'Proceedings of the London Mathematical Society' in terms which might give rise to some misapprehension as to the real nature of my symbolical method. He says that "I reject equations in favour of *implications*," and in so doing "ignore the necessity of the equation for the application of the Principle of Substitution."

Now, it is quite true that I reject equations in favour of implications in those classes of logical problems (and they are very numerous) in which implications lead to the simplest, shortest, and most elegant solutions; but there are other classes of problems, especially in mathematics, which necessitate the equational form of statement; and in these I do not hesitate to adopt it. The simple truth is, that my method admits of both forms; and, as a matter of fact, I employ both, sometimes even in the same problem. In my first paper in the Proceedings of the London Mathematical Society (which treats of the limits of multiple integrals) I adopt the equational form throughout; in my second and third papers, which relate entirely to questions of pure logic, I generally adopt the implicational form, as the simplest and most effective; while in my fourth paper, which treats of probability, I mainly adopt the equational form.

As to the statement that "I ignore the necessity of the equation in the application of the Principle of Substitution," I am not quite sure that I understand what it means. I certainly recognize the principle that if $\alpha = \beta$, then $f(\alpha) = f(\beta)$, or, as the rule may be expressed symbolically in my notation, $(\alpha = \beta) : \{f(\alpha) = f(\beta)\}$; but I cannot in the least understand what bearing this has upon the advantages or disadvantages of my system of implications.

The question whether the implication $\alpha : \beta$, or its equivalent, the equation $\alpha = \alpha\beta$, should be preferred in a symbolical system of logic, must be decided on the broad grounds of practical convenience. I believe it may be taken as a useful

clearly kept in view, viz. that this assumption or theory, by opening out an absolutely limitless field of speculative hypothesis, completely annihilates all *method* or rational system in physical inquiry, and therefore that all progress or insight into the physical processes underlying phenomena is absolutely brought to a standstill so long as this theory is adhered to.

* Communicated by the Author.

principle in symbolical reasoning generally, that conventional symbols of abbreviation should be adopted for all expressions *which have to be employed frequently*. On this principle a^3 was probably used as a convenient abbreviation for aaa , a^4 for $aaaa$, and so on, before the discovery of the important law expressed by the equation $a^m \times a^n = a^{m+n}$. The same necessity for symbolical abbreviation originated the useful symbols $f(x)$, $f(x, y)$, $f'(x)$, and many others. On this principle, since I find that such statements as "If α is true β is true," or " α implies β ," are extremely common in all reasoning, I use the simple symbol $\alpha : \beta$ as a very convenient abbreviation*. Granted that the equation $\alpha = \alpha\beta$ will also accurately express the statement " α implies β ," it is a much less simple and suggestive expression for it. Compare, again, the implication, $\alpha\beta + \gamma\delta : ab + cd$, with its equivalent, the equation $\alpha\beta + \gamma\delta = (\alpha\beta + \gamma\delta)(ab + cd)$, and the superior simplicity of the implication will be still more striking. But the abbreviating power of my symbol of implication becomes most conspicuous in what may be called *implications of the second order*, as in the syllogism

$$(\alpha : \beta)(\beta : \gamma) : (\alpha : \gamma).$$

May I ask Prof. Jevons how he would express this syllogism in his equational notation, *in pure symbols and entirely without words*. I can only see one way in which he could do this consistently with his views, namely by the very clumsy equation

$$(\alpha = \alpha\beta)(\beta = \beta\gamma) = (\alpha = \alpha\beta)(\beta = \beta\gamma)(\alpha = \alpha\gamma).$$

This looks so exceedingly like a *reductio ad absurdum*, that I cannot help hoping that it will lead Prof. Jevons to reconsider his opinion that *the equational form alone* should be employed in symbolical logic.

So far I have argued on the assumption that my $\alpha : \beta$ is equivalent to Prof. Jevons's $\alpha = \alpha\beta$; and both Prof. Jevons and I agree, I believe, in the opinion that practically this is the case. At the same time, it must be borne in mind that, for this assumption to be strictly true, the letters α and β must have the same meanings in the implication $\alpha : \beta$ as in the equation $\alpha = \alpha\beta$; and therefore either each letter must in both forms

* The equivalence of $\alpha : \beta$ and $\alpha = \alpha\beta$ may be proved formally in my notation as follows:—

From the formula

$$(\alpha = \beta) = (\alpha : \beta)(\beta : \alpha)$$

we get

$$(\alpha = \alpha\beta) = (\alpha : \alpha\beta)(\alpha\beta : \alpha) = (\alpha : \alpha)(\alpha : \beta)(\alpha\beta : \alpha) = (\alpha : \beta);$$

for the factors $\alpha : \alpha$ and $\alpha\beta : \alpha$ are each equal to unity—that is to say, *always true*, whatever the statements α and β may be. (See formula 3 of this paper further on.)

denote a *statement*, as in my system, or else each letter must in both forms denote a *quality or thing*, as in Prof. Jevons's system. On the supposition that each letter denotes a statement, my notation exhibits clearly the very remarkable fact that in the syllogism $(\alpha : \beta)(\beta : \gamma) : (\alpha : \gamma)$, the very same relation which connects α with β , and β with γ , connects also the combined premises $(\alpha : \beta)(\beta : \gamma)$ with the conclusion $\alpha : \gamma$. On the assumption that each letter denotes a statement, Prof. Jevons's notation (as I have shown) could only show this coincidence of relation in a very clumsy and roundabout manner; while, on the assumption that each letter denotes a thing or quality (as in his system), his notation could scarcely be used in this extended way at all.

The same remarks apply to many other useful and symmetrical formulæ, which, so far as I can see, are altogether uninterpretable on Prof. Jevons's hypothesis that each letter should denote a thing or quality; while on my hypothesis, that each letter should denote a statement, every formula conveys a clear and precise meaning, which it is scarcely possible to misunderstand. Take, for example, the formulæ:—

- (1) $(A : a)(B : b)(C : c) \dots : (ABC \dots : abc \dots);$
- (2) $(A : a)(B : b)(C : c) \dots : (A + B + C + \dots : a + b + c + \dots);$
- (3) $(x : a)(x : b)(x : c) \dots = (x : abc \dots);$
- (4) $(a : x)(b : x)(c : x) \dots = (a + b + c + \dots : x);$
- (5) $(a : x) + (b : x) + (c : x) + \dots : (abc \dots : x),$
- (6) $(x : a) + (x : b) + (x : c) + \dots : (x : a + b + c + \dots).$

These formulæ express logical laws of undoubted truth, which Prof. Jevons could scarcely express in his notation without the help of words.

Prof. Jevons approves to some extent of my accent to express denial, and occasionally adopts this notation in his new work; but he finds it difficult, he says, to believe that there is any advantage in my innovations in other respects, and he is of opinion that "my proposals tend towards throwing Formal Logic back into its ante-Boolean confusion." To this general condemnatory opinion it is difficult to make any definite reply; I can only express my regret that Prof. Jevons has nowhere throughout his book given a single example of this tendency in my proposals "towards throwing Formal Logic back into its ante-Boolean confusion." Abundant materials were at his disposal for comparing my method with his own in the fairest and most decisive way possible, namely in the actual solution of problems. Out of the various problems of which I have published solutions he might surely have found *one* with which

to point and illustrate his criticism. Friendly contests are at present being waged in the 'Educational Times' among the supporters of rival logical methods; I hope Prof. Jevons will not take it amiss if I venture to invite him to enter the lists with me, and there make good the charge of "ante-Boolean confusion" which he brings against my method.

November 29, 1880.

VI. Note on Prof. Exner's *Papers on Contact Electricity*.

By W. E. AYRTON and JOHN PERRY*.

I. IN the autumn of 1879 Prof. Fleeming Jenkin drew our attention to a paper by Prof. Exner, read before the Vienna Academy of Science, and appearing in the July number of their 'Transactions' for that year. This paper will also be found reprinted in this year's April number of Wiedemann's *Annalen der Physik und Chemie*; and quite recently an English translation, prepared by Mr. Brown, has appeared, in the October number of the Philosophical Magazine.

As then, this paper has been deemed of sufficient importance to be printed at least three times; and as the reasoning employed in it is of so plausible a nature as to mislead a casual reader, and to give him erroneous notions on the subject of contact electricity, we have thought it worth while to draw attention to the inaccuracies it contains.

The calculation given in the Phil. Mag. for 1851 by Sir Wm. Thomson of the electromotive force of a Daniell's cell, based on the principle of the conservation of energy, is of course well known. The method employed, which was due to Dr. Joule, is as follows:—The work done by a quantity of electricity Q passing between two points at a difference of potential E is EQ . Now if this electromotive force is produced by a Daniell's cell, the preceding quantity of work must be equal to the energy-equivalent of the chemical changes that take place in this cell when a quantity of electricity Q passes through it. And since this latter can be determined from the heats of combustion of the products decomposed and formed, and from a knowledge of Joule's mechanical equivalent of heat, Sir Wm. Thomson was enabled to calculate the electromotive force of the Daniell's cell from the supposed known chemical reactions taking place in it. It has been pointed out by Dr. Wright†, and by others, that the great coincidence between the electromotive force of the

* Communicated by the Physical Society, having been read at the Meeting on November 13, 1880.

† "On the Determination of Chemical Affinity in Terms of Electromotive Force," by C. R. Alder Wright, D.Sc., Phil. Mag. April 1880, p. 247.