of Frege, however, are so briefly considered and so little worked out, that a detailed polemic is impossible.

It is also regrettable that Frege takes no notice at all of the previously existing works on the same subject. I mean the investigations of Boole, Jevons, Schröder, MacColl, and others who—partly seeking to solve exactly the same problems as Frege, and partly wishing to establish a logical calculus—necessarily occupied themselves with the establishment of formulas and symbols for logical operations. So far as MacColl is concerned, what sufficiently proves the kinship of Frege's investigations and his (mentioned in the *Educational Times* under the title "Symbolic Language") is that with the help of his symbols, [MacColl] succeeds in easily solving some problems from integral and probability calculus.

Perhaps the utilization of these previous works, which are simple, adaptable, and partly correct, would not be without value to the author. His work [however] remains obviously so much more original and certainly does not lack importance.

Berlin C. TH. MICHAËLIS

D. Review of Frege's *Conceptual Notation* by E. Schröder, *Zeitschrift für Mathematik und Physik*, 25 (1880), pp. 81–94.[1]

This very unusual book—obviously the original work of an ambitious thinker with a purely scientific turn of mind—pursues a course to which the reviewer is naturally highly sympathetic, since he himself has made similar investigations. The present work promises to advance toward Leibniz's ideal of a universal language, which is still very far from its realization despite the great importance laid upon it by that brilliant philosopher!

The fact that a *completed* universal language, characteristic, or general conceptual notation {*allgemeine Begriffschrift*} does not exist even today justifies my trying to say from the beginning what is to be understood by it. I almost want to say, "it is a risk to state [what a completed universal language would be like]"; for, as history teaches, in the further pursuit of such ideals, we often find ourselves led to modify the original ones very significantly; especially once we have succeeded in advancing substantially toward [our goal]. Perhaps one begins by considering the most important point unimportant or overlooking it; one is compelled to leave matters

[[1] This translation was made independently of the one by V. H. Dudman which appears in *The Southern Journal of Philosophy*, 7 (1969), pp. 139–50; and then the two were compared. Wherever Dudman's interpretation or wording seemed better, it was adopted and duly noted. Wherever important differences of interpretation remained, they were also noted to give the reader the benefit of both views.]

which are impossible to know, or to make compromises with reality—not to mention the fact that new aims, which emerge as desirable along the way, may perhaps turn out to be achievable in unexpected ways.

I believe I do not depart from the historical interpretation by formulating the problem in the following way (*mutatis mutandis* for the various basic fields of knowledge):[2] *to construct all complex concepts by means of a few simple, completely determinate and clearly classified operations from the fewest possible fundamental concepts* {*Grundbegriffen*} (*categories*) *with clearly delimited extensions.*

In considering an ideal, it is not improper to refer to an analogue which has already been used. Thus, I wish to add a comparison already employed by Leibniz (if I remember rightly): compare, say, how composite numbers arise from prime numbers through multiplication—or also, if you will, how in a similar way the natural numbers are constructed {*zusammengesetzt*} in general from the first eleven [*sic*] such numbers through the relations of multiplication and addition to form the decimal system. Incidentally, in recent times several other works have been published which concern themselves with listing the fundamental concepts {*Kategorien*}. Nevertheless, such schematizations may be granted only a minor value so long as the proof (which I find lacking in them) is omitted that, in fact, through the combination of the fundamental concepts which those works lay down, all the remaining concepts follow—thus also so long as the investigation lacks [an account of] which combining operations come into question and by which laws the combinations are governed.

Even if, in spite of all earlier attempts and also the latest one now under discussion, the idea of a universal language has not yet been realized in a nearly satisfactory sense; it is still the case that the impossibility of the undertaking has not come to light. On the contrary, there is always hope, though remote, that by making existing scientific technical language {*wissenschaftliche Kunstsprache*} precise, or by developing a special such language, we may gain a firm foundation by means of which it would someday become possible to emerge from the confusion of philosophical controversies, terminologies, and systems whose conflict or disagreement is to be mainly attributed (as indeed can be generally seen) to the lack of definiteness of the basic concepts. The blame must be placed almost entirely upon the imperfections of the language in which we are forced to argue from the outset.

Given the sense [of 'conceptual notation'] which I sought to indicate in the above remarks, it must be said that Frege's title, *Conceptual Notation*, promises too much—more precisely, that the title does not correspond at all to the content [of the book]. Instead of leaning toward a universal characteristic, the present work (perhaps unknown to the author himself)

[[2] Dudman's turn of phrase.]

definitely leans toward Leibniz's "*calculus ratiocinator*". In the latter direction, the present little book makes an advance which I should consider very creditable, if a large part of what it attempts had not already been accomplished by someone else, and indeed (as I shall prove) in a doubtlessly more adequate fashion.

The book is clearly and refreshingly written and also rich in perceptive comments. The examples are pertinent; and I read with genuine pleasure nearly all the *secondary discussions* which accompany Frege's theory; for example, the excellently written Preface. On the other hand, I can pass no such unqualified judgement upon the major content—the formula notation itself. Nevertheless, anyone interested in the methodology of thinking will derive much stimulation by working through the book; and I state explicitly that it merits a recommendation for closer study, in spite of the numerous and in part serious criticisms which now I shall also objectively put forward.

First of all, I consider it a shortcoming that the book is presented in too isolated a manner and not only seeks no serious connection with achievements that have been made in essentially similar directions (namely those of Boole), but even disregards them entirely. The only comment that the author makes which is remotely concerned with [Boole's achievements][3] is the statement on page iv of the Preface, which reads, "I have strictly avoided those efforts to establish an artificial similarity (between the arithmetical and logical formula languages)[4] through the interpretation of the concept as the sum of its characteristic marks {*Merkmale*}." This comment even by itself lends a certain probability to the supposition—which gains confirmation in other ways— that the author has an erroneous low opinion of "those efforts" simply because he lacks knowledge of them.

It may be mentioned here that the book has been reviewed by someone else—Kurt Lasswitz, *Jenaer Literaturzeitung* (1879), No. 18, pp. 248 f. To be sure, I can agree with this very kindly written review on many points, while nevertheless allowing myself at the same time to cast a disapproving glance at it. I must criticize its particular opinions of Boole's orientation; it carries the above-mentioned erroneous conception even further than the author.

Of course the Boolean theory is "onesided", just as almost every investigation within a special scientific field naturally is. It fails, by far, to achieve everything that one could wish and will still require further development in various ways. On the other hand, so long as proof of the contrary has not been specifically furnished, Boolean theory "is based" neither upon an "inadmissible apprehension of the concept", nor above all upon "doubtful" presuppositions (see my argument below).

[3 Schröder assumes that the quoted passage refers to the work of Boole. To the present editor it seems more likely that it refers to the work of Leibniz, which Frege knew well. See the present volume, p. 105.]

[4 Schröder's parenthetical insertion.]

However, the comment (which I shall prove below) that might contribute most effectively to the correction of opinions is that the Fregean "conceptual notation" does not differ so essentially from Boole's formula language as the Jena reviewer (perhaps also the author) takes for granted. With the exception of what is said on pages 15–22 about "function" and "generality" and up to the supplement beginning on page 55 [Part III of Frege's book], the book is devoted to the establishment of a formula language, which essentially coincides with Boole's mode of presenting *judgements* and Boole's calculus of judgements, and which certainly in no way achieves more.

With regard to its major content, the "conceptual notation" could be considered actually a *transcription* of the Boolean formula language. With regard to its form, though, the former is different beyond recognition—and not to its advantage. As I have said already it was without doubt developed completely independently—all too independently!

If the author's notation does have an advantage over the Boolean one, which eluded me, it certainly also has a disadvantage. I think that to anyone who is familiar with both, [the author's notation] must above all give the impression of hiding—to be sure not intentionally, but certainly "artificially"—the many beautiful, real, and genuine analogies which the logical formula language naturally bears with regard to the mathematical one.

In the subtitle, "A Formula Language Modelled Upon that of Arithmetic", I find the very point in which the book corresponds least to its advertised program,[5] but in which a much more complete correspondence could be attained—precisely by means of the neglected emulation of previous works. If, to the impartial eye, the "modelling" appears to consist of nothing more than using *letters* in both cases, then it seems to me this does not sufficiently justify the epithet used.

Now in order to prove my above assertions and be able to critically examine the formula language itself, I cannot help presupposing as known the basic concepts of logical calculus. Concerning the literature of this discipline, which I have described elsewhere, an extensive appendix is to be found at the end [of the present review]. Instead of merely referring to my book (6),[6] in view of the doubts expressed by the other side, I want to explain here the few things which are essential for understanding what follows.

As a propaedeutic for the logical calculus, one can introduce the calculus of identity of *domains of a manifold* {*Calcul der Identität von Gebieten einer Mannigfaltigkeit*}. This is a purely mathematical discipline whose theorems {*Sätze*} clearly must be granted complete certainty and correctness. Then, a mere change in the interpretation or meaning of the symbols

[5 Dudman's turn of phrase.]

[6 Numerals in parentheses refer to the book list at the end of this review.]

leads from this first calculus to the present logical one, which corresponds entirely to the first so far as the [calculating] technique is concerned.

Let there be a manifold of elements—for example, the [elements] of the points of an arbitrarily bounded or even unbounded plane. Letters, such as $a, b, c, \ldots$, are to represent arbitrary domains which belong entirely to this manifold, thus—for our example—to speak generally, any parts of the plane. These domains are to be considered equal only when they are identical.

Relations of size should be entirely disregarded. (The mathematician is so used to associating letters with the idea of the number representing a quantity that for a beginner in our calculus a conscious effort is necessary to free himself from this habit, even though it is not given to him by nature, but laboriously instilled in school. Hence, a stands for the planar region itself, but not the number representing its size.)

The entire domain of the given manifold is symbolized by 1; while the "negation" of a by means of a_1 symbolizes the domain which is the complement of a in the manifold. 0 stands for a supposed domain of the manifold if it happens that [the supposed domain] has absolutely no element in common [with the manifold] and hence actually does not exist as a domain of the latter.

Now, if by $a.b$ (or ab) is understood that domain which the domains a and b have in common, thus [that domain] in which [a and b] intersect each other; and if by $a+b$ [is understood] that domain in which [a and b] are added together; then it is evident that the operations, thus explained, of "logical" multiplication and addition are just as commutative and associative as the arithmetical operations with the same name, which the following formulas express:

$$ab = ba, \quad a(bc) = (ab)c, \quad a+b = b+a, \quad a+(b+c) = (a+b)+c.$$

Because of this, the parentheses can be omitted in products or sums composed of several simple operation terms. Moreover, it is evident that the two operations stand in distributive relation to each other, but not just in one direction (as in arithmetic), but reciprocally. Thus, as it is expressed in formulas:

$$a.(b+c) = (a.b)+(a.c) \quad \text{and} \quad a+(b.c) = (a+b).(a+c).$$

The first to make the latter observation, which I had also made independently, was the American C. S. Peirce (see (4), vol. I).

Obviously, a sum can equal 0 only when each one of its terms equals 0; a product can equal 1 only if each factor also equals 1. Similarly, the little theorems {*Sätze*} expressed in the formulas,

$$a+ab = a, \quad a(a+b) = a, \quad aa = a, \quad a+a = a,$$

which have no analogue in arithmetic, hold as immediately obvious.

Of special note is the first of these according to which terms of a sum which are "contained" {*enthielt*} (included {*mitinbegriffen*}) in other terms (as ab is included in a) may be omitted {*unterdrückt*} at any time.

Once one has convinced oneself—say by thinking about regions of planes—of the validity of the formulas

$$a.1 = a, \quad a+0 = a, \quad a.0 = 0, \quad a+1 = 1$$

(only the first three of which hold in arithmetic),* and then the theorems concerned with negation,

$$a.a_1 = 0, \quad a+a_1 = 1, \quad (a_1)_1 = a, \quad (a.b)_1 = a_1+b_1, \quad (a+b)_1 = a_1.b_1$$

(the latter two of which were partly expressed by Boole and Jevons (1), and first completely expressed by Robert Grassmann), one has acquired everything necessary to understand what follows and indeed many beautiful applications of the logical calculus (such as (8)).

Now, the preceding propaedeutic discipline is converted into the proper logical calculus—more precisely, into the first part of it—or the *calculus of concepts* (where the extension {*Umfang*} of the concept is kept in mind) if one takes a, b, ... as referring to "classes" {*Classen*} of those individuals which fall under the concepts to be investigated, hence which constitute their extension. Then, in this way, 1 will stand for the manifold of all the objects of thought which fall within the sphere of any of the concepts related to the domain under investigation (if necessary Boole's entire "*universe of discourse*" or "*of thought*"). Logical multiplication, then, corresponds to the so-called "determination" of one concept by another,† [logical] addition corresponds to collective union [of sets].

Now, there certainly is a onesidedness in completely disregarding the "*content*" [i.e. intension] of the concept. Also, it should not be claimed that the above [described] calculus has to replace all of logic together with its eventual future development. Nevertheless, it does allow the greatest part of formal logic to date to appear in a new and wonderfully clear light.

That onesidedness, however, is motivated—indeed, justified for the immediate aim—by the fact that many concepts with undoubtedly definite

* The choice of the symbol ∞ instead of 1, to which Wundt (9) is partial, would deprive us also of the first of the three mentioned formulas;[7] though in this way the fourth, less familiar, formula would then conform to arithmetic. Moreover, this symbol [i.e. ∞] would be just as unsuitable for all finite manifolds as the symbol 1, to which he objected, would be for the infinite ones. Over and above this, the applications of the present discipline to the calculus of probabilities undoubtedly urges retention of the [symbol 1].

† Wundt (9) has recently opposed this claim—a point which I intend to consider on another occasion.

[[7] Dudman incorrectly has "would deprive us of the first three of these formulas".]

extension have no existing content [intension] at all. So it is for most concepts which arose through negation; for example, as H. Lotze‡ wittily remarks, for the human mind it remains an ever unfulfillable task to abstract the common characteristics from everything which is not a man—thus from triangle, melancholy, and sulphuric acid—to combine them into the concept "non-man".

Now Frege's "conceptual notation" actually has almost nothing in common with that portion of the logical calculus just characterized; that is, with the Boolean calculus of concepts; but it certainly does have something in common with the second part, the Boolean calculus of judgements.§ The following simple consideration brings us to this [second part of the logical calculus]: the calculus of domains {*Calcul mit Gebieten*} is also applicable to the domain of intervals on a straight line; it is just as applicable to periods of time, if again these are not thought of as measured, but simply taken as manifolds (classes) of the (individual) moments contained in them or also as arbitrary time segments.

Every investigation proceeds from certain presuppositions which are constantly taken as fulfilled throughout the entire course of the investigation. Now, in order to leave eternity out of the question here as far as possible, let 1 stand for the time segment during which the presuppositions of an investigation to be conducted are satisfied. Then let $a, b, c,\ldots$ be considered *judgements* {*Urtheile*} (propositions {*Aussagen*}, assertions {*Behauptungen*}—English equivalent "*statements*") (8), and at the same time, *as soon as one constructs formulas or calculates* (a small change of meaning taking place), *the time segments during which these given propositions are true*. Thereupon, it is obvious from what has been said that one will be in a position to represent—through formulas or equations obeying the laws of the logical calculus—simultaneous holding and mutual exclusion, even one-directional implication (conditional) {*das einseitige Zurfolgehaben* (*Bedingen*)} of the most diverse propositions. The applications which follow will illustrate this sufficiently; and we can now proceed to the main part of Frege's book, which culminates with the section "Representation and Derivation of Some Judgements of Pure Thought". To this end, I must first introduce and explain some of the simplest of the author's schemata.

Frege signifies by ⊢—— a that a holds; which, according to the above, is to be represented in Boolean notation by $a = 1$ or $a_1 = 0$. Frege signifies by ⊢┬— b that b does not hold;[8] that is, that $b_1 = 1$ or $b = 0$. (It is obvious that the latter [of Boole's] ways of writing it could also be

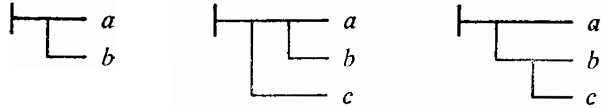
‡ *Logik*, Leipzig, 1874.

§ The title is incorrect in this respect as well, and actually should have been replaced by "Judgemental Notation" {*Urtheilsschrift*}.

[[8] Dudman is missing the negation stroke here.]

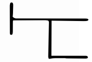
introduced purely conventionally in order to represent, respectively, the truth or falsehood of a proposition—without at all introducing, as Boole does, the intervening time segments; MacColl, among others, does it this way.)

With the first of the schemata,



Frege represents the proposition: When {*wann*} b holds, then a also holds (if not actually necessarily, then at least in fact); that is, in the notation of the logical calculus, $a_1 b = 0$ or also $a+b_1 = 1$—two equations, the first of which asserts that the case in which b holds but at the same time a does not hold does not occur; the second emphasizes that the cases in which a holds or b does not hold are the only possible ones. Also, one equation would be derivable from the other through negation (more precisely, duality {*Opposition*}), since $(a_1 b)_1 = a+b_1$ and $0_1 = 1$.

With the second schema, the author represents the proposition: When {*wann*} b and c both hold, then a holds also; that is $a_1 bc = 0$ or $a+b_1+c_1 = 1$.

With the third schema, which is of fundamental importance for the book, the author unfortunately makes a mistake (p. 7—however, it is the only one which I noticed in the whole book): he gives two explanations which do not correspond with each other; and only the second one, which is correct, is in accordance with all further applications made or intended. In addition, the wording of the assertion represented by the schema is misleading because of the synonymity of the conjuctions "if" {*wenn*} and "when" {*wann*} ("as soon as" {*sobald*}, "in case" {*falls*}, "always, when" {*immer dann, wenn*}, *etc.*) which, though often interchangeable, here yield an essentially different sense. For this reason, it is perhaps instructive to dwell on the point for a moment. The schema is supposed to link the assertion ⊢—— a (that is, that a holds) to the antecedent  (or, $b_1 c = 0$), which is thought of as represented in the manner of the first schema. Hence, it says: a holds as soon as b holds when c holds. More precisely: if we assume that the antecedent of the sentence is fulfilled, then the possibility of $b_1 c$ (that is, c holds, but b does not) is ruled out; then only the possibilities remain which can be summarized in several ways in

$$(b_1 c)_1 = b+c_1 = bc_1+bc+b_1 c_1 = bc+c_1.$$

Now, for all these possibilities which still remain, a should hold. Consequently, this is expressed by the equation

$$a_1(b_1 c)_1 = 0,$$

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in other words $$a_1(b+c_1) = 0,$$

or also $$a+b_1\,c = 1.$$

(The mentioned mistake of the author is only that, basically, he omits the negation of $b_1\,c$ in the first equation. Thus, $a_1\,b_1\,c = 0$ is assigned as the *first* interpretation of his schema, since according to the [assigned] wording, the schema "denies the case in which c is affirmed, but b and a are denied".)*

Now, if we were to take as the meaning of the [third] schema the proposition:

"If b is dependent upon c, then a holds.",

which fairly accurately corresponds to the author's second interpretation (which, properly understood, is correct); then it would seem inconceivable to common sense that this sentence is fully synonymous with the following two taken together:

"If b holds, a holds." and

"If c does not hold, a holds."

And, yet, this is the case, since in fact the equation

$$a_1(b+c_1) = 0$$

can be divided into the two equations

$$a_1\,b = 0 \quad \text{and} \quad a_1\,c_1 = 0.$$

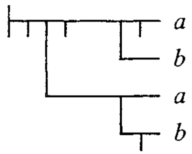
The difficulty arises because, from the wording of the sentence—not only from the use of the grammatical particle "if" instead of "whenever", but also from the designation of the relation as a conditional (as Frege puts it "a necessary consequence")—the reader will tend to make the following interpretation: either b is always dependent (as it were, causally) upon c, and then a surely holds; or else, this is not always the case, and then the proposition is empty {*inhaltlos*}—gives us no information at all about whether a holds or does not hold. This latter is not at all what is intended; on the contrary, even if the conditional of b and c does not come true at times, the schema is to state—assert—something for these times; namely that a holds. The conditional wording,† therefore, misleads one into the

* I employ different letters here, since it seems to me that the author's frequent, utterly unnecessary, change in the choice of letters only detracts from the perspicuity and rather offends good taste.

† For reasons of brevity, I shall have to adopt this wording myself hereafter.

unintended interpretation of the antecedent as holding universally—into an imputation of "generality"—about which, by the way, the author later (p. 19) makes some very pertinent remarks.

Now, in order to represent, for example, the disjunctive "or"— namely, to state that *a holds or b holds, but not both*—the author has to use the schema



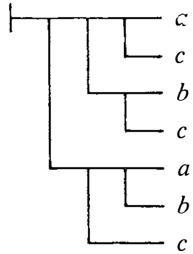
which definitely appears clumsy compared to the Boolean mode of writing:

$$ab_1+a_1\,b = 1$$

or also $$ab+a_1\,b_1 = 0.$$

From the section "Representation . . . of Some Judgments of Pure Thought" I cite an example as an illustration:

Nr. 2



This should be read: if a is dependent upon b and c, and b is dependent upon c, and c holds, then a holds. In the Boolean fashion, this would be expressed: if $a_1\,bc = 0$ and $b_1\,c = 0$, then it is also the case that $a_1\,c = 0$. Here is the proof:

$$a_1\,c = a_1(b+b_1)c = a_1\,bc+a_1\,b_1\,c = 0+a_1.0 = 0.$$

Of course, we can also write it all in a *single* formula; namely, as you like:

$$a+c_1+b_1\,c+a_1\,bc = 1$$

or also $$a_1\,c(b+c_1)(a+b_1+c_1) = 0.$$

The latter form is the easiest to verify as an identity (by cross-multiplication), since in this way factors keep coming together which, as negations of each other, mutually cancel and yield the product 0. Also, from the latter form, one can easily derive (going from right to left) the interpretation desired by the author, by taking it as a guideline that if, in one of the products equal to 0—A, B, C ... = 0—some of the factors—... C, B ... are considered equal to 1 (that is, are taken as true), then (the product of) the remaining factor(s) A vanishes (that is, A_1 must be true).

Let no one conceive it an advantage of the Fregean notation that it employs only one mode of connection of its judgement (or better, inference) links,[9] while Boole's calculus, except for negation (to be sure, also abundantly used by the author), needs two kinds (+ and ×) of linking operations; for it can be shown that the latter [that is, Boole's calculus] can get by with just one—and indeed in four ways. Written with only multiplication, the latter formula, for example, runs as follows:

$$a_1\, c(b_1\, c)_1(a_1\, bc)_1 = 0;$$

and the dual would also yield a formula with only addition. On the other hand, since an equation is itself an assertion, nothing prevents our allowing an equation to occur as a logical factor, etc.; thus writing

$$a_1\, c(b_1\, c = 0)(a_1\, bc = 0) = 0,$$

to which there is again a dual.

The author also uses identities as inference links. Perhaps Robert Grassmann was the first to introduce formulas as operation links; yet, it seems to me, [he did it] in an illicit way, not conforming to the principles of his own calculus, always linking them by means of the plus sign instead of the multiplication sign.

Besides, it has no great value; it verges upon pedantry to actually express the theorems with one single connective each time they occur. One can be justly satisfied to have recognized once and for all the theoretical possibility of doing it.

We list here a few further "judgements of pure thought", with the numbers given them by the author, in a form of notation modelled upon the Leibnizian–Boolean calculus:

$$(1)\ \ a_1\, ba = 0, \quad (5)\ \ a_1\, c(b+c_1)(a+b_1) = 0,$$

$$(7)\ \ a_1\, cd(b+c_1+d_1)(a+b_1) = 0, \quad (11)\ \ a_1\, b(a+b_1\, c) = 0,$$

$$(12)\ \ a_1\, cbd(a+b_1+c_1+d_1) = 0, \quad (21)\ \ a_1(b+c_1)(c+d_1)(a+b_1\, d) = 0,$$

$$(24)\ \ a_1\, bc(a+c_1) = 0, \quad (27)\ \ a_1\, a = 0, \quad (28)\ \ ba_1(a+b_1) = 0,$$

$$(33)\ \ b_1\, a_1(a+b) = 0, \quad (46)\ \ a_1(a+c_1)(a+c) = 0,$$

$$(51)\ \ a_1(b+c)d(a+b_1)(a+c_1+d_1) = 0, \text{ etc.}$$

[[9] Dudman's turn of phrase.]

All of the "judgements etc." which the author compiled and derived on pages 25 to 50 could easily be rendered in the above manner on *half* a printed page; and would, at the same time, immediately show themselves as evident (through mental cross-multiplication); namely, by leading to identity (27). In fact, the author's formula language not only indulges in the Japanese practice of writing vertically, but also restricts him to only *one* row per page, or at most, if we count the column added as explanation, two rows! This monstrous waste of space which, from a typographical point of view (as is evident here), is inherent in the Fregean "conceptual notation", should definitely decide the issue in favour of the Boolean school—if, indeed, there is still a question of choice.

In other respects, the numerous "judgements, etc." presented by the author seem to be logical identities (from which I sought above to pick out only the most interesting) which, for the most part, offer nothing especially interesting. Also to be criticized with regard to the arrangement and choice of theorems is the really enormous lack of systematization (which, to be sure, is acknowledged in the title of the section).

In addition, numerous *repetitions* also occur—statements differ only with regard to the order in which the factors appear or only in that an element is replaced by its double negation. After the commutativity of the former or the substitutivity of the latter is recognized and exhibited in the simplest possible schema, it seems hardly worth also expressing it again and again in complicated examples. Finally, expendable premisses (that is superfluous ones contained in others) of judgements or deductions—and thus some of these deductions themselves—should have been suppressed; see Nos. (3), (4), (32), (45).

The author's method of deduction consists essentially, either directly or indirectly, of enumerating and summarizing which cases remain if one eliminates from all imaginable ones those excluded by the premisses.

The preceding criticisms do not concern the clarity and readability of the book, which in other sections offers something of more value; and anyone who cares to do so will *easily* be able to translate into the better notation in accordance with the examples given above.

There is a defect in Boole's theory, perceived by many and recently very effectively illustrated by Wundt (9) against Jevons, in the fact that particular judgements are only inadequately expressed in it (strictly speaking, not at all). The indeterminate factor v, which Boole uses, for example, in the first part of the logical calculus in the form $va = vb$ to express the sentence "Some a's are b's.", does not fulfil his purpose because, through the hypothesis $v = ab$, this equation always comes out an identity, even when no a is a b. Now in the section concerning "generality", Frege correctly lays down stipulations that permit him to express such judgements precisely. I shall not follow him slavishly here; but on the contrary, show that one may not perchance find a justification here for his other deviations from Boole's notation, and the analogous modification or extension can easily be achieved in Boolean notation as well. The author achieves this essentially by introducing Gothic letters as symbols for generality and establishing a notation for negating this generality—for which I shall use a stroke above [the symbol in question]. The equation $f(\mathfrak{a}) = 1$ asserts: all $\mathfrak{a}$'s have the property f. Then $\{f(\mathfrak{a})\}_1$, or more briefly $f_1(\mathfrak{a}) = 1$, will assert: all $\mathfrak{a}$'s have the property not-f; that is, all $\mathfrak{a}$'s lack the property f. On the other hand, $f(\bar{\mathfrak{a}}) = 1$ will assert: not all $\mathfrak{a}$'s have the property f, or: some $\mathfrak{a}$'s do not have the property f. The equation $P(\mathfrak{a})M(\mathfrak{a}) = 0$ asserts (also in agreement with Frege): no M is a P. Then, the equation

$$P(\bar{\mathfrak{a}})M(\bar{\mathfrak{a}}) = 0$$

will deny that the previous equation would be true for *every* meaning one could assign to $\mathfrak{a}$; hence, expressing that there is at least one $\mathfrak{a}$ for which [the first equation] would be false, or [in other words] that some M's are P's, etc.

By the way, one can adopt various methods to accomplish the same thing; for example (the basic idea of Cayley), through a sign such as $\neq$ for "not equal", in which case $va = vb$ together with $va \neq 0$ (or, even shorter, $ab \neq 0$) would say that some a's are also b's. Peirce (4, I) corrected the mentioned deficiency in another way.[10]

The explanation which the author gives for the concept of (logical) "function" is very broad and entirely original. It is much broader than all previous explanations and to me seems to be not without justification. With regard to this, however, because of limited space, I wish to refer [the reader] to the book itself and merely mention that through the kindness of the publisher I have received an offprint from the *Sitzungsberichte der Jenaischen Gesellschaft für Medizin und Naturwissenschaft* (1879, the session of 10 January) in which the author presents two applications of his "conceptual notation"—one regarding the expression of a geometrical relation (that three points lie on a straight line), the other regarding a number-theoretical theorem—which are indeed appropriate to elucidate the way in which he intends to apply his "notation"—though less appropriate to indicate its value.

The "appendix" [Ch. III] of the *Conceptual Notation* concerns "Some Topics from a General Theory of Sequences" and appears very abstruse—the schemata are ornate with symbols! Here it would be desirable that if new symbols do have to be introduced for certain complicated relations which are expressible in the existing system, simpler ones should be chosen (even at the expense, temporarily, of complete expression {*Ausdrucksfülle*}). Three such relations occur, which concern [1] the following of one element after another in a certain "sequence", which is left very indeterminate; [2]

[10 Dudman left out the reference to Peirce.]

the "inheritance" of a property in the same [kind of "sequence"] by one element from the previous element; and [3] the "many-one-ness of a (not further characterized) procedure". The "sequence" is characterized only by the fact that a certain kind of advancement (which is otherwise left general) from one element to another is possible—I want to say, perhaps, that a particular procedure of deduction leads from one element to another. Of course, the deductive paths here can eventually also intersect, branch off, and run together again; and the author is proud of the great generality that is given in this way to the concept of *sequence*. It seems to me, however, that there is absolutely nothing of value in such a generalization; on the contrary, in my opinion, if the graphic ordering[11] of elements along a straight line is immaterial, unfounded, or inadmissible, then instead of "sequence", one should use simply the designation "set" {*Menge*}, "system" {*System*}, or "manifold" {*Mannigfaltigkeit*}.

According to the author, he undertook the entire work with the intention of obtaining complete clarity with regard to the logical nature of *arithmetical* judgements, and above all to test "how far one could get in arithmetic by means of logical deductions alone". If I have properly understood what the author wishes to do, then this point would also be, in large measure, already settled—namely, through the perceptive investigations of Hermann Grassmann. However, given the considerable extent of the literature related to this effort, it seems in any case not unjustified to wish that the author had taken better account of already existing efforts. May my comments, however, have the over-all effect of encouraging the author to further his research, rather than discouraging him.

In conclusion, I believe I shall earn the thanks of all those interested in the more recent analytical development of logic (and at the same time fulfil an obligation to the works that were unknown to me when I wrote my book) (6), if I give below a list of the relevant works of which I am now aware, though they cannot be found in the Bibliography of (6).

(1) William Stanley Jevons: *Pure Logic, or the Logic of Quality Apart from Quantity, with Remarks on Boole's System and on the Relation of Logic and Mathematics.* London and New York, 1864. 87 pp.

(2) ——: *The Substitution of Similars, the True Principle of Reasoning, Derived from a Modification of Aristotle's Dictum.* London, 1869. 86 pp.

(3) ——: *The Principles of Science, a Treatise on Logic and Scientific Method*—a very significant work, whose 3rd edition,[12] London, 1879, 786 pp., is now before me.

[11 Dudman's turn of phrase.]
[12 {*dessen 3. Aufl.*} Dudman renders this "whose three volumes".]

(4) Charles S. Peirce: (Three papers on logic, read before the American Academy of Arts and Sciences) I. "On an Improvement in Boole's Calculus of Logic", pp. 250–61. II. "On the Natural Classification of Arguments", pp. 261–87. III. "On a New List of Categories", pp. 287–98. *Proceedings of the American Academy of Arts and Sciences* (1867). Article I anticipates various results at which the present reviewer arrived in (6).

(5) ——: *Description of a Notation for the Logic of Relatives Resulting from an Amplification of the Conceptions of Boole's Calculus of Logic*, extracted from the *Memoirs of the American Academy*, vol. IX. Cambridge, 1870. iv+62 pp.

(6) Ernst Schröder: *Der Operationskreis des Logikkalkuls*. Leipzig: Teubner, 1877. 37 pp.

(7) J. Delboeuf: *Logique algorithmique*. Liège and Bruxelles, 1877. 99 pp.

(8) Hugh MacColl: "The Calculus of Equivalent Statements and Integration Limits", *Proceedings of the London Mathematical Society*, vol. IX (1877–78), pp. 9–20, 177–86. The first part gives an interesting application of the logical calculus to the (purely mechanical) solution of problems: to determine the new limits, if with repeated integrations between variable limits the sequence of integration is modified as desired. The second part is spoiled by the fact that the author introduces symmetrical symbols (: and –) to express the asymmetrical relations of subordination and non-subordination, as a result of which he certainly gets himself confused.

(9) Wilhelm Wundt: *Logik, eine Untersuchung der Principien der Erkenntniss und der Methoden wissenschaftlicher Forschung*, vol. I: *Erkenntnisslehre*. Stuttgart, 1880. 585 pp.—The work devotes 52 pages to the logical calculus. Even if some details in it may be criticized, we should welcome the fact that professional philosophers are beginning to concern themselves with the *mathematical reform of logic*, which certainly deserves consideration.

Karlsruhe ERNST SCHRÖDER

E. Review of Frege's *Conceptual Notation*[1] by P. Tannery, *Revue Philosophique*, 8 (1879), pp. 108–9.

The author attempts to establish a system of symbolic notation applicable to all types of judgement, to all modes of reasoning. His small book

[[1] The present review appeared originally in French. Tannery gives the following French translation of the title of Frege's book: *Représentation écrite des concepts, système de formules construit pour la pensée pure d'après celui de l'algèbre*. In a footnote, he reports that a literal translation (into French) of the title would be nearly unintelligible.]

contains little more than an explanation of the symbols which he believes he must adopt and the combinations which he forms with them. They differ essentially from those of algebra: the two algorithms have nothing in common but their use of letters. On the other hand, the logical point of view is most unique.

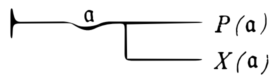
In such circumstances, we should have a right to demand complete clarity or a great simplification of formulas or important results. But much to the contrary, the explanations are insufficient, the notations are excessively complex; and as far as applications are concerned, they remain only promises.

Dr. Frege has very few illusions about the greeting which the present work will probably receive. To defend it, he compares ordinary language to the human eye and his "conceptual notation" to the microscope, a valuable instrument, but one too difficult to use outside of the special studies for which it was meant. The author intends to apply his invention at first to arithmetic. With it, he plans to illuminate the concepts of number, magnitude, and so on. We strongly advise him, if it attains his goal, to *project* a given image with his microscope; that is, to translate his arguments into ordinary language.

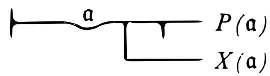
It will suffice for the moment to indicate the salient point of his system as far as logic is concerned. The [author] abolishes the concepts of *subject* and *predicate* and replaces them by others which he calls *function* and *argument*. Thus, 'the circumstance that carbon-dioxide is heavier than hydrogen' and 'the circumstance that carbon-dioxide is heavier than oxygen' can be considered indifferently either as the same function with different arguments (hydrogen, oxygen) or as different functions with the same argument (carbon-dioxide). We cannot deny that this conception does not seem to be very fruitful.

If we wish an example of the notation, this is how the four kinds of propositions usually considered in logic are rendered:

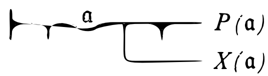
(a) All *X* is *P*.



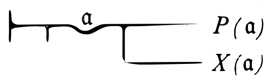
(e) No *X* is *P*.



(i) Some *X* is *P*.



(o) Some *X* is not *P*.



The first symbol-combination can be analysed this way:
The vertical stroke on the left indicates that a judgement is affirmed.