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BOOK REVIEWS

L. E. J. Brouwer collected works, Volume I, *Philosophy and foundations of mathematics*, A. Heyting, ed., North-Holland/American Elsevier, Amsterdam, New York, 1974, 628 + xv pp., \$96.25.

This book contains the principal sources of intuitionistic logic and mathematics, Brouwer's particular brand of so-called constructive foundations (c.f.). To understand it, one must compare its merits and defects with those of other, better known versions of c.f. (including incidentally the bulk of Brouwer's early writings on c.f.).–To put first things first: Brouwer's final version is incomparably more imaginative. The commonplace versions are preoccupied with the business of 'pure' existence theorems $\exists x A(x)$ and the search for 'explicit' realizations t: $A(t)$. For the silent majority of mathematicians this business is hardly dramatic: there is nothing to stop one from presenting such t even if one does not reject pure existence theorems. What is more, mathematics has developed a whole arsenal of notions for stating significant differences between such t, much more pertinent than the crude idea of an 'explicit' t or the crude distinction between 'constructive' and 'nonconstructive' (definitions of) t.[1] Commonplace c.f. constitute a *restriction*, and thus form a proper part of ordinary mathematics–usually accompanied by grand, but dubious foundational (cl)aims, to which we return later on.

Brouwer's version of c.f. is *incomparable* with ordinary mathematics. On the one hand it does not contain higher set theory with the (transfinite) iteration of the power set operation applied to infinite sets. On the other it includes as principal objects of mathematical study (i) choice sequences of various kinds, for example, (the idealization of) the random sequences of throws of a die, and

[1] Specialists, for whom this and other footnotes are intended, may want some documentation. (i) A happy coincidence shows the *appreciation* by the Mathematical Establishment of significant 'explicit' realizations. Without much exaggeration: a Fields Medal was awarded in 1958, to Roth, for the 'pure' existence theorem $\forall n \exists q_0 \forall p \forall q (q > q_0 \to |\sqrt[3]{2} - p/q| > q^{-2-1/n})$, and another one in 1970, to Baker, for the 'worse' result $\exists q_0 \forall p \forall q (q > q_0 \to |\sqrt[3]{2} - p/q| > q^{-3+0.05})$ where, however, a (manageable) value for q_0 was supplied. So much for blind prejudice against an appropriate search for explicit realizations. (ii) In (topological) algebra, one asks whether for polynomials of odd degree, say a cubic with leading coefficient 1, a (real) *zero* is determined continuously in the coefficients. The answer is *No* for the field **R** with the usual topology (take $x^3 - 3x - c$); the answer is *Yes* for what Brouwer called *real number generators* (r.n.g.) such as binary expansions with the product topology provided the usual equivalence relation for r.n.g. need not be respected. (Quite generally, Brouwer's insistence on r.n.g. is appropriate when continuity is paramount: a continuous mapping from **R** into a discrete space is constant, but not for r.n.g.) (iii) In analysis, one asks about Brouwer's fixed point theorem, for the uniform convergence topology of mappings f of, say $S^2 \mapsto S^2$: Is there a continuous $\zeta: f \mapsto x \in S^2$ such that $f[\zeta(f)] = \zeta(f)$? The answer is *No* (also for r.n.g. x): approximations to a fixed point of f are not generally determined by approximations to f (and so one need not even ask if they are 'constructively' determined).—Once the attention of mathematicians is drawn to the ideas involved in (i)–(iii), their relevance is plain without any foundational preoccupation. Incidentally, several questions seem still open, for example: Are there topological versions of Hilbert's Nullstellensatz or of Artin's solution of Hilbert's 17th problem for complex (resp. real) coefficients (or their generators)?

(ii) proofs which enter into a new meaning of the familiar logical operations, the new meaning being used to state laws or 'axioms' concerning (i). In contrast, ordinary mathematics which (of course) *uses* proofs as tools, does not make them explicit objects of study, and *paraphrases* properties of random sequences, for example, in measure-theoretic or set-theoretic terms. Brouwer himself did not give full fledged axiomatic theories of (i), let alone of (ii). But it may fairly be said that modern axiomatic theories, especially of (i), stand in much the same relation to Brouwer's writings, as modern axiomatic theories of sets stand to Cantor's.[2] While it would be quite inappropriate to go here into the details of axiomatic theories of (i) or (ii), it seems worthwhile–and easy!–to present the general idea.

Quite naively: 'freely chosen' sequences, s, say of natural numbers are thought of as–necessarily– 'incomplete', only finite initial segments being 'given'; so *all* operations on such s must be continuous for the product topology. (Thus, if we think of euclidean spaces as including all points given by such freely chosen sequences, Brouwer's theorem on the invariance of dimension, mentioned in footnote 2, holds for *all* 1-1 mappings, bicontinuity now being a consequence of the (new) conception of euclidean space.) More generally, if P is an arbitrary predicate of s, where s is *freely* chosen (no restriction on the choices being allowed except for a finite initial segment), we expect

$$(*) \qquad \forall s[P(s) \rightarrow \exists n \forall s'\{(\forall m \leqslant n)[s(m) = s'(m)] \rightarrow P(s')\}]$$

inasmuch as $P(s)$ can only 'depend' on a finite segment of s. Evidently, in (*) the logical particles cannot have the usual truth functional meaning given in texts on logic, since (*) plainly contradicts $p \vee \neg p$; take $\exists n[s(n) = 0]$ for p, $\neg p$ for $P(s)$, and s such that for all 'given' initial segments $s(n) \neq 0$. There is nothing dramatic about all this: in ordinary reasoning we relatively rarely use the truth functional meaning such as: q is true or p is false, for 'p implies q'. The latter meaning happens to have a particularly simple theory. Brouwer indicated, and Heyting, the editor of this volume, developed another meaning of the logical particles, well adapted for an analysis of (*)–but perhaps even further removed from most ordinary reasoning than the truth functional meaning in the logic books. The new meaning is well illustrated by the case of implication.

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[2] A readable account is in *Choice sequences, a chapter of intuitionistic mathematics*, by A. S. Troelstra, to appear in the Oxford Logic Notes (Oxford University Press). The parallel between (theories of) sets and choice sequences goes further. (i) The-currently-most successful theories do not treat the most general notions involved but rather (the cumulative hierarchy of) those sets which are generated from ϕ by iterating the power set operation, resp. lawless sequences—and there is no evidence that, even if there are such things as 'the most general' notions, of set or choice sequence, they would lend themselves to a rewarding theory. (ii) In fact, our knowledge of the cumulative hierarchy and of lawless sequences is–at the present stage–most effective when applied to other notions defined in terms of those things: we know more about the so-called constructible sets, L, than about the full cumulative hierarchy (used to define L), and have more applications of so-called projections of lawless sequences than of the latter.–Incidentally, there is a little-known overlap in the interests of Cantor and Brouwer, the founders of the theories of sets and choice sequences. Ever since 1877, Dedekind and Cantor speculated that regions of different *dimension* are not in 1-1 *bicontinuous* correspondence; only Cantor's attempted proof was defective [Nachr. Ges. Wiss. Göttingen (1879), 127–135].

First of all, the *data determining a proposition*, p, are not simply truth values (true or false). This would obviously be inappropriate for $P(s)$ when s is 'incomplete'. Instead, we have a condition, C_p, determining what are *proofs* of p (not: whether or not there is a proof of p). Then $C_{p \to q}$ is built up from C_q and C_q as follows: by definition, we require an operation I and an argument, say π_0:

$$\text{For any } \pi,\ C_p(\pi) \Rightarrow C_q[I(\pi)],(*) \text{ for short,}$$

$$\text{and } \pi_0 \text{ establishes } (*),^3$$

where π consists, hereditarily, of operations and arguments.–The reader can guess the corresponding explanations for other particles. The new meaning satisfies different formal laws from the truth functions, for example, as we have seen: $\neg \forall p(p \vee \neg p)$. Clearly, (*) must be expected to be quite sensitive to the domain Π of the π; by restricting Π, one restricts the domain on which I must satisfy (*), and thus increases the possibilities of proving $p \to q$; but one also restricts the permitted range of I, and thus decreases those possibilities. Because of that sensitivity, no one set of formal logical laws can be expected to be 'fundamental'–in contrast to ordinary first order logic familiar from model theory. As a so-to-speak positive counterpart, the particular formal laws first stated by Heyting for the intuitionistic meaning apply also to situations only quite vaguely related to constructions in the literal sense (meant by Brouwer); for example, they apply to particular 'explicit' definitions in axiomatic set theory involved in (weak) forcing or to certain 'uniform'

[3] There is an obvious–though not necessarily vicious! –circularity here, unless $\Rightarrow$ in (*) is different in 'kind' from $\to$. The usual idea is that the conditions C_p are decidable, and so $\Rightarrow$ has simply its truth functional meaning. For coherence, this then requires that it be decidable, for any pair (I, π_0), whether or not π_0 establishes (*) for variable π. All this would not only be pretentious, but genuinely dubious if we were realistically thinking of arbitrary proofs and operations. But it makes good sense when applied to a wide range of proofs and operations, hereditarily formalizable in various 'logic-free' systems. For reference in footnote 7 below: For a given (proposition) p, the range of I must be restricted, corresponding to the quite naive malaise about the idea of a totality of all possible proofs (of any one proposition).–Speaking of circularities, the notion of a *continuous* operation F on choice sequences s would also be circular if defined in the usual way by

$$\forall s \exists n \forall s' \{(\forall m \leqslant n)[s(m) = s'(m)] \to F(s') = F(s)\}$$

since, for choice sequences s, the quantifier combination $\forall s \exists n$ is in turn required to be continuous. Without stating this issue, Brouwer proposed an independent 'inductive' definition of a notion, $\mathfrak{B}$, of continuous operation (or rather of an arbitrary operation on choice sequences!): Constant operations $\in \mathfrak{B}$, and if each $F_n \in \mathfrak{B}$ so does F^* where $F^*(s) = F_{s(0)}(\vec{s})$ and $\vec{s}(n) = s(n+1)$. (For the meaning of continuity in ordinary mathematics, $\mathfrak{B}$ is demonstrably the class of continuous operations.) In connection with $\mathfrak{B}$, Brouwer introduced the idea of 'fully analyzed', possibly *infinite* proofs; cf. his footnore 9 on p. 394. This idea, developed by Gentzen for formal systems–with *cut-free* replacing *fully analyzed*–and extended beyond expressions of the particular form $\forall s \exists n$, has been very influential in proof theory.

Putting together the analyses of choice sequences sketched here, one gets a useful *elimination theorem.* For any formula A containing (possibly) bound, but not free, variables for choice sequences there is an A_e not containing any such variables: $A \longleftrightarrow A_e$. Of course, this result does not make choice sequences useless—no more than any other discovery in mathematics of a relation between two sets of notions makes one of them useless; cf. footnote 9.

definitions in category theory applied to sheaves or Cartesian closed categories. It is fair to say that at least the elementary exposition of such 'constructions' benefitted from experience with formal intuitionistic logic.[4]

So much then for the extension of ordinary mathematics by detailed systematic developments of specifically intuitionistic notions. In the reviewer's opinion they have substance and some mathematical wit. But it can hardly be claimed that they (ought to) have a central place in the 'mainstream' of mathematics. As is clear from footnote 1, the vague general ideas which preceded those developments have been 'absorbed' in ordinary mathematics (without any logic-chopping). Those ideas certainly fired the imagination of mathematicians like Poincaré and the young Brouwer. (Contrary to an almost universal misunderstanding, Brouwer's work in topology was *preceded* by his interests in constructivity, for example, in his dissertation now available in English, and followed by his work on choice sequences.) Those two constructivists are associated with the switch to algebraic topology operating on finite 'pieces' from set theoretic topology in the style of Schoenflies; for an interesting account, by Newman, see Biographical Memoirs of Fellows of the Royal Society, vol. 15, The Royal Society, London, 1969, p. 47. But also more modern developments in ordinary mathematics are related to vague, general preoccupations of constructivists, for example, how objects are 'given' to us: elementary category theory points out the consequences of 'giving' a function by its graph together with a bound on its range; even though the exact range is determined by the graph, the passage involved may require an operation not in the category considered. Bishop's book[5] illustrates this state of affairs very well (in effect if not by intention): leaving aside the introduction, the style is perfectly familiar to the modern mathematician.

[4] The sensitivity of (*) to the choice of domain Π should be compared to the sensitivity of (ordinary) *second order logic* to the class C of sets involved in the (set theoretically explained) meaning of logical formulas. It was a discovery that the validity of first order formulas (which is of course also defined set theoretically) is remarkably insensitive to C: once the set of natural numbers and so-called Δ_2^0 subsets are included in C, the validity of first order formulas is stable. Without any evidence to the contrary, the patent sensitivity of validity in the case of intuitionistic logic suggests that the latter cannot be expected to be often useful (in intuitionistic mathematics). Without going into the particular consideration above, Brouwer was certainly skeptical of the rôle of logic (tacitly, in his kind of mathematics). Incidentally, he was also skeptical of the logical importance of another favorite of present-day logicians: higher types; cf. pp. 462–464, or at least the title! Without denying the elegance of the language of higher types, after 20 years of experimentation this reviewer shares Brouwer's view. After all, even in set theory, axioms insuring the existence of higher types, like the replacement axiom, have little proof power unless combined with the power set axiom.

[5] *Foundations of constructive analysis*, N.Y., 1967, reviewed in Bull. Amer. Math. Soc. **76** (1970), 301–323. It will not have escaped the reader's notice that the review compares Bishop's exposition to impressions current in the twenties (or to views of those like Fraenkel whose interests turned elsewhere at the end of the twenties); that is, *before* the very significant progress in the thirties by Gödel, Gentzen, and others. For example, some 40 years ago Gentzen realized that a great deal of mathematical analysis can be formalized by use of quite weak 'existential' axioms; cf. his asides on p. 136 and p. 200 of *The collected papers of Gerhard Gentzen*, M.E. Szabo (editor), North-Holland, 1969; reviewed J. Philosophy **68** (1971), 238–265.–Indeed, when in the fifties people began to look seriously for theorems of ordinary mathematics not derivable from such weak axioms, they had to go to such curiosities as the theorem of Cantor-Bendixson.–Formalizations in weak (classical) systems provide, automatically, 'constructivizations' by means of functional interpretations discussed in footnote 9.

Brouwer's foundational critique. As everybody knows, and as the title of this volume indicates, Brouwer's own case for the work here collected had little to do with extending our ordinary (view on our knowledge of) mathematics, but with *correcting* it. There is wide-spread misunderstanding concerning his specific critique. (i) He by no means confined himself to *finite* objects: in fact, the 'fully analyzed' proofs mentioned in footnote 3, are infinite. (ii) He was not tempted by hackneyed generalized doubts about the legitimacy of abstract notions: he studied proofs, that is, thoughts, which he distinguished emphatically from the linguistic objects used to represent them; for example–but not necessarily–formal derivations of some specific formal system. (iii) Though he saw defects in the theory of sets (tacitly: as presented in the first decade of this century), the antinomies were not particularly prominent in his critique.[6]

What Brouwer did do in his foundational critique was really absolutely orthodox–at least since Kant, and particularly in the first quarter of this century. According to Brouwer, our ordinary view neglects the *role of the subject* (called 'observer' in physical contexts; it goes without saying that this stress on the subject acquiring knowledge got a boost from Einstein's singularly successful use of the observer in his special theory of relativity, shortly before Brouwer's dissertation). A natural and–in the short run–effective reaction to the neglect of anything is to make it the sole object of study. Brouwer's version of c.f. is an instance of this: as described on p. 88 above, he made *proofs*, that is, the activity of the subject (also called 'creative subject' by Brouwer and 'ideal mathematician' by others, as in 'ideal fluid'), part of the meaning of mathematical assertions. Although similar 'subjectivist' analyses of scientific knowledge have been proposed in the philosophical literature for all sorts of other sciences, nothing in that literature seems as imaginative as Brouwer's notions introduced in his attempt at a purely subjectivist analysis of mathematics. If the latter has not gone very far, this is surely partly due to inherent weaknesses in the scheme itself; but probably

[6] It is perhaps natural that the antinomies are often used–in effect if not by intention–to introduce a bit of drama into foundations, a subject by and large devoted to the undramatic business of 'analyzing' what (we believe) we know anyway. But it is simply historically false to think that the antinomies provide evidence for any failure of the 'logical intuitions' of Cantor, let alone of his contemporaries (who were *over*suspicious of his notions). Here are the facts. Back in 1885, in a review (of Frege's *Grundlagen*) easily accessible in Cantor's *Collected works*, he objected to Frege's: (*)$\exists x \forall y[y \in x \leftrightarrow P(y)]$ for the precise reason that precautions are needed to ensure that the predicate P has an extension which can be comprehended (as a 'unity'). And Frege himself, in the introduction to the *Grundgesetze*, discussed the possibility that (*) might be–not only false for the intended meaning, but–formally contradictory. His own malaise is apparent from the thoughtless 'evidence' proposed loc. cit. for (*), namely its wonderful consequences (which, at best, provide a reason for our interest in (*), certainly not for its validity or consistency). –As this reviewer reads Brouwer's diatribes against set theory, for example, in his dissertation, the main source of his 'gut reaction' seems to have been less the topic of (infinite) sets than the extraordinarily pretentious claim for set-theoretic foundations in Russell's *Principles of mathematics*, as providing the true analysis of all mathematical concepts (and that the formal deductions in axiomatic set theory analyze all mathematical reasoning). Brouwer surely had a point. Though even today, set theory is better known as a 'general framework' for mathematics than as a branch of mathematics (which, after a long period of stagnation, has made remarkable progress in the last 15 years), the value of this or any other 'general framework' is dubious: axioms are given in the first chapter of a text, but hardly ever has one occasion to refer to them later (in any detail).

even more because he was preoccupied with hackneyed traditional questions such as: Is mathematics about an external reality or about our own 'free' constructions? (Those questions are so banal that we ask and understand them when–ontogenetically or phylogenetically speaking–we know next to nothing (about mathematics); reflecting only, as somebody said, *wie sich der kleine Moritz die Dinge und das Denken vorstellt.*[7]) More specifically, Brouwer remained hung up on the validity of (principles of) proofs, neglecting more 'structural' relations between proofs, and between proofs and other things.

The famous dispute between Brouwer and Hilbert. To put first things first, Brouwer and Hilbert were in the same camp, accepting only constructive principles as *prima facie* legitimate. The difference was elsewhere. Brouwer's principal concern was to develop constructive mathematics, without the distraction of studying metamathematically (by constructive means) the principles of ordinary mathematics. Hilbert wanted to 'justify' the latter by using (i) formalizations $\mathfrak{F}$ of valid principles for then-current mathematical concepts and (ii) proving the consistency of $\mathfrak{F}$ (tacitly, constructively): he was convinced that (ii) did not need any development of constructive mathematics because he thought that so-called *finitist* methods (of which proofs in

[7] Of course, for this very reason the questions have a perennial pedagogic interest (at least, for the untamed spirits among us).–If the reference above to the business about external reality and our own constructions (discovery and invention) appears irreverent, the reader should stop to give second thoughts to the favorite implications attributed to this matter, for example, concerning *certainty* (of mathematical knowledge): we are supposed to be peculiarly certain of our own–mental, presumably not necessarily also of our physical–productions. Is the idea already mentioned in footnote 3 of all possible proofs of any **one** theorem (or all possible definitions of any **one** object, say the empty set) as clear, let alone clearer than the idea of the collection of all subsets of say ω? A second favorite is the would-be dramatic 'conflict' between *external reality and our free constructions.* Getting knowledge of any (external) reality requires activity or constructions on the part of the subject; the bit about their being 'free' is particularly unconvincing since they are certainly not made by consciously arbitrary choices, no more so than constructions of material tools (which are limited by the properties of the material available, quite apart from the intended purpose). Besides, why expect a conflict between our own possibilities and the external reality in which we have evolved? –To avoid misunderstanding: all this pretentiousness does not discredit, by itself, all foundational questions *like* those that excited Brouwer; early speculations on the question: What is matter (made of)?–which used to be considered philosophical–were pretentious too, and spiced with such 'conflicts' as: Matter is atomic *versus* All is flux. (Even using hindsight we would be hard put to find the 'conflict' in what Born called: the restless universe of atoms.)–*Historical note*: It hardly seems an accident that the great interest in–set theoretic or constructive–foundations in the first quarter of this century coincided with the huge success of the atomic theory: if the physical world can be built up from a few basic elements, why not mathematical concepts (Whitehead-Russell) or proofs (from a few basic intuitions; Brouwer)? But mathematical foundational schemes lack some of the most obviously essential features of modern atomic theory (not: of early generalities about atoms). (i) The basic foundational elements, such as sets, are really quite close to objects of ordinary mathematical experience: do they even *look* fundamental enough for analyzing in any depth the great diversity of mathematics? (ii) Where are the analogues to geometric relations and binding forces between atoms so essential for refining (crude chemical) atomic theory? (iii) On the 'phenomenological' level, foundations lack the analogues to such prerequisites of the atomic theory as the isolation of chemically pure substances, let alone the periodic table.–One wonders whether our experience of mathematics is at a comparable stage to that of physics and chemistry which was, patently, needed for progress on the structure of matter.

'elementary number theory' are typical) would be enough.[8]

Brouwer was in any case dubious about the adequacy of any formalizations–without, however, getting anywhere near the precise incompleteness results established by Gödel. (Why all the fuss about a consistency proof for a necessarily-incomplete $\mathfrak{F}$ if tomorrow we can think of stronger principles $\mathfrak{F}^+$ which are also valid?) More importantly, Brouwer objected strongly to consistency as a *sufficient* condition on $\mathfrak{F}$, quite apart from the secondary matter of the methods used in a consistency proof. And he surely had a point.

What *does* consistency of $\mathfrak{F}$ insure (for $\mathfrak{F}$ of the kind considered in Gödel's incompleteness theorem)? Suppose A is an 'elementary' proposition, for which, by work of Matyasevic, $(\forall n_1 \in \omega) \cdots (\forall n_9 \in \omega)\ (p \neq 0)$ is typical, where p is a polynomial in 9 variables with integral coefficients. If (the translation in $\mathfrak{F}$ of) A is derivable in–or even only formally independent of!–$\mathfrak{F}$ then the diophantine equation $p = 0$ has no solutions. For if it had, a counterexample to A could be computed, and so $\neg A$ would be formally derivable in $\mathfrak{F}$. But *this is all*, in the following precise sense. If such an A is not derivable in (a necessarily consistent) $\mathfrak{F}$, then $\mathfrak{F} \cup \{\neg A\}$ is also consistent, though, as we have just seen, $\neg A$ is false. Thus *consistency by itself does not even insure the truth of* (formally derived) *purely existential arithmetic theorems*.

Hilbert's pious rhetoric, as saviour of classical analysis against Brouwer's Bolshevic revolution, has a hollow ring. All that is 'saved' is–as Hilbert put it–a formal game, $\mathfrak{F}$, with symbols (where $\mathfrak{F}$ is one of the formalizations of analysis developed in the first quarter of the century). Except for elementary propositions, by no means the whole content of (the ordinary interpretation of) ordinary analysis, the latter is not 'saved' by the consistency of $\mathfrak{F}$. Actually Brouwer's attack, by way of 'contradictions' with ordinary mathematics, was hardly disturbing since he got them by changing the meaning of the logical operations and the domain of variables, for example, replacing sequences in the sense of ordinary mathematics by choice sequences (naturally, supplemented by grand foundational doubts about our ordinary notions, doubts which, by footnote 7, are generally more dubious than the notions themselves).

Ironically, if, after recognizing the inadequacy of the consistency *criterion*, one actually looks at consistency *proofs* one finds that, properly formulated, they do 'save' a remarkable amount of ordinary mathematics (for use in constructive mathematics). In fact, progress over the last 25 years allows a precise formulation of the issue whether (i) the methods developed in work on

[8] Up-to-date and quite readable accounts of Hilbert's (consistency) program are to be found in the articles on Hilbert's second and tenth problems in Proc. Sympos. Pure Math., Vol. **28** (to appear). Conditions on systems $\mathfrak{F}$ to which Gödel's incompleteness theorem applies are stated, in particular, that numerical computations can be mimicked in $\mathfrak{F}$; so if a diophantine equation $p(x_1, \ldots, x_9) = 0$ has a solution $(n_1, \ldots, n_9)$, $p(n_1, \ldots, n_9) = 0$ can be verified by computation, a fact used in the next paragraph but one. It is to be stressed that the *general* conclusions are independent of any precise analysis of the notion of finitist proof. Besides, there is no evidence for any particular reliability of finitist methods (Hilbert's principal claim for them)–tacitly, at the present time; of course, 100 years ago mathematicians had to treat nonfinitist, logically compound expressions quite gingerly, such as the negation of uniform convergence! On the contrary, inasmuch as nonfinitist proofs are often simpler than finitist ones (of the same theorem), and the nonfinitist *principles* equally reliable, the actual probability of error in a finitist proof is likely to be higher. This is borne out by the literature on finitist consistency proofs which contains remarkably many oversights. No other compelling virtue of finitist methods has turned up either (except the fact that they were one of the first that occurred to us).

Hilbert's consistency program or (ii) those developed from Brouwer's ideas on choice sequences are more effective for this purpose.[9]

Writings on the philosophy of life and politics. The editor has included excerpts from *Leven, Kunst en Mystiek* (1905, translated as: *Life, art and mysticism*), and given reasons for omitting others, for example, on p. 565: 'In many places Brouwer runs on inconsiderately, for instance, on the position of women in society.' Some of the omissions are translated in the dissertation of Dr. van Stigt, for example, one expressing Brouwer's view that every woman is more like a lioness than a twin is like his brother. Curiously, neither Brouwer nor Heyting points out the relevance of some of the mystical business to the 'main stream' of foundations, which *assumes* that we must be capable of making the grounds for our knowledge conscious to ourselves, and that this would be rewarding to boot. In sober terms, the 'mystical' alternative would be that we have a lot of knowledge, also in mathematics, which is simply more convincing than any proposed analysis–of our actual grounds for this knowledge, let alone of possible grounds.

On pp. 465–471, there is a kind of political manifesto, published (1946) soon after the war, apparently designed to improve the world by distinguishing between 5 levels of language (and their logical connections, bottom of p. 466). It concludes, on pp. 470–471, with an unorthodox combination of concrete proposals: (i) the protection of private property by the state, (ii) far reaching socialization of means of production and heavy taxes, and, above all, (iii) the free circulation of gold. It is not said whether Brouwer himself helped carry out any of these proposals, for example (iii).–At about the same period the reviewer knew some high-minded mathematicians who wanted to destroy the capitalist system by means of (iii): they bought gold in England and took it to France where the price was usually $>$ 30% higher (and converted back to sterling in England at the controlled rate of exchange).

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[9] The standard scheme for *extending* Hilbert's consistency program goes by the name of (functional) interpretation, generally of the following form: To each formula A of $\mathfrak{F}$ is associated a sequence A_1, A_2, ... such that (i) if A is derivable in $\mathfrak{F}$, some A_i is derivable constructively, (ii) judged by our ordinary interpretation, the content of A is 'saved', that is, formally A is (classically) derivable from each A_i (but not generally conversely). The interpretations are called 'functional' because even if A is number theoretic, the A_i will generally contain variables for functions (or choice sequences). One of the simplest examples is the so-called no-counterexample–interpretation, illustrated by A of the form $\exists n \forall m A_0(n,m)$. (We may have proved A without knowing an 'explicit' n; take for $A_0(n,m)$: $P(n) \vee \neg P(m)$ and for P, say: $2n$ is not the sum of 2 primes). A is replaced by $\forall f \exists n A_0[n, f(n)]$ where f ranges over number theoretic functions. Then, given $\mathfrak{F}$, we find a class of (continuous) functionals F_1, F_2, ... and take for A_i: $A_0[\bar{F}_i, f(\bar{F}_i)]$ where $\bar{F}_i = F_i(f)$. More interestingly, consider Brouwer's fixed point theorem (FPT); suppose φ is a topological mapping, φ: $S^2 \mapsto S^2$. Let ξ range over S^2 and $\Phi(\xi, n)$ be a neighborhood or 'approximating' function of φ, that is $|\Phi(\xi,n)| < n^{-1}$ and $\varphi(\xi) \in \Phi(\xi,n)$. Then (FPT) asserts: $\exists \xi \forall n[\xi \in \Phi(\xi,n)]$, the expression inside [] being elementary for point *generators* ξ (in the sense of (ii) in footnote 1). The no-counter example-interpretation ensures a constructive proof of $\forall N \exists \xi[\xi \in \Phi(\xi, \bar{N})]$ for functionals N: $S^2 \mapsto \omega$, where $\bar{N} = N(\xi)$. The interpretation applies since (FPT) can obviously be proved in the kind of weak system mentioned in footnote 5. (For continuous N, this reduces to the triviality $\forall n \exists \xi[\xi \in \Phi(\xi, n)]$ unless generators ξ are used.)–The open issue mentioned in the text arises for relatively many A, though not for (FPT) itself, as follows: A is formally derivable both classically and for choice sequences, but not for narrower classes of constructive functions, for example, recursive ones. By the elimination result at the end of footnote 3, we have A_e (valid for such narrower classes), and can compare A_e with the A_i.