Institute for Language, Logic and Information

REMARKS ON INTUITIONISM AND THE PHILOSOPHY OF MATHEMATICS

revised version

A.S. Troelstra

ITLI Prepublication Series
X-90-01

University of Amsterdam

Instituut voor Taal, Logica en Informatie
Institute for Language, Logic and Information

Faculteit der Wiskunde en Informatica
(Department of Mathematics and Computer Science)
Plantage Muidergracht 24
1018TV Amsterdam

Faculteit der Wijsbegeerte
(Department of Philosophy)
Nieuwe Doelenstraat 15
1012CP Amsterdam

REMARKS ON INTUITIONISM AND THE PHILOSOPHY OF MATHEMATICS

revised version

A.S. Troelstra
Department of Mathematics and Computer Science
University of Amsterdam

To be published in the Proceedings of the
Congresso Internazionale
"Nuovi problemi della logica e della filosofia della scienza"
Viareggio, 8-13 gennario 1990

Received January 1990

REMARKS ON INTUITIONISM AND THE PHILOSOPHY OF MATHEMATICS

by

A.S. Troelstra[1]

dedicated to the memory of Osvald Demuth

1. Introduction.

Just before 1930, the overall picture of the foundations of mathematics was rather simple and straightforward. Logicism had been tried, and found wanting, not in the last place because it had turned out to be difficult to decide what deserved to be called logical (= the purely analytical "tautological" truth). Russell's solution of making everything which was not obviously logical into an "hypothesis" was certainly not satisfactory to everyone.

Then there was formalism, offering a safe retreat into straightforward combinatorial games, avoiding difficult questions of ontology, coupled with an attractive programme for having one's cake and eating it too: justification of the use of abstract methods by indirect means.

Or one could follow Brouwer and become an intuitionist, insisting on the content of mathematics, willing to amputate parts of mathematics which could not be interpreted as mental constructions, and to sacrifice dubious logical principles.

Maybe the reader thinks that this thumbnail sketch is a bit of a caricature and leaves out many subtleties. Then he / she is right, but nevertheless I think this sketch is grosso modo correct as a description of the atmosphere.

Since 1930, a lot has changed. Intuitionism never became very popular, except as a subject for metamathematical research. Certain basic notions of Brouwerian intuitionism were widely regarded as puzzling or mysterious, such as choice sequences. To confuse the picture on the constructivist side, in later years new constructivist trends, with a philosophy different from Brouwer's circle of ideas, have joined the melee: constructive recursive mathematics in the spirit of A.A. Markov, and the rather pragmatical constructivism of E. Bishop.

Due to the failure of Hilbert's programme in its original form, formalism has lost much of its attraction as well. For the "working mathematician" it is still a convenient shelter, if he wants to dodge difficult questions about the existence of mathematical objects, but rather far removed from the way he actually works; his abstract objects are all very real to him.

Platonism has been there all along, but was perhaps not very respectable around 1930; since then it has become more so, after intuitionism and formalism had revealed their weaknesses and after Gödel had defended it in connection with axioms for set theory.

Logicism and formalism had a goal in common, namely, to put mathematics on a firm and certain basis. Intuitionism primarily consists in a different view of the nature of mathematics; certainty, as far as humanly possible, would be the byproduct of following the intuitionistic line.

The first two schools have failed, or at least not succeeded in their principal goal in a way which carried conviction. In the case of intuitionism, it was discovered that "following the intuitionist line" could be interpreted in various not necessarily compatible ways, so that in practice also intuitionism could not guarantee "certainty".

After finishing, jointly with D. van Dalen, a lengthy introduction to constructivism in mathematics (Troelstra and van Dalen 1988), I felt the desire to take stock of the situation, and asked myself what had become of Brouwer's intuitionism, or to put it more subjectively, what I thought about intuitionism in the light of the experience of the last few decades. This paper is the result.

It is often far from easy to say what constitutes progress in the philosophy of mathematics. Thus for example, most of what has been said concerning our theme in several papers by Bernays (1935, 1946, 1950) is in my opinion still valid, and I could leave it at that, and let my paper end here. Nevertheless, something can be added. Since 1950 there has been extensive research, most of it of a technical nature, in intuitionistic mathematics and metamathematics. And even if, in general terms, one has not much to add to views formulated at least 40 years ago, one may still ask in what way the experience gained in the last 40 years supports or modifies these views. To mention a few developments which are, I think, of interest in this connection:

(a) the emergence of "rival" constructivist enterprises, already mentioned above;

(b) the axiomatization of Brouwer's theory of choice sequences and the continuum;

(c) the discovery of the link between type theory and intuitionistic logic ("formulas-as-types") and the development of type theories in the style of Martin-Löf;

(d) the discovery of an "internal logic" associated with certain types of categories; this logic is in general not classical, but intuitionistic;

(e) the discovery that certain proof-theoretically weak systems nevertheless are capable of expressing very substantial parts of mathematics, and the investigations in e.g. bounded arithmetic (Nelson 1986, Buss 1986).

For example, the experience gained with Bishop's and Markov's constructive mathematics corrects the impression that Brouwer's theory of choice sequences is the only viable route to a mathematically satisfactory constructive theory of the continuum (for the historical context, cf. Troelstra 1982). From Bishop's book (1967, rewritten as Bishop and Bridges 1988) one sees that continuity principles and bar induction (or more properly its most important corollary, the fan theorem) do not play a vital rôle in the development of constructive mathematics. Where the traditional intuitionist would appeal to the continuity principles or the fan theorem, Bishop makes suitable assumptions of continuity or uniform continuity of functions in the statement of his theorems.

(d) above is of interest since it leads us to the consideration of structures which in certain respects differ considerably from the "intended model" of traditional constructive mathematics, e.g. by failure of the axiom of countable choice. Since many of these structures can themselves be studied in a constructive setting, thus becoming something like an "inner model" of the intended model, we see that there is a good deal of freedom in the interpretation of logical operators, in particular quantification.

2. Absolute and relative certainty.

First of all, I think that the aim of “absolute certainty” for mathematics is a mistake (or illusory). I do not see any road to absolute certainty (if it exists) - unless perhaps we are prepared to cripple mathematics by reducing it to an insignificant fragment. Cf. the careful discussion of the rôle of evidence in Bernays (1946, 1950). Evident knowledge is usually regarded as certain knowledge; Bernays' discussion clearly shows that here "certain" should not be understood in an absolute sense. In his 1950 paper Bernays notes that opinions differ as to what is to be counted as absolutely evident, and he remarks that this difficulty disappears if we give up the idea of absolute, a priori evidence, and instead assume that cooperation between rational elements and evidence applies not only to the natural sciences, but also to mathematics; that is to say, the set of ideas (conceptual framework) is not fixed in advance, but is developed in mental (German: "geistige") experimentation. Such experimentation indicates a failure from the traditional point of view, appears to be well-adapted to the subject matter from the viewpoint of mental experience ("geistige Erfahrung"). We return to the idea of "experimentation" in section 4 below.

The attitude towards certainty described above seems to be commonplace nowadays (Hersh[2] (1970) is an example of an author explicitly rejecting the quest for absolute certainty), and is really not in conflict with the intuitionistic tradition. To quote Heyting (1958):

“It can be asked whether in intuitionistic mathematics absolute certainty and absolute rigour are realized. The obvious answer seems to be that absolute certainty for human thought is impossible and even makes no sense.”

Even Brouwer, I think, also would have denied that intuitionism gave “absolute certainty”.

Now that I have expressed my disbelief in absolute certainty, I hasten to say that I do believe in “relative certainty”, that is to say that questions of relative certainty are meaningful and ought to be a legitimate subject of discussion in the philosophy and foundations of mathematics. Cf. also the final section of Maddy 1988: "No one would expect even the best scientific arguments [in the natural sciences] to be absolutely justifying. Our epistemological inquiries in mathematics will be hampered if we set an unreasonably high standard. ... The success of set theory - its objectivity and applicability - confirm the enterprise and its justificatory practices as a whole, but within that whole, the particular methods can be analyzed, supported or criticized individually."

But I must add that "certainty" is a rather misleading term. "Certain" is actually a mixture of notions. There are in fact several, not necessarily compatible, forms of certainty, such as "intuitively evident", "surveyable", "tested by mathematical experience".

3. Intuitionism as a theory about mathematics, and the rôle of language.

In traditional intuitionism, "intuitionistic mathematics is a mental construction, essentially independent of language" (Brouwer 1947). Of course Brouwer is well aware of the fact that for flesh-and-blood mathematicians language is necessary, since our memory is imperfect. A detailed discussion of Brouwer's "dogma of languageless mathematics" has been given by van Dalen (1990). The Brouwerian principle is, I think, above all directed against formalism: relying on the "formal regularities of language" which, for Brouwer, is characteristic of Cantorism and Hilbertian formalism alike, cannot guarantee mathematical truth. Brouwer also distrusts language as a means of communicating truth in general, though I agree with van Dalen in believing that his views often have been interpreted (though not by Heyting) in a much more extreme way than intended by Brouwer.

Traditional intuitionism is a <u>theory</u> (i.e. a schematic description) of human mathematical activity, and as a theoretical construct it introduces some idealizing assumptions which are, quite obviously, not fulfilled in actual practice, such as: mathematical constructions are "in principle" carried out without the use of language, the ideal mathematician has perfect recall and unlimited memory, and the results of introspection (in Brouwer's sense) are unambiguous and sharply defined.

One has only to inspect the actual construction of intuitionistic mathematics and its basic concepts (such as natural number, choice sequence) to discover that the languagelessness does not enter into the justification of principles and existence assumptions for mathematical objects - not even in theory of the self-reflecting idealized mathematician. At the same time, this does not mean that we can consistently assume that any object considered in intuitionistic mathematics can always be adequately described by means of language - this is false for an individual lawless sequence, for example.

In the light of the preceding, I prefer not to make "languagelessness" into a dogma (= basic principle) of intuitionistic mathematics, nor do we want to embrace the opposite principle: every construction must be capable of a linguistic description. The crucial difficulty is this. We cannot, in practice, erect our intuitionistic theories without the help of language - this might cause a divergence between the sort of postulated ideal languageless mathematics and our actual construction of intuitionistic mathematics, but we have no means to test for divergence!

Bernays (1970) has described mathematics as the science of idealized structures; in the construction of theories of external reality, the correspondence between theory and reality is a schematic one. On the one hand, the theoretical description does not catch every aspect of reality, but on the other hand the perfection of the schemata is only approximated by reality. This makes the "schemata" a special category in itself: the domain of mathematics. Intuitionism seems to bear a similar relation to our actual intuitions and cognitive powers - it is a schematization of these intuitions and powers. In other words mathematical experience plays here a rôle comparable to the rôle of physical reality in the construction of a physical theory[3].

4. Formalization and the evidence for axioms.

In modern mathematics we have learnt to axiomatize; if the axiomatizing includes the logical reasoning, we call it formalizing. The result of this is that the justification of a given piece of mathematics (whether classical, intuitionistic or otherwise) is neatly split into two components: the verification of the correctness of the deductions, given the axioms and rules of deduction; and the business of "believing the axioms".

The act of formalization brings the theory within the domain of actualistic (that is, concretely verifiable, not only verifiable in principle) combinatorial truth, and thereby within the intersubjective domain, where we are certain to agree with each other in judging correctness. Formalization is thus a tool for separating the problematic from the unproblematic. But note that in our reliance on formalization the legitimacy of iteration of deduction rules is implicit.

The problematic, that is the justification of the axioms. A mathematician is usually not interested in axioms if he feels that there is no interpretation (model) for them, that is if he does not have at least an intuition concerning a structure fulfilling the axioms.

In intuitionistic mathematics we find many examples of justification arguments for axioms in the theory of choice sequences (see e.g. chapter 12 of Troelstra and van Dalen 1988), and Maddy (1988) reviews the arguments for and against a number of axioms of classical set theory. Maddy divides the arguments into two categories, "intrinsic" (motivated by the concept of set) and "extrinsic" (pragmatic, e.g. leading to a nice theory, or strong explanatory power). Here I shall consider only intrinsic motivations.

Inspection of the examples from classical set theory and the theory of choice sequences shows us that the motivations for the axioms range from speculations (often obtained by bold extrapolations) to detailed analysis of concepts. For example, for the so-called lawless sequences the evidence for accepting the continuity axiom can be presented in a much more compelling way than the reasons for accepting bar induction.

In the case of classical axiomatic set theory, many authors try to base their justification on the cumulative hierarchy idea. The arguments used vary from plausibility arguments, and analyses of the concept of set, to more speculative extrapolations of ideas, guided by certain rules of thumb (in Maddy's paper characterized by slogans such as “one step back from disaster”, “inexhaustibility”, “uniformity”).

There is something unsatisfactory about a mathematical theory with a highly speculative basis. Of course, there is always the good old (logicist) device of regarding such a theory as an extensive piece of reasoning based on hypotheses, without having a satisfactory (conceptual) model for the hypotheses, but in many cases this attitude does not seem to do justice to the insights and intuitions behind such a theory. It should be noted, however, that the mathematical insights of speculative theories may afterwards find an application in a less speculative, more “concrete” setting. For example, combinatorial properties of large cardinals may suggest or motivate systems of recursive ordinal notations. From this it will become clear that the unsatisfactory status of highly speculative theories cannot be used as a sufficient argument against their development. I see the exploration of the consequences of such axioms as “mathematical experimentation” (that is, the exploration of imprecise notions and the consequences of doubtful, possibly incoherent assumptions). Mathematicians have been continually experimenting (= gaining mathematical experience) in mathematics throughout its history, though I think we are deceiving ourselves if we invoke strong platonism to make our experimenting look more “solid” (Bernays (1935) introduced the distinction between limited platonism, which accepts the totality of certain infinite collections, in particular **N**, which leads to the acceptance of the principle of the excluded middle for arithmetical statements, and strong platonism, which postulates an objective reality corresponding to all mathematical and logical notions.) Unmitigated strong platonism leads directly to the Russell paradox and so has to be modified in one way or another; we shall not discuss this here. From now on, we shall refer to restricted platonism only. One may picture limited platonism as obtained by extrapolating human cognitive powers: one accepts that the infinite totality of **N** can be surveyed.

In traditional intuitionism one does not freely experiment in exactly the same style as in axiomatic set theory. But certainly nothing prevents us from exploring assumptions, within an intuitionistic framework, about rather imperfectly understood / formulated informal notions.

Next I want to discuss the extraction of axioms by concept analysis; this may be regarded as a sort of "real-world correlate" of Brouwer's theoretical notion of introspection.

Concept-analysis can be carried out with "informal rigour" . By concept analysis we mean the isolation of mathematically relevant aspects of informally given concepts; applying informal rigour means that we carry the analysis as far as possible with the means at our disposal, in other words we do not consciously neglect mathematically relevant aspects. It would be misleading to think of concept analysis being applied only to "first impressions", i.e. notions which occur to us right from the beginning. In practice, the informal notion to be analysed may have arisen in the course of a long and complicated mathematical development, already presupposing a good deal of mathematical sophistication.

Clearly, there is no absolute standard of informal rigour; various degrees of informal rigour are expressed by phrases such as "it is plausible that ...", "this seems to suggest that ...", "we are led inescapably to the conclusion that ..." (inescapability is seldom inescapable, however). Of course, a judgement on the degree of rigour attained contains a subjective element, but on the other hand, if a renewed analysis of (a mixture of) notions introduces new mathematically relevant distinctions, then the new analysis is more rigorous than the old one. Informal rigour is time-dependent; what is regarded as informally rigorous ("intuitively evident") may change in the light of increasing mathematical experience, as is illustrated, e.g., by the history of the theory of choice sequences (cf. Troelstra 1983).

In this connection it is interesting to note that Heyting in (1949) explicitly commented on the various degrees of evidence among the basic concepts of intuitionistic mathematics. As examples of decreasing evidence he mentions: arithmetic of small natural numbers; operating with large natural numbers; the concept of order type ω; negation; the introduction of choice sequences; reflection on the form of mathematical proofs as used in Brouwer's proof of the fan theorem ("it is as if we descend a staircase, leading from the daylight into a dark hole ...").

Typically, in applying informal rigour in our concept analysis, we find from time to time that we have to take "intuitive jumps". By this I mean that we arrive at a step in our justification of a principle where we see no possibility of refining our analysis (with our present means) and the jump to the next step (conclusion) in our analysis is a matter of "take it or leave it" (examples follow). More pragmatically, one may also ask whether we should stop only in our analysis when we do not see how to refine it any further. Usually we do have some idea in advance as to which aspects of the notion we want to analyze are mathematically relevant, and we feel satisfied if we have found a precise mathematical formulation which takes care of these aspects, and nothing more.

The activity of formulating / discovering axioms or mathematical principles is something different from giving rigorous mathematical proofs; the latter activity is in principle formalizable, the first one not. But I regard concept analysis as much a part of mathematics as the construction of rigorous mathematical proofs.

5. Examples of concept analysis.

To make the preceding discussion more concrete, we briefly review four examples of informal rigour and concept analysis.

(1). The intuitive concept of area below a curve (in a cartesian coordinate system) can be mathematically completely characterized for a wide class of curves, by observing a few properties only of the intuitive notion, such as monotonicity, finite additivity and agreement with the usual area definition for polygons.

(2) A lawless sequence is a process of choosing natural numbers as values, such that the process is never finished, and at any moment of the process we know only a finite initial segment of the sequence, and at no moment restrictions on the future choices are imposed. All finite sequences of natural numbers occur as initial segment of a lawless sequence (detailed discussion in Troelstra and van Dalen 1988, section 12.2).

For such sequences we have the extension principle: if F is a continuous operation assigning to each lawless sequence a natural number, then F may be extended to an extensional operation F' defined on all sequences of natural numbers. For let α be an arbitrary sequence, then we compute $F'(\alpha)$ as follows. We generate successively $\alpha 0$, $\alpha 1$, $\alpha 2$, ... and at each stage we ask ourselves whether we can compute F from $\langle \alpha 0, \alpha 1, \dots, \alpha n \rangle$, when we think of this initial segment as belonging to a lawless sequence (that is, we systematically "forget" whatever further information we may possess about α, and try to apply F to a lawless sequence beginning with $\langle \alpha 0, \alpha 1, \dots, \alpha n \rangle$). There is an intuitive jump involved in the assumption that the method for computing F must also work for the "pseudo-lawless" sequence obtained by deliberately forgetting all extra information concerning α except initial segments. We accept this jump, because we do not see how F can escape yielding a result for this "pseudo-lawless" sequence. The argument obviously does not work if we take the domain of total recursive functions instead of the lawless sequences, since we cannot think of the initial segments of an arbitrary α as all belonging to one and the same recursive function. For another, more subtle, informal rigour argument see Troelstra (1983, footnote 10).

The discovery that one can distinguish many different notions of choice sequence, with distinct mathematical properties, provides an excellent illustration of the "intensional" (perhaps better "epistemic") element in intuitionistic mathematics: the assertions one can make about a particular kind of choice sequence are determined by the kind of data concerning such sequences available to the ideal mathematician. At the same time, the difficulty in making principles for choice sequences completely evident (without a residue of "intuitive jumps") makes it clear that in proposing our theories intuitive evidence is inextricably linked with a formal element.

(3) Turing's analysis (1937) of a the notion of computable function is another example of concept analysis, carried through with informal rigour. The analysis carries conviction, a conviction which is in part based on a review of all kinds of possible extensions of the possibilities of Turing machines, and showing that all these possibilities can ultimately be mimicked by the action of a Turing machine. In this type of analysis there always remains a loophole inasmuch we can never be certain that we have really reviewed all possibilities; later authors (Friedman 1971, Gandy 1980) have sharpened Turing's analysis in different ways.

(4) Finally we wish to mention here von Mises' axioms for his notion of Kollektiv (random sequence), as an example of an incomplete concept analysis with considerable appeal (see on this topic van Lambalgen 1990).

Is the notion of "constructive" as used in intuitionism or Bishop's constructive mathematics also something which can be analyzed and described mathematically in the same spirit as the preceding examples? I think this example is a bit different. Looking at the practice of constructive and intuitionistic mathematics, we see that the notions of construction and constructive are mainly delimited from below, by stipulating successively what we shall accept as constructive / intuitionistic, guided by a very rough idea of constructivity. Thus, in Brouwer's intuitionism we accept natural numbers, lawlike sequences, choice sequences, properties of numbers and choice sequences (at least if defined predicatively), etc. We do not seem to have a very accurate a priori criterion for constructivity in the intuitionistic sense. The situation for the concept of set in the sense of the cumulative hierarchy seems to be similar.

6. Actualism and intuitionism; meaning and understanding.

The observation that there is a genuine difference between our understanding of say 5, and $10\uparrow(10\uparrow10)$, where $n\uparrow m$ is n^m, is quite old (going back to Mannoury 1909, pp.56-58, at least). Bernays (1935) was the first to point out that intuitionism in this respect transcends concrete immediate evidence: "intuitionism completely disregards the possibility that the arithmetical operations required for the application of recursive definitions do not have a concrete meaning for very large numbers."

There have been repeated attempts to take such observations seriously and to develop a mathematics based on our actual cognitive powers. I shall use the term "actualism" as a general label for such theories. In actualistic theories, in particular, exponentiation is not provably everywhere defined. That is to say, if we think of the natural numbers as representable by a sequence of units (concretely represented by strokes, for example) we have no direct evidence for regarding $10\uparrow(10\uparrow10)$ as a natural number; alternatively, we may accept $10\uparrow(10\uparrow10)$ as a natural number; but then we cannot insist that it is representable as a sequence of strokes, and we must face the possibility that between 0, 1, 2, 3, ... and $10\uparrow(10\uparrow10)$ there are gaps as a result of the fact that classically and intuitionistically existing numbers in the interval $[0,10\uparrow(10\uparrow10)]$ are not in the actualist sense representable.

Actualism is also known as ultra-finitism, ultra-intuitionism,or strong finitism. The supporters of actualism by no means present a uniform picture (e.g. Esenin-Vol'pin 1961, 1970, Parikh 1971, C.Wright 1982, R.O. Gandy 1982, E. Nelson 1986), in fact, the diversity of the various approaches is striking. From an actualist point of view intuitionism contains a strong idealization, coming to light in the fact that numbers such as 5, and $10\uparrow(10\uparrow10)$ are treated as objects of the same kind.

The papers quoted show that it is by no means easy to develop a coherent actualist philosophy of mathematics; by some, doubts has been raised whether this is at all possible.Nevertheless, as remarked in the introduction, interesting and non-trivial mathematical theories, such as bounded arithmetic, have been developed which take into account at least one important aspect of the actualist criticism: in these theories **N** is closed under addition and multiplication, but not under exponentiation, that is to say exponentiation is not a provably total operation.

Here is the place to make some remarks concerning Dummett's argument in favour of intuitionistic logic, and against classical logic. Dummett has advanced, in a number of publications (1975,1976,1977, sections 7.1-2), an argument for intuitionistic and against classical logic (cf. Troelstra and van Dalen 1988, section 16.4 and the references given there), which has been criticized from an actualist point of view (Wright 1982, and in particular George 1988).

In a nutshell, Dummett's argument in favor of intuitionistic logic over classical logic amounts to this. The meaning of expressions must reveal itself in the use of expressions, in other words, the meaning of a sentence is determined by the conditions for correctly asserting it, i.e. the proof conditions for the sentence. The platonist's understanding of $\forall n \in \mathbf{N}\, A(n)$ cannot be described by such proof conditions, and the defense of the platonistic understanding of the truth of $\forall n \in \mathbf{N}\, A(n)$ as obtained by an extrapolation of human powers of cognition (inspecting infinitely many cases, instead of finitely many) is rejected on the grounds that it are our human powers of cognition which count. It is to be noted that Dummett for example accepts $A(10\uparrow(10\uparrow10)) \vee \neg A(10\uparrow(10\uparrow10))$ for primitive recursive A because the decision can be effected "in principle".

But the actualist in his turn may criticize $A(10\uparrow(10\uparrow10)) \vee \neg A(10\uparrow(10\uparrow10))$, if it means that the decision takes "unfeasibly many" steps. There seems no non-circular way of explaining to an actualist the intuitionistic concept of natural number, and the meaning of "in principle", in a non-circular way. (George's "first thesis" (1988) reads: "all characterizations of the intuitionistic understanding of the natural numbers are elucidatory, exhibiting sooner or later some kind of circularity"). Thus it appears that actualism stands to intuitionism more or less as intuitionism stands to platonism, and it follows that Dummett's argument for preferring intuitionistic over classical logic is not conclusive. One does not need to be a full-fledged actualist to acknowledge that there is a difficulty here. Even if we have only a very partial idea as what actualist mathematics ought to be, the fact that a theory such as bounded arithmetic at least incorporates an essential element of actualism, makes it necessary to take this objection seriously. Should one conclude from this that actualism ought to replace intuitionism, or are perhaps some of the assumptions on which Dummett's argument rest in doubt? I incline to the latter possibility.

Dummett's view of a satisfactory theory of meaning is molecular, not holistic. "Molecular" means that the meaning of logically compound statements is given explicitly in terms of the meaning of the component parts. In a holistic theory, nothing less than the total use of language determines its meaning. But is meaning in mathematics really molecular?

George (1988) casts doubts on Dummett's requirements for a theory of meaning, more specifically he doubts whether meaning is only conveyed by use (George's second thesis states that "intuitionistic mathematical practice cannot itself fully display the understanding underlying it, and appeal has to be made to special faculties of induction"). A possible hypothesis would be that an innate mental structure predisposes us for the understanding of certain patterns, while excluding others.

In Dummett (1977), the construction of a theory of meaning satisfying Dummett's requirements, is described as a programme. Perhaps the work of Martin-Löf's (see e.g. his 1984) may be regarded as making Dummett's idea of a "molecular" theory of meaning[4] concrete.

Apart from the arguments given by George, there are other reasons to feel some doubts about the possibility of a molecular theory of meaning for let us say intuitionist mathematics. In mathematics there is an everyday, practical sense of "grasping the meaning of a statement or definition" for which I shall use here simply the word "understanding". For example, when I present an informal description of a notion of choice sequence, on the basis of which I, or someone else, can see that $\forall\alpha\exists x$-continuity holds; then I, or the person listening to my description of the notion, has understood the meaning of "$\forall\alpha\exists x$". It is certainly not obvious that this informal understanding is arrived at in a "molecular" way, in the sense that our reasons for asserting an instance A of $\forall\alpha\exists x$-continuity are explainable in terms of the component parts of A. In short, there is a considerable gap between "understanding" and grasping a meaning in the sense of Dummett's theory of meaning; and one ought not to be satisfied with a theory of meaning if one cannot give a satisfactory account of "understanding" in relation to such a theory as well. This discrepancy between unsderstanding and a (more-or-less hypothetical) theory of meaning is also the reason for the doubts raised in Troelstra and van Dalen (1988, section 16.4), as to whether in mathematical practice a molecular theory of meaning is really adequate; perhaps meaning in mathematics is neither all-out holistic nor strictly molecular[5].

To summarize the preceding discussion, the actualist criticism makes us aware of the fact that not only platonism, but intuitionism too involves a strong idealization of human cognition - and it remains to be seen which step is the more drastic one: from actualism to intuitionism, or from intuitionism to platonism. The actualist criticism also represents an obstacle to Dummett's argument for preferring intuitionist logic over classical logic. From this perspective, intuitionism and platonism have more in common than is commonly thought; they represent only two possible sets of idealizing assumptions[6] relative to our actual cognitive powers. In view of this, the differences between intuitionism and (restricted) platonism[7] lose their pungency; there is no absolute contrast between mathematics justified by its content (intuitionism) and speculations based on a platonistic idea of truth (classical mathematics as Brouwer saw it).

8. Certainty.

The comparison between platonism, intuitionism, and actualism reveals something else as well. It is a fact that we have learned to use the idealizations of "uniform **N**" and "restricted platonism" (in particular the simultaneous surveyability of **N**) with remarkable ease and certainty. On the other hand, the demands of actualism on mathematical practice are as yet only very imperfectly understood.

Keeping closer to our human powers of cognition is in itself no guarantee for a more easily understood and "certain" theory. Perhaps it is true that less idealization means more certainty of one kind, but there seems to be a loss of certainty of another kind, due to an increase in complexity. An extremely complex proof using only actualistic principles does not necessarily inspire more confidence than a simpler proof using intuitionistic or classical methods.

So what is the right degree of idealization? actualism, intuitionism or platonism, or yet another "ism"? I think this should depend on the mathematical phenomenon we want to study. If we are interested in choice sequences, intuitionism provides the appropriate setting. If we want to investigate the difference in character of exponentiation and multiplication, it might be appropriate to use a theory that incorporates some elements of actualism. (My attitude here is pragmatic, but should not be confused with conventionalism.)

In this connection there is another relatively recent discovery which I think is highly interesting from an epistemological point of view. It is the following. For a long time it was believed, and perhaps is still believed by many, that we needed proof-theoretically strong principles for modern mathematics (such as the powerset axiom in **ZF**, and impredicative comprehension in higher-order logic). But already Weyl (1918) showed that much of 19th century analysis could be carried out in a quite weak ("arithmetical") system. This line of research has been continued and extended by a.o. Feferman (e.g. 1964; see also the section "Theories for mathematical practice" in Feferman 1987), Kreisel (1960, 1962), Lorenzen (1955, 1965), Takeuti (1978). In recent work under the (rather inappropriate and misleading) label of "reverse mathematics", the minimum strength needed to prove certain important theorems from various areas of mathematics has been investigated (a survey[8] is in Simpson 1988). Thus we very often need far less than what is suggested by the standard proofs. Already primitive recursive arithmetic (Mints 1976, Sieg 1985) is quite powerful. Expressive power of the language is often more relevant than proof-theoretic strength. Of particular interest is the observation that many theorems usually proved impredicatively can in fact be proved in a predicative theory (without an excessive increase in complexity of the proofs, cf. Simpson 1988). Since I do not understand impredicative comprehension all that well, I am pleased with these steps towards "more certainty".

In consequence of the preceding discussion I see the following tasks, among others, for the philosophy and foundations of mathematics:

(a) to assess the present position of mathematical principles on a scale ranging from "speculative" to "justified by concept analysis";

(b) to gain further insight into the acquisition of mathematical experience by historical studies. Relevant material is scattered throughout the literature (see e.g. Hallett 1984), but it would be worthwhile to undertake further historical studies with this specific aim in mind;

(c) to investigate and evaluate proof-theoretic reductions and programmes such as "reverse mathematics" in connection with (a).

9. Equality in constructive mathematics.

I want to finish with the discussion of a slightly more special question, which however, plays an important rôle in discussions of intuitionistic mathematics, namely: what is the nature of equality in constructive mathematics? (Equality in intuitionistic mathematics is discussed in Troelstra and van Dalen 1988, section 16.2.)

My starting point is that in introducing a domain (a collection over which we can quantify; I want to avoid "set") in constructive mathematics, we must at the same time introduce a notion of equality on the domain; understanding a domain means at the same time understanding equality between elements of the domain. This certainly permits us to think of a domain as given as a collection with an equivalence relation on it, provided we do not think of these two components as necessarily specified independently. Thus in constructive mathematics equality is not a general a priori ("logical") notion, but rather a mathematical one.

"Intensional equality" in constructive mathematics is not a mysterious new primitive[9], but arises as follows. Many domains $\mathbf{D} := (D,=)$ are introduced in mathematics by taking quotients ($\mathbf{D} := \mathbf{D}'/\sim$) of some domain $\mathbf{D}' := (D',=')$ (e.g. $\mathbf{R}$ is obtained as the set of equivalence classes of fundamental sequences of rationals).

In the constructive setting, it often matters how objects are given to us; intuitionistic continuity axioms for choice sequences are motivated by the way the axioms are given to us. If we consider the constructive reading of $\forall x{:}\mathbf{D}\exists y$, the y is given by an operation acting on the data needed to determine an element of $\mathbf{D}$; this operation does not necessarily respect =, but perhaps only =$'$.

Sometimes it is convenient to "abuse language" and to treat =$'$ as "intensional equality" on $\mathbf{D}$. For example, in discussing functions in $\mathbf{N}\to\mathbf{N}$ in constructive recursive mathematics, we may use $f=g$ to indicate that f and g are given to us by the same gödelnumber. So it is primarily a matter of linguistic and technical convenience, whether we want to handle "intensional equality" via the presentation axiom (see below), or whether we use = between elements of $\mathbf{D}$ for =$'$ of the underlying $\mathbf{D}'$.

In this connection there is one practical point which should be kept in mind. If we insist on the BHK-interpretation, it is natural to postulate for our domains of quantification an axiom of choice

$$\forall x{:}\mathbf{D}\exists y\, A(x,y) \to \exists f \forall x{:}\mathbf{D}\, A(x,f(x));$$

and if we may assume that f respects =, then f is a function of type $\mathbf{D}\to\mathbf{D}''$ and we may write

$$\forall x{:}\mathbf{D}\exists y{:}\mathbf{D}''A(x,y) \to \exists f{:}\mathbf{D}\to\mathbf{D}''\forall x{:}\mathbf{D}\, A(x,f(x));$$

but if $\mathbf{D}$ has been introduced as a quotient of $\mathbf{D}'$, then it may be that f is a function on $\mathbf{D}'$ only. That is, if we insist on extensional equality we cannot have choice generally.

In other words, if we insist on extensional equality and choice for the quantifier combination $\forall\exists$, the following does not generally hold:

(1) $$\forall x{:}\mathbf{D}\exists y{:}\mathbf{D}''(x = y/\sim) \rightarrow \exists f{:}\mathbf{D}\rightarrow\mathbf{D}''\forall x{:}\mathbf{D}\,(x = f(x)/\sim);$$

A general principle which seems very plausible[10] in this connection is the "presentation axiom": every domain **D** is quotient of a domain **D'** such that "choice" holds relative to **D'** (the elements of **D** are given as equivalence classes of elements of **D'**).

Notes

(1) This paper is a revision of a lecture held at Heyting '88, September 13-23, Chaika near Varna, Bulgaria. Substantial comments by W. Sieg induced me to revise the paper a second time. I am also indebted to J. Diller and M. van Lambalgen for helpful comments and conversations.

(2) Hersh (1970) states "three facts from mathematical experience": (a) mathematical objects are invented or created by humans; (b) they are created, not arbitrarily, but arise from activity with already existing mathematical objects, and from the needs of science and daily life, and (c) once created, mathematical objects have properties which are well-determined,which we may have great difficulty in discovering, but which are possessed independently of our knowledge of them. I certainly can agree with (a) and (b), but regard (c) as a very "dubious" fact, it certainly does not correspond to my mathematical experience; to me the combination of (a) and (c) seems to be a curious mixture of anti-platonism with platonism. We may feel that we have invented the notion of set (or that Cantor invented it), and in terms of set theory the continuum hypothesis presents itself as a perfectly definite statement, but I see little evidence so far that its truth is well-determined. To me therefore Hersh's "third fact from experience" seems rather a matter of belief - a belief to which I do not subscribe. N.B. Kreisel (1967) observes that in a second-order version of Zermelo's set theory with infinity axiom Z the truth of the continuum hypothesis CH is well-determined, that is to say for a suitable formalized version $\models_2$ of second-order consequence we have ($Z \models_2$ CH) or ($Z \models_2 \neg$CH). This does not settle CH however, since it turns out that ($Z \models_2$ CH) precisely if CH holds on the meta-level.

(3) As I read them, Bernays' concluding remarks in (1970) point somewhat in the same direction.

(4) Note that also there we do not get something for nothing: to see the correctness of the the W-rules (rules for tree classes or well-founded types) for the semantics given by Martin-Löf, requires an insight which amounts to a form of bar induction (Troelstra and van Dalen 1988, 11.7.6). Van Dalen (1990) makes the interesting suggestion that Dummett's theory is a candidate for filling a gap in Brouwer's philosophy, concerning the communication of mathematics.

(5) The sort of intuitive picture we associate with a system of axioms, and hence the significance of each axiom individually, may have to be revised if we add axioms. The defenders of a purely molecular theory of meaning will perhaps object that adding axioms corresponds to a change in the meaning of the primitive statements, not in the logic. I think there is reason to doubt whether this is generally correct. The fate of the logicist programme indicates already that it is difficult to separate "logic" from "mathematics", and this is certainly impossible if one takes the BHK-explanation of logic as fundamental, since the interpretation of quantifier combinations $\forall\exists$ is connected with the notion of function. The notion of function is at least partly determined by the axioms one accepts. Mathematics based on a molecular theory of meaning in any case imposes special restraints on the axioms one can accept (cf. also footnote 4).

(6) Many more positions are possible, e.g. finitist, or intuitionistic without absurdity in the sense of G.F.C. Griss, etc.

(7) Tait (1983) proposes to use the BHK-explanation in combination with the formulas-as-types concept as a universal schema, applicable to both classical and intuitionistic mathematics. Since classical logic is obtained by postulating additional introduction rules for certain types ((j) and (k) on page 189 of Tait 1983), Tait regards intuitionistic mathematics as part of classical mathematics. Contrary to Tait, I tend to regard the acceptance of (j) and (k) as an indication that indeed a different concept of function is involved. Tait's arguments for the inclusion of intuitionistic mathematics in classical mathematics seem to me to be entirely formal in character. (If one looks at things from the point of view of models, one might with some justification maintain that classical structures are special cases of intuitionistic structures.) It is not clear to me how theories of choice sequences fit into Tait's schema, unless one is prepared to "explain choice sequences away" in a purely formal manner. In this section on the other hand, we have tried to argue, not for an inclusion of one kind of mathematics into another, but for a greater degree of similarity in position between classical and intuitionistic mathematics than is commonly allowed, thereby excluding absolutistic claims for one of them.

(8) Simpson (1988) uses dramatic terminology: "It is also an embarrassing defeat for those who gleefully trumpeted Gödel's theorem as the death knell of finitistic reductionism", and "The need to defend the integrity of mathematics has not abated ... The assault rages as never before". Clearly I disagree with such a view of the situation.

(9) For Tait (1983), "the intensional concept of function" means "functions as rules". This differs from our use of intensional in this section. I am not certain that I follow Tait's argument, but I see no reason to disagree with his conclusion that in a typed context the extensional notion of function is more fundamental than the intensional one (in Tait's sense) (in fact, in the light of the preceding remarks this almost amounts to a triviality) provided the point made below, concerning the validity of (1), is not overlooked.

(10) How evident is the presentation axiom? If one wants to adopt the BHK-explanation of logic and evade the problems of "intensional equality" as a primitive notion, then the presentation axiom may be seen as a requirement on constructively acceptable domains (D,=), namely that each (D,=) is a quotient of a domain for which choice holds.

References

P. Bernays (1935), Sur le platonisme dans les mathématiques, L'Enseignement Mathématique 34,52-69. Translation into German: Bernays 1976, 62-78.

P. Bernays (1946), Quelques ponts de vue concernant le problème de l'évidence, Synthese 5, 321-326. Translation into German: Bernays 1976, 85-91.

P. Bernays (1950), Mathematische Existenz und Widerspruchsfreiheit, in: Études de Philosophie des Sciences, Éditions du Griffon, Neuchâtel, 465-471. Also in Bernays 1976, 92-106.

P. Bernays (1970), Die schematische Korrespondenz und die idealisierten Strukturen, Dialectica 24, 53-66.

P. Bernays (1976), Abhandlungen zur Philosophie der Mathematik, Wissenschaftliche Buchgesellschaft, Darmstadt.

E. Bishop (1967), Foundations of Constructive Analysis, McGraw-Hill, New York.

E. Bishop and D. Bridges (1988), Constructive Analysis, Springer Verlag, Berlin.

L.E.J. Brouwer (1947), Guidelines of intuitionistic mathematics, Indag. Math. 9, 197.

S. Buss (1986), Bounded Arithmetic, Bibliopolis, Napoli.

D. van Dalen (1990), Brouwer's dogma of languageless mathematics and its rôle in his writings. To appear in: E. Heijerman, H. W. Schmitz (eds), Significs, Mathematics and Semiotics - The Signific Movement in the Netherlands (Proceedings of the International Conference held at Bonn, November 19-21, 1986), John Benjamins Publ. Co., Amsterdam/ Philadelphia.

M.A.E. Dummett (1975), The philosophical basis of intuitionistic logic, in: H.E.Rose, J.E. Shepherdson (eds.) Logic Colloquium '73, North-Holland, Amsterdam, 5-40.

M.A.E. Dummett (1976), What is a theory of meaning? (II), in G.Evans, J.McDowell (eds.), Truth and Meaning, Oxford University press, Oxford, 67-137.

M.A.E. Dummett (1977), Elements of Intuitionism, Clarendon Press, Oxford.

A. S. Esenin-Vol'pin (1961), Le programme ultra-intuitionniste des fondements des mathématiques, in: Infinitistic Methods. Proceedings of the Symposium on the Foundations of Mathematics, PWN. Warsaw, 201-223.

A. S. Esenin-Vol'pin (1970), The ultra-intuitionistic criticism and the anti-traditional program for foun- dations of mathematics, in: A. Kino, J. Myhill, R.E.Vesley (editors), Intuitionism and Proof Theory, North-Holland Publ. Co., Amsterdam 1970, 3-45.

S. Feferman (1964), Systems of predicative analysis, J. Symb. Logic 29, 1-30.

S. Feferman (1987), Proof theory: a personal report, in: G. Takeuti, Proof Theory (second edition), North-Holland Publ. Co., Amsterdam, 447-485.

H.M. Friedman (1971), Algorithmic procedures, generalised Turing algorithms and elementary recursion theories, in: Logic Colloquium '69, North-Holland Publ. Co., Amsterdam, 361-389.

R.O. Gandy (1980) Church's thesis and principles for mechanisms, in: K.J. Barwise, J. Keisler, K. Kunen (editors), The Kleene Symposium, North-Holland Publ. Co., Amsterdam, 123-148.

R.O. Gandy (1982), Limitations to mathematical knowledge, in: D. van Dalen, D. Lascar, J. Smiley (editors, Logic Colloquium '80, North-Holland Publ. Co., Amsterdam, 129-146.

A. George (1988), The conveyability of intuitionism, an essay on mathematical cognition, J. Phil. Logic 17, 133-156.

M. Hallett (1984), Cantorian set theory and limitation of size, Clarendon Press, Oxford.

R. Hersh (1970), Some proposals for reviving the philosophy of mathematics, Advances in mathematics 31, 31-50.

A. Heyting (1949), Spanningen in de wiskunde (Dutch), Inaugural address, delivered at the University of Amsterdam, 16-V-1949, Euclides 25, 233-236, 301-312.

A. Heyting (1958), Intuitionism in mathematics, in: R. Klibansky (editor), Philosphy in the Mid-Century. A Survey, 101-115.

G. Kreisel (1960), La prédicativité, Bull. Soc. Math. France88, 371-379.

G. Kreisel (1962), The axiom of choice and the class of hyperarithmetic functions, Indagationes Mathematicae 24, 307-319.

G. Kreisel (1967), Informal rigour and completeness proofs, in: I. Lakatos (editor), Problems in the philosophy of mathematics, North-Holland Publ. Co., Amsterdam, 138-186.

M. van Lambalgen (1990), The axiomatization of randomness, to appear in J. Symb. Logic.

P. Lorenzen (1955), Einführung in die operative Logik und Mathematik, Springer Verlag, Berlin.

P. Lorenzen (1965), Differential und Integral. Eine konstruktive Einführung in die klassische Analysis. Akademische Velagsgesellschaft, Wiesbaden.

P. Maddy (1988), Believing the axioms I, II. J. Symb. Logic 53, 481-511, 736-764.

G. Mannoury (1909), Methodologisches und Philosophisches zur Elementar-Mathematik, Visser, Haarlem.

P. Martin-Löf (1984), Intuitionistic type theory. Notes by Giovanni Sambin of a series of lectures given in Padua, June 1980, Bibliopolis, Napoli.

G.E. Mints (1976), What can be done with PRA (Russian), Zapiski Nauchn. Semin. Leningr. Otd. Mat. Inst. Steklov. 60, 93-102. (Translation: J. Sov. Math. 14, 1487-1492).

E. Nelson (1986), Predicative arithmetic, Princeton University Press, Princeton, New Jersey.

R.J. Parikh (1971), Existence and feasibility in arithmetic, J. Symb. Logic 36, 494-508.

D. Prawitz (1977), Meaning and proofs: on the conflict between classical and intuitionistic logic, Theoria 43, 1-40.

W. Sieg (1985), Fragments of arithmetic, Annals of Pure and Applied Logic 28, 33-71.

S.G. Simpson (1988), Partial realizations of Hilbert's program, J. Symb. Logic 53, 349-363.

W.W. Tait (1983), Against intuitionism: constructive mathematics is part of classical mathematics, J. Phil. Logic 12, 173-195.

G. Takeuti (1978), Two applications of logic to mathematics, Princeton University Press, Princeton.

A.M. Turing (1937), On computable numbers, with an application to the Entscheidungsproblem, Proc. London Math. Soc. 42, 230-265, Corrections: Ibidem 43, 544-546.

A.S. Troelstra (1968), The theory of choice sequences, in : B. van Rootselaar, J. F. Staal (editors), Logic, Methodology and Philosophy of science III, North-Holland Publ. Co., Amsterdam, 201-223.

A.S. Troelstra (1982), On the origin and development of Brouwer's concept of choice sequence, in: A.S. Troelstra, D. van Dalen (eds), The L.E.J. Brouwer Centenary Symposium,

North-Holland Publ. Co., Amsterdam, 465-486.
A.S. Troelstra (1983), Analyzing choice sequences, J. Phil. Logic 12, 197-260.
A.S. Troelstra, D. van Dalen (1988), Constructivism in mathematics, an introduction (2 vols.), North- Holland Publ. Co., Amsterdam.
H. Weyl (1918), Das Kontinuum, Veit & Co., Leipzig.
C. Wright (1982), Strict Finitism, Synthese 51, 202-282.

Faculteit Wiskunde en Informatica
Universiteit van Amsterdam
Plantage Muidergracht 24
1018TV AMSTERDAM - NL

The ITLI Prepublication Series

1986
86-01 The Institute of Language, Logic and Information
86-02 Peter van Emde Boas — A Semantical Model for Integration and Modularization of Rules
86-03 Johan van Benthem — Categorial Grammar and Lambda Calculus
86-04 Reinhard Muskens — A Relational Formulation of the Theory of Types
86-05 Kenneth A. Bowen, Dick de Jongh — Some Complete Logics for Branched Time, Part I Well-founded Time, Forward looking Operators
86-06 Johan van Benthem — Logical Syntax

1987
87-01 Jeroen Groenendijk, Martin Stokhof — Type shifting Rules and the Semantics of Interrogatives
87-02 Renate Bartsch — Frame Representations and Discourse Representations
87-03 Jan Willem Klop, Roel de Vrijer — Unique Normal Forms for Lambda Calculus with Surjective Pairing
87-04 Johan van Benthem — Polyadic quantifiers
87-05 Víctor Sánchez Valencia — Traditional Logicians and de Morgan's Example
87-06 Eleonore Oversteegen — Temporal Adverbials in the Two Track Theory of Time
87-07 Johan van Benthem — Categorial Grammar and Type Theory
87-08 Renate Bartsch — The Construction of Properties under Perspectives
87-09 Herman Hendriks — Type Change in Semantics: The Scope of Quantification and Coordination

1988
Logic, Semantics and Philosophy of Language:
LP-88-01 Michiel van Lambalgen — Algorithmic Information Theory
LP-88-02 Yde Venema — Expressiveness and Completeness of an Interval Tense Logic
LP-88-03 Year Report 1987
LP-88-04 Reinhard Muskens — Going partial in Montague Grammar
LP-88-05 Johan van Benthem — Logical Constants across Varying Types
LP-88-06 Johan van Benthem — Semantic Parallels in Natural Language and Computation
LP-88-07 Renate Bartsch — Tenses, Aspects, and their Scopes in Discourse
LP-88-08 Jeroen Groenendijk, Martin Stokhof — Context and Information in Dynamic Semantics
LP-88-09 Theo M.V. Janssen — A mathematical model for the CAT framework of Eurotra
LP-88-10 Anneke Kleppe — A Blissymbolics Translation Program

Mathematical Logic and Foundations:
ML-88-01 Jaap van Oosten — Lifschitz' Realizabiility
ML-88-02 M.D.G. Swaen — The Arithmetical Fragment of Martin Löf's Type Theories with weak Σ-elimination
ML-88-03 Dick de Jongh, Frank Veltman — Provability Logics for Relative Interpretability
ML-88-04 A.S. Troelstra — On the Early History of Intuitionistic Logic
ML-88-05 A.S. Troelstra — Remarks on Intuitionism and the Philosophy of Mathematics

Computation and Complexity Theory:
CT-88-01 Ming Li, Paul M.B.Vitanyi — Two Decades of Applied Kolmogorov Complexity
CT-88-02 Michiel H.M. Smid — General Lower Bounds for the Partitioning of Range Trees
CT-88-03 Michiel H.M. Smid, Mark H. Overmars, Leen Torenvliet, Peter van Emde Boas — Maintaining Multiple Representations of Dynamic Data Structures
CT-88-04 Dick de Jongh, Lex Hendriks, Gerard R. Renardel de Lavalette — Computations in Fragments of Intuitionistic Propositional Logic
CT-88-05 Peter van Emde Boas — Machine Models and Simulations (revised version)
CT-88-06 Michiel H.M. Smid — A Data Structure for the Union-find Problem having good Single-Operation Complexity
CT-88-07 Johan van Benthem — Time, Logic and Computation
CT-88-08 Michiel H.M. Smid, Mark H. Overmars, Leen Torenvliet, Peter van Emde Boas — Multiple Representations of Dynamic Data Structures
CT-88-09 Theo M.V. Janssen — Towards a Universal Parsing Algorithm for Functional Grammar
CT-88-10 Edith Spaan, Leen Torenvliet, Peter van Emde Boas — Nondeterminism, Fairness and a Fundamental Analogy
CT-88-11 Sieger van Denneheuvel, Peter van Emde Boas — Towards implementing RL

Other prepublications:
X-88-01 Marc Jumelet — On Solovay's Completeness Theorem

1989
Logic, Semantics and Philosophy of Language:
LP-89-01 Johan van Benthem — The Fine-Structure of Categorial Semantics
LP-89-02 Jeroen Groenendijk, Martin Stokhof — Dynamic Predicate Logic, towards a compositional, non-representational semantics of discourse
LP-89-03 Yde Venema — Two-dimensional Modal Logics for Relation Algebras and Temporal Logic of Intervals
LP-89-04 Johan van Benthem — Language in Action
LP-89-05 Johan van Benthem — Modal Logic as a Theory of Information
LP-89-06 Andreja Prijatelj — Intensional Lambek Calculi: Theory and Application
LP-89-07 Heinrich Wansing — The Adequacy Problem for Sequential Propositional Logic
LP-89-08 Víctor Sánchez Valencia — Peirce's Propositional Logic: From Algebra to Graphs
LP-89-09 Zhisheng Huang — Dependency of Belief in Distributed Systems

Mathematical Logic and Foundations:
ML-89-01 Dick de Jongh, Albert Visser — Explicit Fixed Points for Interpretability Logic
ML-89-02 Roel de Vrijer — Extending the Lambda Calculus with Surjective Pairing is conservative
ML-89-03 Dick de Jongh, Franco Montagna — Rosser Orderings and Free Variables
ML-89-04 Dick de Jongh, Marc Jumelet, Franco Montagna — On the Proof of Solovay's Theorem
ML-89-05 Rineke Verbrugge — Σ-completeness and Bounded Arithmetic
ML-89-06 Michiel van Lambalgen — The Axiomatization of Randomness
ML-89-07 Dirk Roorda — Elementary Inductive Definitions in HA: from Strictly Positive towards Monotone
ML-89-08 Dirk Roorda — Investigations into Classical Linear Logic
ML-89-09 Alessandra Carbone — Provable Fixed points in $I\Delta_0+\Omega_1$

Computation and Complexity Theory:
CT-89-01 Michiel H.M. Smid — Dynamic Deferred Data Structures
CT-89-02 Peter van Emde Boas — Machine Models and Simulations
CT-89-03 Ming Li, Herman Neuféglise, Leen Torenvliet, Peter van Emde Boas — On Space Efficient Simulations
CT-89-04 Harry Buhrman, Leen Torenvliet — A Comparison of Reductions on Nondeterministic Space
CT-89-05 Pieter H. Hartel, Michiel H.M. Smid, Leen Torenvliet, Willem G. Vree — A Parallel Functional Implementation of Range Queries
CT-89-06 H.W. Lenstra, Jr. — Finding Isomorphisms between Finite Fields
CT-89-07 Ming Li, Paul M.B. Vitanyi — A Theory of Learning Simple Concepts under Simple Distributions and Average Case Complexity for the Universal Distribution (Prel. Version)
CT-89-08 Harry Buhrman, Steven Homer, Leen Torenvliet — Honest Reductions, Completeness and Nondeterminstic Complexity Classes
CT-89-09 Harry Buhrman, Edith Spaan, Leen Torenvliet — On Adaptive Resource Bounded Computations
CT-89-10 Sieger van Denneheuvel — The Rule Language RL/1
CT-89-11 Zhisheng Huang, Sieger van Denneheuvel, Peter van Emde Boas — Towards Functional Classification of Recursive Query Processing

Other Prepublications:
X-89-01 Marianne Kalsbeek — An Orey Sentence for Predicative Arithmetic
X-89-02 G. Wagemakers — New Foundations: a Survey of Quine's Set Theory
X-89-03 A.S. Troelstra — Index of the Heyting Nachlass
X-89-04 Jeroen Groenendijk, Martin Stokhof — Dynamic Montague Grammar, a first sketch
X-89-05 Maarten de Rijke — The Modal Theory of Inequality
X-89-06 Peter van Emde Boas — Een Relationele Semantiek voor Conceptueel Modelleren: Het RL-project

1990 SEE INSIDE BACK COVER

The ITLI Prepublication Series

1990

Computation and Complexity Theory

CT-90-01 John Tromp, Peter van Emde Boas — Associative Storage Modification Machines

Other Prepublications

X-90-01 A.S. Troelstra — Remarks on Intuitionism and the Philosophy of Mathematics, Revised Version