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REMARKS ON INTUITIONISM AND THE PHILOSOPHY OF MATHEMATICS

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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 least 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 a "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 eat 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 a way which carried conviction in their principal goal. 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". In a sense, platonism is therefore "next best" : the certainty rests in the objective reality of mathematical objects, the problem of certainty has been shifted to: how can we have discovered a truth about the abstract universe?

2. Absolute and relative certainty.

The remainder of this talk I want to use to describe my present-day views on the philosophy of mathematics in general, and on intuitionism in particular. I do not claim any originality for the ideas expressed here; I believe all the ingredients can be found elsewhere as well, though presumably not in the same combination or with the same emphasis.

First of all, I think that the aim of "absolute certainty" for mathematics is 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.

This 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."

I think that Brouwer also would have denied that intuitionism gave "absolute certainty".

Hersh (1970) in his paper "some proposals for reviving the philosophy of mathematics", also rejects the quest for absolute certainty, and after some discussion, states "three facts from mathematical experience":

(1) mathematical objects are invented or created by humans;

(2) they are created, not arbitrarily, but arise from activity with already existing mathematical objects, and from the needs of science and daily life, and

(3) 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 (1) and (2), but regard (3) as a very "dubious" fact, it certainly does not correspond to my mathematical experience; to me the combination of (1) and (3) 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[2] 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.

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; it seems to me that her conclusions tend in the same direction as mine.)

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. Mathematics as the science of idealized structures.

To set the stage for my remarks on the nature of mathematics, I take Bernays' view of mathematics as the science of idealized structures (1970); Bernays was in turn influenced by the Swiss mathematician and philosopher F. Gonseth, who viewed the relation between reality and theoretical science as a "schematic correspondence"; science does not faithfully represent reality, but corresponds to reality only schematically. The schematic character consists in the fact that the theoretical description is always adapted to the aspects one wants to study , and to pragmatical aspects (think of the passage from classical physics to quantum physics when moving from a macro- to a micro-level).

The schemata are a category in itself: the realm of mathematics. We observe structures; by idealizing and abstraction we obtain the idealized structures of mathematics. In the words of Bernays, "the idealization consists in an adaptation to the conceptual, a kind of compromise between intuition (Anschauung) and the conceptual". Bernays also observes that in constructive mathematics one tries to restrict the idealization, without fully banishing it.

To set the stage for the sequel, we give some further quotations from Bernays. In his (1955) he remarks:

"The philosophy of mathematics usually tends to substitute for acquired evidence (intuition) an evidence ab ovo, an evidence which is present from the beginning. Thereby one is either tempted to stretch the notion of evidence beyond its capacity, since one wants to grasp all attainable levels at once, which leads to paradoxes, or to posit the evidence at a particular level as absolute, which results in a restriction of mathematics, in such a way that we unnecessarily give up our freedom of deciding.

These flaws we can evade, if we do not regard mathematics itself as something which is self-evident. The element of familiarity we find in domains of mathematics, in particular elementary mathematics, is an acquired familiarity ..."

And again from Bernays (1970):

"When we take the idea of mathematics as the science of idealized structures as our basis, we have reached an attitude towards the foundations of mathematics which preserves from exaggerated despair and forced constructions and is also not threatened when we sometimes make surprising discoveries in the foundations of mathematics."

I do not want to claim that the view of mathematics as the science of schemata, which corresponds only schematically to our intuitions and cognitive powers solves everything; in a sense, it explains very little. It is something in the nature of a metaphor, evoking a picture of what mathematics is. If we talk about mathematics on this level of generality, we are unavoidably led to imprecise, evocative descriptions - since, borrowing from the title of Bernays (1955), mathematics is at the same time familiar and unknown.

4. The role 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.

This Brouwerian principle I read as being primarily directed against formalism; it marks the intuitionistic insistence on the <u>content</u> of mathematics.

But it should be clear that traditional intuitionism is a <u>theory</u> about human mathematical activity; as a theoretical construct it introduces some idealizing assumptions not fulfilled in acual 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, sharply defined.

Thus intuitionism is a very schematic description of human mathematical activity. In particular the assumption of languagelessness is quite obviously not fulfilled in practice.

It seems to me that it is possible to make sense of intuitionistic mathematics, without a sudden break with intuitionistic tradition, and also without accepting all the principles of intuitionism mentioned before. In particular I think we have no need of the postulate of "languagelessness". Which is not to say that I want to replace it by a <u>positive</u> doctrine as to the use of language. (The latter might be called for, if we want to justify in a certain context a principle such as Church's thesis: the possibility of <u>communicating</u> a complete description of a rule imposes constraints on the possible rules.)

5. 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 in 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.

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 form 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.

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 am inclined to 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 surveyability of infinite collections, in particular **N**, as an extrapolation of human cognitive powers, which leads to the acceptance of the principle of the excluded middle, 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 diccuss this here. From now on, we shall refer to restricted platonism only.).

In traditional intuitionism one does not freely experiment in exactly the same style as in axiomatic set theory. But 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" (a term coined, I believe, by G.Kreisel). 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.

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 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).

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.

6. Examples of informal rigour and 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 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 $\alpha 0, \alpha 1, \alpha 2, \ldots$ and at each stage we ask ourselves whether we can compute F from $\langle \alpha 0, \alpha 1, \ldots, \alpha n \rangle$, when we think of this initial segment as belonging to a lawless sequence (that is, we system- atically "forget" whatever further information we may possess about α, and try to apply F to a lawless sequence beginning with $\langle \alpha 0, \alpha 1, \ldots, \alpha n \rangle$). 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).

(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, although the principle of the analysis has remained the same.

(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 1987).

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 seems to be similar.

7. Actualism, intuitionism and platonism.

The various frameworks for mathematics (such as intuitionism, platonism, finitism etc.) vary widely in the degree of idealization (abstraction from the limitations) of human cognitive powers.

Platonism may be understood as a strong idealization of human cognitive powers, and is comparable with intuitionism, in a sense which I shall attempt to explain below.

But not only I want to compare intuitionism with platonism, I also want to contrast, on the other hand, intuitionism with actualism.

Actualism is also known as ultra-finitism or ultra-intuitionism, and its supporters by no means present a uniform picture; in fact one is struck by the diversity of the various actualistic approaches.

Nevertheless, to keep the discussion simple, I shall talk as if there is a single "actualist philosophy" and a body of actualist mathematics in keeping with this philosophy. On the negative side, the actualists (e.g. Esenin-Vol'pin 1961, 1970, Parikh 1971, C.Wright 1982, R.O. Gandy 1982, E. Nelson 1986) agree: Intuitionism itself contains a strong idealization, in treating all natural numbers as in principle representable by a sequence of units (concretely represented by strokes, for example), even $10\uparrow(10\uparrow10)$ (where $n \uparrow m \equiv n^m$). More precisely, in the actualist view we either regard as natural numbers only those numbers which permit a concrete representation of units, and then $10\uparrow(10\uparrow10)$ presumably cannot be regarded as a natural number, or we 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 any way representable.

Or, if we approach things from a slightly different angle (Nelson 1986), why should we assume exponentiation to be everywhere defined? And is there not a suspect form of impredicativity in unrestricted induction, since (a) induction is the principle par excellence to characterize **N** in the traditional theories, but on the other hand (b) it is applied to properties invoving quantification over **N**, i.e. which presuppose the totality which the principle is supposed to characterize.

The observation that there is a genuine difference between our understanding of say 5, and $10\uparrow(10\uparrow10)$ is quite old (going back to Mannoury 1909, at least); actualists are people who want to <u>do</u> someting about it, that is to say, an actualist thinks that the difference just mentioned should be visible in our development of mathematics.

It has been said that actualism is incoherent. Personally I believe that a coherent actualist philosophy is possible, though not quite achieved yet; but in any case mathematical theories which make visible some of the distinctions introduced by actualism are perfectly well possible, as witnessed by certain weak subsystems where **N** is closed under addition and multiplication but not necessarily under exponentiation. But I have another, and for the purposes of this paper more important, reason to be interested in the actualist criticism of intuitionist and platonist practice as regards **N**.

Dummett has advanced, in a number of publications, 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 (in particular George 1988, also Wright 1982, Troelstra and van Dalen 1988 l.c.). This actualist criticism will play an important rôle in what follows.

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 is <u>our human powers</u> 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 actualists 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.

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.

In Troelstra and van Dalen (1988, section 16.4) doubts are raised as to whether in mathematical practice a molecular theory of meaning[3] is really adequate. 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[4]. It seems to me that meaning in mathematics is neither all-out holistic nor purely molecular.

More drastically, 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.

To summarize the preceding discussion, the actualist criticism makes us aware of the fact that 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. Thus it seems that intuitionism and platonism have more in common than is commonly thought; they represent only two possible sets of idealizing assumptions[5] relative to our actual cognitive powers. In view of this, the differences between intuitionism and (restricted) platonism[6] lose their pungency; there is not any longer a sharp 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 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 is perhaps 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 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 that we needed proof- theoretically strong principles for modern mathematics (such as the powerset axiom in **ZF**, and impredicative comprehension in higher-order logic). Recent work under the (rather inappropriate and misleading) label of "reverse mathematics", investigating the minimum strength needed to prove certain important theorems from various areas of mathematics has shown that very often we need far less than what was suggested by the standard proofs[7]. 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 predficative theory (without an excessive increase in complexity of the proofs, cf. the survey 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, play 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[8], 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

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

if we may assume that f repects =, 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:

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

A general principle which seems very plausible[9] in this connection is the "presentation axiom": every domain $\mathbf{D}$ is quotient of a domain $\mathbf{D}'$ such that "choice" holds relative to $\mathbf{D}'$ (the elements of $\mathbf{D}$ are <u>given</u> as equivalence classes of elements of $\mathbf{D}'$).

Notes

(1) This paper is a revision of a lecture held at Heyting '88, September 13-23, Chaika near Varna, Bulgaria.

(2) 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 $\vdash_2$ of second-order consequence we have ($Z \vdash_2$ CH) or ($Z \vdash_2 \neg$ CH). This does no settle CH however, since it turns out that ($Z \vdash_2$ CH) precisely if CH holds on the meta-level.

(3) The work of Martin-Löf (see e.g. his 1984) may be regarded as an interpretation of Dummett's idea concerning a "molecular" theory of meaning. But 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).

(4) 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 3).

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

(6) 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 intutionistic mathematics than is commonly allowed, thereby excluding absolutistic claims for one of them.

(7) 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". I find such language embarrassing.

(8) 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 above, concerning the validity of (1), is not overlooked.

(9) 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.

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