

FOUNDATIONS OF MATHEMATICS OR MATHEMATICAL PRACTICE: IS ONE FORCED TO CHOOSE?*

Jean Paul Van Bendegem

Introduction.

Philosophers of mathematics can be roughly divided into two groups. Type I is particularly fond of questions such as: What are *the* foundations of mathematics? What are numbers? What is a set? What is Church's thesis really about? What is decidability? What is infinity? What is mathematical truth? These questions are all situated within mathematics proper. Formalists, logicians, intuitionists, constructivists, finitists, strict and otherwise, are in this sense definitely type I. Type II however wants answers to questions such as: How is mathematics done? What is a *real* mathematical proof? Why do mathematicians make such a fuss over the use of computers in order to find and construct proofs? Is it possible to gather evidence as to the plausibility of the correctness of a mathematical statement? How is it possible that an accepted proof turns out to be wrong? Type II is still a rare species but happily enough - that is, if you happen to be type II as well - this is changing.¹ But it would be an exaggeration to claim that something like a theory of mathematical practice, mathematics as it is done, exists. There are plenty of ideas, plenty of detailed studies, but no general framework. I take it that hardly any argument, in fact none, is needed to show the importance of such a theory. If you wish to study problems having to do with the educational aspects of mathematics, or the diverse and complex relations between mathematics and the culture at large, or the psychological and social processes of mathematical invention and construction, you will obviously need a theory or at least a model of what mathematical practice is about.

In this short paper I do not have the intention nor the pretence to present (the outline of) such a theory or model. My aim is quite modest although the point I wish to present is a, philosophically speaking, important one. In the search for this model or theory of mathematical practice, most type II research-

ers seem to agree that models and theories used by type I philosophers of mathematics are not interesting. After all, their approach is a highly normative one, ignoring all aspects of real mathematical life. Either these models are criticized, or they are just simply ignored. My point is that, although acknowledging that type I and type II researchers are in really different fields, their theories and models are (to use a fashionable term) to a large extent, commensurable. The basis allowing for the possibility of commensurability, is constituted by the notion of an *artificial mathematician*. In Type I research, there are plenty of artificial mathematicians around. The two most famous ones are Hilbert's ideal mathematician and Brouwer's creative subject. In Type II research, we obviously are talking about real mathematicians. It is therefore a natural question to ask whether real and artificial mathematicians are related. And if so, can these possible relations form the background on which to compare type I and type II theories. As will be shown, there is a gradual transition from extreme Type I theories to extreme Type II theories.

The God-like mathematician.

No doubt most working mathematicians assume set theory - i.e. ZFC, Zermelo-Fraenkel set theory with Axiom of Choice - as the best (type I) foundations around for mathematics at the present moment. The standard formulation consists of (i) some version of classical first-order logic and (ii) the typical set-theoretical axioms. In such a foundational theory no mention is made of a mathematician. The set-theorist will (rightly) claim that the logical axioms and rules mention only the logical signs and the set axioms mention only sets and operations on sets. However the fact that no properties of a mathematician are listed implicitly or explicitly in the theory, does not imply that therefore the theory deals only with mathematics and not with mathematicians. A straightforward way to associate a mathematician with a mathematical theory is quite simply to ask the following question. Suppose there is a being that has the property that it knows everything that the mathematical theory claims. What properties does that being have? Note that the question is a trivial one if asked in a type II approach. In that case one starts with the mathematician (or the mathematical community) and then studies how the mathematician does mathematics. The question is less trivial when asked in a type I context. In order to clarify this strategy, let me present a first example.

Most mathematicians would agree on the following statements: (i) there is something like a mathematical universe, (ii) this universe is unique and (iii) in it all mathematical problems are settled. The mathematicians' task is to discover and chart this universe, with the knowledge that a complete map is impossible. But suppose that there is a being with the property that it has full knowledge of the mathematical universe. What epistemic properties does this being possess? Two important properties follow straight away. First, its knowledge is strongly complete. By (iii), all mathematical problems are settled, therefore given a mathematical problem or statement A, either A is the case in the mathematical universe or not-A is the case in the mathematical universe. Secondly, by (i) and (ii), its knowledge is weakly complete as well. In model-theoretic terms, (i) guarantees the existence of a model, whereas (ii) guarantees the uniqueness of this model. If this hypothetical mathematician has full knowledge of this model, this obviously implies the weak completeness. From these two properties, a third, crucial one is derived: this being must have truly god-like powers! The reason is quite simple. For an epistemic subject, to know a strongly and weakly complete first-order theory, implies it must have an actual infinite capacity to store knowledge. If the capacity were restricted to potential infinity, then undecidability results become unavoidable and full knowledge of the mathematical universe is no longer possible. Errett Bishop summarized his critique on classical mathematics when he wrote in his [1976]²: "... classical mathematics concerns itself with operations that can be carried out by God" and "If God has mathematics of his own that needs to be done, let him do it himself". In terms of the above analysis, an even stronger statement can be made: he is the only one who can do it, he has to do it himself.

It is perhaps interesting to present an example of the epistemic strength of this God-mathematician (GM). Bishop himself introduced in his [1985]³ the following example. Let (A_n) be a binary sequence. Then the GM will accept the following principle, the so-called *Limited Principle of Omniscience* (LPO): Either there is an n such that $A_n = 1$, or else $A_n = 0$ for all n . If we assume that GM can indeed decide this problem, then he can solve the following problem. Take an unsolved mathematical problem, e.g. Fermat's Last Theorem (FLT) or Goldbach's Conjecture. Now consider the following sequence: in the sequence (A_n) , $A_n = 1$ if FLT is provable and $A_n = 0$ if not-FLT is provable. Obviously the sequence (A_n) will consist either of all ones, or of all zeros. However it is not obvious at all which one is the case (at least for us, human mortals). Yet, if LPO holds, GM

can decide the matter. Thus GM can decide whether FLT or not-FLT holds. Note that for GM mathematics ceases to be an interesting enterprise, for the simple reason that everything is already known.

It is interesting to note the close similarity between GM and the Demon of Laplace. In the very same sense that Laplace's Demon corresponds to the ideal physicist, GM corresponds to the ideal mathematician. For the Demon too, the universe ceases to be an interesting place, as it holds no secrets. For the Demon too, time ceases to be real, just as GM lives in a timeless realm. One might well wonder whether the parallel breakdown of the Demon and GM is related or not.

The constructivist mathematician.

If God-like mathematicians have little or nothing to do with us, are we not best advised to scale this hypothetical being down to our size? Basically, there are two options : (i) assume the existence of a unique, mathematical universe, but deny one can have a full knowledge of it, and (ii) deny the existence of a unique mathematical universe altogether. The second option corresponds roughly to the route taken by Brouwer, whereas the first option is currently explored in epistemic mathematics⁴. The crucial difference between these two approaches is directly linked to the discovery-construction distinction. Are we wondering around in a mathematical universe wherein we discover mathematical theorems, or are we just exploring a creation of our own making? I will not go into this discussion - this is a quite separate topic - for it is sufficient to note that in both cases the answer will be the same to the following question: what is the epistemic content of a hypothetical mathematician whose capacity is limited to potential infinity? The answer is: what is accessible to the mathematician on the basis of construction and proof. Although perhaps at first sight, this answer may seem a clear one, it is nevertheless highly ambiguous. The history of (the philosophy of) mathematics has shown us that there are many different ways to sharpen this answer. In other words, there are many constructivist mathematics (CM) imaginable. However, as I will argue, they all share a set of non-human properties. Or, to put it differently, CM still has some distinctly type I properties that distinguish it clearly from a type II mathematician. Thus the differences do not appear to be essential for the argumentation presented in this paper. Nevertheless, let me briefly present three examples to illustrate the richness of the constructivist

approach.

For the intuitionist, CM(I) knows A if there is in principle, a proof or construction of A available, i.e. CM(I) is capable of producing a proof of A, or a construction for A. Obviously, CM(I) will reject LPO. But CM(I) will also reject Markov's principle (MP): If (A_n) is a binary sequence such that it is not the case for all n , that $A_n = 0$, then there is a n such that $A_n = 1$. The reason is that for the intuitionist not-A means that given a proof or construction of A, this proof or construction can be extended into a proof of something absurd or into an impossible construction. Thus not-A stands for "If A, then absurdity". In the case of MP, if CM(I) has shown that it is not the case that for all n , $A_n = 0$, then he has only shown that the assumption that all $A_n = 0$ leads to an absurdity. This given him no clue as to how the n , such that $A_n = 1$, can be found or constructed.

The Russian constructivist, CM(R), however accepts MP. The reason here is that the notion of construction is replaced by the notion of algorithm in an extended sense. Cases such that, on the one hand, one knows that the algorithm will end on a certain input, but, on the other hand, no finite bound can be specified beforehand, are accepted. On the other hand, CM(R) will reject some intuitionist principles, such as the Fan Theorem (FT).

A third version is Bishop's constructivist, CM(B). This is the weakest version, as neither MP nor e.g. FT are accepted. The main advantage of Bishop's constructivism is that it is consistent with classical analysis (assuming the consistency of the latter, of course). Both intuitionist and Russian constructivism are extensions of Bishop's constructivism but both are inconsistent with classical analysis. Furthermore, intuitionism is inconsistent with Russian constructivism. Note too, that these three approaches do not exhaust the whole range of constructivist theories. I refer the reader to Beeson's [1985]⁵ for an overview.

Let me now return to the main line of the argument. What properties of CM(x) - where x is your favourite brand of constructivism - are still clearly of type I. Basically, there are two aspects of prime importance. Actually, these two problems will appear only too familiar to anyone acquainted with epistemic logic.⁶

The first problem has to do with true knowledge. If CM(x) knows A, then A must be the case. In other words, the case wherein CM(x) knows A, but not-A is a mathematical theorem, does not occur. I will refer to this principle as IE, the *principle of Immunity of Error*. It is hardly necessary to argue that IE is a typical type I property. Real mathematicians do believe impossible things from time to time. They did e.g. believe such non-

sensical statements as $(\sqrt{-1})^2 = -1$. Moreover they knew that these statements were nonsensical. The fact that these mathematicians were fully aware of the absurdity involved, shows that an argument of the following type does not apply. One might propose to weaken the IE-principle. Instead one could adopt the principle *wIE* (the *weak Immunity Principle*): If one knows that one knows A, then A must be the case. In other words, just knowing A does not guarantee the correctness of A. But, as said, that does not work. And it is rather useless, to weaken *wIE* even further, for what meaning could be given to the statement that 'One knows that one knows that one knows that A without it being the fact that one knows that one knows that A'? Furthermore, they managed to deal with these absurdities and to derive interesting, important and, above all, correct mathematical conclusions from them. To quote another famous historical example, Berkeley did show convincingly that Newton's treatment of infinitesimals was inconsistent, but most historians will agree that it was a good thing for the development of mathematics, analysis in particular, that Newton largely ignored this criticism and continued to develop this inconsistent theory. Actually, with the advent of non-standard analysis, one could argue that consistent talk about infinitesimals is, in fact, possible.

The second problem has to do with the principle IC, the *principle of Immediate Consequences*. Suppose that $CM(x)$ knows A and that B is a logical consequence of A. Then $CM(x)$ must also know B. IC is surely acceptable, for it says nothing but: if you know A and there is a proof - according to your favourite brand x - of B from A, then surely you must know B. But if this is acceptable, then it has the immediate, startling conclusion that if $CM(x)$ knows A, then $CM(x)$ must know *all* logical consequences from A. And this seems less or not at all acceptable when discussing real mathematicians. Obviously no real mathematician has such insight. I mentioned at the beginning of this paper, ZFC as the foundations used today by most working mathematicians. Every mathematician who knows these axioms, therefore knows all the logical consequences of these axioms, i.e. (s)he knows all the theorems of set theory. One might object that for $CM(x)$ to know that B is a logical consequence of A, means that $CM(x)$ has a proof *in principle* of B from A. Thus, to know a logical consequence, means quite simply to be able to present a proof when asked to do so. But this only increases the mystery: what kind of knowledge is this knowledge of "proofs in principle"? One way or another, this must reduce to having direct access to the mathematical universe, where one can "see" whether B is a logical consequence of A or not. True, $CM(x)$ can

only see part of the universe, nevertheless, it is somewhat startling to come to the conclusion that GM and CM(x) are closer relatives than one might have imagined.

The finitist mathematician.

How should we modify CM(x) such that the IE principle and the IM principle no longer hold? In order to reject the IM principle, it is sufficient to replace the notion of proof *in principle* by the notion of *real* proof. A real proof is characterized by the fact that it should be recognizable as a proof by a mathematician that is bounded in time and in space. Real proofs are sequences of signs written in some language or other. It seems appropriate to call a mathematician thus limited, a finite mathematician (FM). Actually, in this case too, it would be better to speak of FM(x) for, as Ernst Welti has shown in his excellent, historical study, there are many types of finitist mathematics, strict or otherwise, around.⁷ However, just as in the constructivist's case, it is not necessary to go into details. It is easy enough to see that the presence of finite bounds must result in the violation of the IM principle. For suppose, to keep matters simple, that an overall bound, say L, is defined on the length of proofs. FM can only check and thus accept or reject proofs below a certain upper bound. Suppose further that FM has accepted A as a theorem after inspecting the proof of A, having a length less than L. Finally, suppose that FM has also accepted a proof of 'if A, then B', this proof equally having a length less than L. It does not follow that therefore FM has to accept the proof of B, since the proof of B may have a length larger than L. For the proof of B will be the result of the concatenation of the proof of A and of the proof of 'if A, then B'. Thus it is rather easy to reject the IM principle. The IE principle, however, is a quite different problem.

On the one hand, it is obvious that the possibility of error should be allowed. The history of mathematics presents an interesting story of, what one could call, *creative* mistakes. Precisely because mistakes were made, the mathematical community was able to see the next step to take. But, on the other hand, it is not clear at all how one should proceed. What principles can be formulated about an artificial mathematician that allow this being to make mistakes *and* to learn from them? Two alternatives present themselves. The first one is to replace the underlying contradiction-free logic of mathematics by a paraconsistent or a dialectical logic.⁸ The possibility is then allowed

for to accept that 'if FM knows A, then A is the case', that 'FM knows A', yet that 'not-A is the case'. However, this first alternative will surely have to be supplemented by some methods for 'repairing' the error. But, as must be obvious, these methods cannot be algorithms. If they were, it would be sufficient to apply them each time a contradiction arises thus establishing a modified form of the IE-principle. If an error occurs, it can be 'calculated away'. Thus heuristics have to be introduced. Innocent though this conclusion may seem, it is of fundamental importance. So far, we always assumed that whatever the artificial mathematician learns about the mathematical universe, it is learned truthfully. At this point, the possibility is introduced that the artificial mathematician may be misled by what he or she thinks to be the case in the mathematical universe. In other words, this universe itself can no longer be used as a justificatory device. FM can no longer say, 'I believe or I know A, because A is a mathematical fact, and, therefore, true in the mathematical universe.' FM will have to look for other criteria to convince himself or herself, that he or she knows A truthfully.

The real individual mathematician.

The type I philosopher might remark at this point that it is clearly impossible to have mathematicians making errors. If we restrict ourselves to FM-like mathematicians, then any proof presented will be a surveyable proof because of the limits imposed on time and place resources. But a surveyable proof can decidably be found out to be error-free or not. If not, the error can be located and repaired. Why, then, do we need the heuristics? The answer, in all its simplicity, is this: what most mathematicians write and read most of the time are *not* proofs in the formal sense of the word. They are, what I have called elsewhere *proof-outlines*.⁹ That is, what are presented, are the major steps in the proof. The mathematician who writes the proof, thereby assumes that a trained mathematician with sufficiently knowledge of the particular mathematical field the proof is about, is capable to fill in the missing steps. The problem is not that mathematicians should be accused of laziness or sloppiness, the matter is quite simply that the demand of, formally speaking, correct proofs, is an impossible one. If something has been made clear by *Principia Mathematica*, then surely, it is the fact that that is not the way to do mathematics. If errors occur in a proof-outline, they do because the mathematician assumed wrongly that a particular step could be filled in. Therefore, errors are likely

to occur - the history of mathematics tells us so¹⁰ - and heuristics are needed to repair these errors.

To illustrate this thesis, let me present three heuristics that have been frequently employed in mathematics to search for errors and to repair the damage if error occurred.

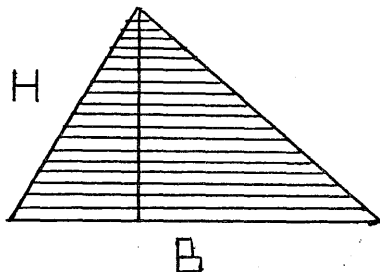
The first example is well-known from Lakatos' brilliant study *Proofs and Refutations* on Euler's conjecture, $V-E+F = 2$, i.e. the statement that, given a polyhedron, the number of vertices minus the number of edges plus the number of faces always equals two. The three-part heuristic Lakatos arrives at, is the following:

"Rule 1. If you have a conjecture, set out to prove it and to refute it. Inspect the proof carefully to prepare a list of non-trivial lemmas (proof-analysis); find counterexamples both to the conjecture (global counterexamples) and to the suspect lemmas (local counterexamples).

Rule 2. If you have a global counterexample discard your conjecture, add to your proof-analysis a suitable lemma that will be refuted by the counterexample, and replace the discarded conjecture by an improved one that incorporates that lemma as a condition. Do not allow a refutation to be dismissed as a monster. Try to make all '*hidden lemmas*' explicit.

Rule 3. If you have a local counterexample, check to see whether it is not also a global counterexample. If it is, you can easily apply Rule 2."¹¹

The second example concerns a heuristic that I have labelled *confining inconsistencies*.¹² Especially, in the pre-Newtonian and pre-Leibnizian period in the development of analysis, many mathematicians - Giles Persone de Roberval, John Wallis, François Viète to name but a few - were developing mathematical theories that were clearly inconsistent. However they did manage to work with these inconsistencies because in many cases these inconsistencies were confined. An example may clarify the matter. Wallis had the following beautiful proof for the area of a triangle.¹³



The triangle is divided into an infinite number, ω , of lines. The area of the triangle is the sum of all the tiny rectangles. Each of these rectangles has a height H/ω and a length b . Thus the area of the triangle is equal to $\sum b.H/\omega$ or $H/\omega.\sum b$. $\sum b$ is an arithmetical progression with an infinite number of terms, with first term B and last term 0 . Hence $\sum b = B.\omega/2$. Thus the area is equal to $H/\omega.B.\omega/2 = H.B/2$. Wallis knew that was the result he should obtain. Because there are many ways, and different methods to obtain the area of a triangle. Although the proof seems hilarious – at least to modern eyes – the conclusion is correct. Therefore in any other proof in which the area of a triangle had to be calculated, Wallis could safely insert the above proof. In this sense, the inconsistency is confined, it is not allowed, so to speak, to escape from the proof wherein it occurs.¹⁴

The third example is a heuristic, so familiar in mathematics, that probably, most mathematicians are not aware of the crucial role it plays: *multiple proofs* or proof-outlines. Mathematicians do spend a lot of their time, rewriting proofs and searching for different proofs of theorems that already have been proved. For some famous theorems, the list is quite impressive: there are no less than 96 different proofs of the Pythagoras theorem.¹⁵ Every mathematician knows at least two proofs of the existence of an infinite number of primes (the classical Euclidean proof and the proof related to the Riemann zeta-function). In another paper, I have shown how this heuristic plays an important role in evaluating the importance of a mathematical problem.¹⁶

One important note should be added here: the heuristics presented here, are clearly *rough* heuristics. True, they are more specific than e.g. Polya's first heuristics in *How To Solve It*. But, tasks such as, 'Try to prove the conjecture' and 'Find an alternative proof' are not exactly helpful for the working mathematician. What is needed, is a worked-out theory of more specific, domain-related detailed heuristics. In the area of Automated Reasoning, one of the many branches of Artificial Intelligence, this is precisely what one is looking for. However, the way things look at the present moment, it is still the case that only formal proofs are considered. Up to my knowledge, no work is being done on the level of proof-outlines. On the other hand, it must be mentioned that within the computer world, non-monotonic logic is highly developed. This logic enables its user to revise currently held beliefs. In this sense, it shares some properties with paraconsistent and dialectical logics, although very different from it.¹⁷ As indicated while discussing the finitist mathematician, this is certainly needed as a crucial element in any theory of the real individual mathematician.

The real social mathematician.

Individual heuristics, however, do not tell the whole story. Of course, it would be an easy way out to claim that the mathematician, as part of the mathematical community, only exists *as a mathematician* in virtue of his or her membership of that community. But, that does not explain why, if one is interested in understanding the dynamics of mathematical change (as type II philosophers are), social elements should be taken into account. As in the case of the use of heuristics, I believe there to be at least two major arguments in support of this thesis.

The first argument relates to a point, argued for in the preceding paragraph. Mathematicians do not write proofs, but proof-outlines. Proof-outlines do not have a standard form in the sense that formal proofs do. They are not a sequence of formulas, where each formula is either an axiom or the result of the application of a derivation rule on formulas already occurring in the list. Instead they can take many different forms. As an example, compare these two proofs of the same theorem, namely the fundamental theorem of arithmetic. (Two minor notational changes have been introduced. Instead of using subscripts, the notation a^b is used to indicate a with subscript b . a to the power b is written $[a,b]$)¹⁸

Version 1. To prove the result, note first that if a prime p divides a product mn of natural numbers then either p divides m or p divides n . Indeed if p does not divide m then $(p,m) = 1$ whence there exist integers x, y such that $px + my = 1$; thus we have $pnx + mny = n$ and hence p divides n . More generally we conclude that if p divides $n^1 n^2 \dots n^k$ then p divides n^l for some l . Now suppose that, apart from the factorization $N = [p^1, j^1] \dots [p^k, j^k]$ derived above, there is another decomposition and that p' is one of the primes occurring therein. From the preceding conclusion we obtain $p' = p^l$ for some l . Hence we deduce that, if the standard factorization for N/p' is unique, then so also is that for N . The fundamental theorem follows by induction.

Version 2. First, N must have at least one representation, $N = [p^1, a^1][p^2, a^2] \dots [p^n, a^n]$ (1). Let a be the *smallest* divisor of N which is > 1 . It must be prime, since if not, a would have a divisor > 1 and $< a$. This divisor, $< a$, would divide N and this contradicts the definition of a . Write a now as p^1 , and the quotient N/p^1 , as N^1 . Repeat the process with N^1 . The process must terminate, since $N > N^1 > N^2 > \dots > 1$. This generates Eq. (1). Now if there were a second representation, by the corollary of Theorem 6, each p^i must equal some q^i , since $p^i | N$. Likewise

each q^i must equal some p^i . Therefore $p^i = q^i$ and $m = n$. If $b^i > a^i$, divide $[p^i, a^i]$ into Eqs. (1) and (2). (Note: Eq. (2) is the second representation: $[q^1, b^1][q^2, b^2] \dots [q^n, b^n]$). Then p^i would divide the quotient in Eq. (2) but not in Eq. (1). This contradiction shows that $a^i = b^i$.

These two proofs are sufficiently different to warrant the introduction of the notion of *style* in mathematics. It is not an exaggeration to claim that a mathematician develops a certain type of style and that one can identify him or her by it. It also implies - and here the social element enters the picture - that mathematicians sharing the same style will understand each other better. After all, they do speak the same language, or, should one say, the same mathematical dialect. Seen from this perspective, the Bourbaki project, apart from its mathematical content, was an equally important project in its proposal for a new mathematical style. The Bourbaki volumes aspired to be a new foundations of mathematics, but at the same time, they constituted a manual of style for it.

The second argument has to do with a recent, intertwined, two-fold development or, better, change in mathematical practice.

Long proofs are not uncommon in mathematics, as is well-known. However, it is a quite recent phenomenon that some proofs turn out to be so long that an individual mathematician is incapable of surveying it. The exemplar in this case is the classification theorem of finite groups, estimated at about 15.000 pages.¹⁹ Instead one can only claim that the proof is socially surveyable, not individually surveyable. Mathematician A has checked part X and mathematician B part Y, and putting their efforts together, they come to the conclusion that the whole proof is correct. Neither A nor B individually can make this claim, but together they can. Or, in other words, the proof as a *mathematically accepted proof*, exists only on the social level. Hence, the basic unit to consider is not the individual mathematician, but the mathematical community.

The related part has to do with computer-proofs. Since the 'drama' of the four-colour theorem, it has become apparent that the presence of the computer as a symbol manipulating device, must have its effect on mathematical practice. If part of the proof has been carried out by computer, and the calculations are that cumbersome and intricate that neither a human mathematician, nor the mathematical community, is likely to check it in detail, are we then in a position to accept the proof? If one is tempted to answer 'yes' to this question, then one must accept the conclusion that 'proof', as classically understood, is not the only way to establish new mathematical results. This is really

going beyond heuristics, for heuristics point the way to a classical proof, whereas, in the computer case, this computer calculations are the best available. The problem is not a recent one. Mark Steiner in his *Mathematical Knowledge*²⁰ already made a case for other methods, besides mathematical proof (in the classical sense, i.e. up to the real individual mathematician (RIM)), to establish the truth of a mathematical proposition. Perhaps one is not inclined to follow along such a route, but, if understanding mathematical practice is the goal, these aspects will have to be taken into account.

Although the real social mathematician (RSM) is not the end of the continuum - surely we should go further and consider the real social mathematician in society at large (RSLM) - I hope to have made a convincing case for the idea that GM and RSM, although worlds apart, are related.

A tentative conclusion.

The subject of this paper, basically, was to answer this question: If X is any type of mathematician, then for X to know A, where A is a mathematical statement, means exactly what? We have progressed from the God-like mathematician, GM, for whom the answer was quite straightforward. For GM to know A, is simply equivalent to A being true in the unique mathematical universe. Along come the constructivists who want to scale down GM to some kind of 'ideally real' mathematician CM(x). CM(x) knows A if CM(x) has a proof or construction available of A, in principle. Replacing the 'in principle' part by 'actually', CM(x) is transformed into some kind of finitist mathematician, FM. But - relying on some well-known arguments about epistemic logic - as it turns out, even FM is still a highly idealized being. Establishing a link with the real - individual or socialized - mathematician must force us into introducing elements in the story that one would perhaps not expect in the mathematical context: heuristics, failure and error (and therefore revision), style (and therefore aesthetics) and the social coherence of the mathematical community.

Although it is clear that the transition from GM to RSM, is a gradual one, the differences between the extremes of the continuum are enormous. But that has been known all along. The more interesting part is that it is gradual. Seen from the viewpoint of the RIM or the RSM, FM, CM(x) and GM are to be seen as increasing, thereby, simplifying and helpful abstractions. Note too that all mathematicians mentioned, artificial and otherwise,

are only snapshots from an immense gallery of possibilities.

Perhaps the reader wonders why I am so insistent on this point. Basically, there are two reasons. The first reason has to do with our understanding of RIM and RSM. As said, FM, CM(x) and the like, may turn out to be very helpful fictions, in much the same way, that propositional logic is a quite interesting, yet highly fictional logic. In this paper, I hope to have made the point that, e.g., epistemic logic is really worth while to look into. Thereby, I am also claiming that the project to formulate a theory of mathematical practice, will benefit from the use of formal tools such as epistemic logic. In the best of cases, it should be possible to formulate theorems about the nature of mathematical practice. As must be obvious, this position is *not* similar to Wittgenstein's attitude. Without going into details, one example may suffice to make the distinction clear. For Wittgenstein, the social coherence of the mathematical community, does not need to be explained for. It just happens to be that way, and it would not make sense to ask a mathematician why he or she is willing to accept the verdict of his or her colleague as final.²¹ In this case, I want to find arguments that explain the (necessity of the) coherence. One argument mentioned surely is that, if A wants to check the proof of B, it increases efficiency, if A and B share the same mathematical style. But the latter feature is precisely an important element that contributes to social coherence. It is, at the same time, a refusal to let the history, psychology, sociology and economy of mathematics degenerate into a loose collection of interesting, anecdotal, therefore accidental, bits and pieces. The second reason, related to the first one, is that it is still possible to maintain the existence of a unique mathematical universe while holding the view that the way mathematics is done, is best described using a RIM or RSM type of model. What is said here, will sound only too familiar to any philosopher. The only thing that I am claiming, is that the minimal realist position holds for mathematics as well. It does not follow - and I emphasize this point most strongly - that taking a sociological, psychological or whatever point of view, implies the impossibility of the existence of something like, *the* mathematics. True, one might argue, that a separate entity such as the unique mathematical universe, is not called for, but, as must be clear, an appeal to the practice of mathematics to deny its existence, does not carry the force many authors expect or want it to do.

It would be wishful thinking to believe that the above plea will bring together type I and type II philosophers of mathematics. Perhaps they do not need to be brought together physically. If a common language is available - and a modest proposal for a

candidate is sketched out in this paper - it will be there for whoever wants to use it. Now, all too often, a false dichotomy is drawn.

Bevoegdverklaard Navorsers NFWO - Rijksuniversiteit Gent
Vrije Universiteit Brussel

* A first draft of this paper was presented at the Center for Philosophy of Science, University of Pittsburgh, October 1988 on invitation of Jerry Massey. This version has benefited from criticisms both from Jerry Massey, Ken Manders and the other fellows of the Center present at the moment. Especially Ken Manders' criticisms were important but, taken seriously (as they should), they constituted a new research program.

NOTES

- 1 An excellent overview of the literature is provided in Thomas Tymoczko (ed.), *New Directions in the Philosophy of Mathematics*, Birkhauser, Stuttgart, Boston, 1986. A few additional important works not mentioned are: David Bloor, *Knowledge and Social Imagery*, RKP, London, 1976; Sal Restivo, *The Social Relations of Physics, Mysticism, and Mathematics*, Reidel, Dordrecht, 1983 and Eric Livingston, *The Ethnomethodological Foundations of Mathematics*, RKP, London, 1986 (for a critical review of this work, see David Bloor, 'The Living Foundations of Mathematics', *Social Studies of Science*, vol.17, 2, 1987, pp.337-358. Also: Philip Kitcher (ed.), *Philosophie des Mathématiques-Philosophy of Mathematics*, Revue International de Philosophie, 4/1988, 167.
- 2 Errett Bishop, *Foundations of Constructive Analysis*, McGraw-Hill, New York, 1967, p.2.
- 3 Errett Bishop, 'Schizophrenia in Contemporary Mathematics', in: Murray Rosenblatt (ed.): *Errett Bishop: Reflections on Him and His Research*, AMS, Providence, Rhode Island, 1985, pp.1-32. See also Douglas Bridges and Fred Richman, *Varieties of Constructive Mathematics*, Cambridge UP, Cambridge, 1987 for an excellent and introductory discussion and presentation of principles such as LPO.
- 4 For the epistemic mathematics approach, see Stewart Shapiro (ed.), *Intensional Mathematics*, North-Holland, Amsterdam, 1985. Related articles are: Nicolas D. Goodman, 'The Knowing Mathematician', *Synthese*, 60, 1, 1984, pp.21-38;

- Stewart Shapiro, 'On the Notion of Effectiveness', *History and Philosophy of Logic*, 1, 1980, pp.209-230. A historically interesting contribution is Kurt Gödel, 'An Interpretation of the Intuitionistic Sentential Logic', in: Jaakko Hintikka (ed.), *The Philosophy of Mathematics*, Oxford UP, Oxford, 1969, pp.128-129.
- 5 Michael J. Beeson, *Foundations of Constructive Mathematics*, Springer, Heidelberg, 1985, esp. chapter III, pp.47-57.
 - 6 Classics in the field of epistemic logic are: Jaakko Hintikka, *Knowledge and the Known: Historical Perspectives in Epistemology*, Reidel, Dordrecht, 1974; by the same author, *The Intentions of Intentionality and Other New Models for Modalities*, Reidel, Dordrecht, 1975 and Karel Lambert (ed.), *The Logical Way of Doing Things*, Yale UP, New Haven, 1969.
 - 7 Ernst Welti, *Die Philosophie des strikten Finitismus. Entwicklungstheoretische und mathematische Untersuchungen über Unendlichkeitsbegriffe in Ideengeschichte und heutiger Mathematik*, Peter Lang, Bern, 1987. Also: Ernst Welti, 'The Philosophy of Strict Finitism', *Theoria*, II, 5-6, 1987, pp.575-582.
 - 8 For an overview of paraconsistent and dialectical logic, see Richard Routley and Graham Priest (eds.), *Essays on Paraconsistent Logic*, Philosophia Verlag, München, to appear. Also: Ayda I. Arruda, 'A Survey of Paraconsistent Logic', in: A.I. Arruda, R. Chuaqui and N.C.A. da Costa, *Mathematical Logic in Latin America*, North-Holland, Amsterdam, 1980, pp.1-41.
 - 9 Jean Paul Van Bendegem, 'Non-Formal Properties of Real Mathematical Proofs', in: Arthur Fine and Jarrett Leplin, *PSA 1988*, Volume One, PSA, East Lansing, pp.249-254.
 - 10 Besides the example of Euler's conjecture mentioned in the text, errors have occurred in Fermat's Last Theorem - see my 'Fermat's Last Theorem seen as an Exercise in Evolutionary Epistemology', in: Werner Callebaut and Rik Pinxten (eds.), *Evolutionary Epistemology*, Reidel, Dordrecht, pp.337-363 - in The Four-Colour Theorem - Ian Stewart in his *The Problems of Mathematics*, Oxford UP, Oxford, 1987, p.111 speaks of a 'comedy of errors' - and in the Goldbach conjecture, in the Riemann hypothesis, in the Bieberbach conjecture.
 - 11 Imre Lakatos, *Proofs and Refutations. The Logic of Mathematical Discovery*, edited by John Worrall and Elie Zahar, Cambridge UP, Cambridge, 1976, p.50.
 - 12 Jean Paul Van Bendegem, 'Dialogue Logic and Complexity', *Preprints*, 16, Ghent, 1985.

- 13 See Carl B. Boyer, *The History of the Calculus and its Conceptual Development*, Dover Books, New York, 1959 for historical details. Wallis' proof dates from 1656-57 and is to be found in his *Opera Mathematica* (see Carl B. Boyer, op. cit., pp.168-174).
- 14 It would be a mistake to believe that the practice of confining inconsistencies is typical for the mathematical period preceding the age of rigour. Dirac's delta-function is a similar, quite recent case.
- 15 See J. Versluys, *96 bewijzen voor het theorema van Pythagoras*, A. Versluys, Amsterdam, 1914. (*96 Proofs of Pythagoras' Theorem*).
- 16 'Fermat's Last Theorem seen as an Exercise in Evolutionary Epistemology', see note 10.
- 17 For an excellent overview, see Matthew L. Ginsberg (ed.), *Readings in Nonmonotonic Reasoning*, Morgan Kaufmann, Los Altos, 1987.
- 18 The first version is to be found in Alan Baker, *A Concise Introduction to the Theory of Numbers*, Cambridge UP, Cambridge, 1984, p.4.. The second version is taken from Daniel Shanks, *Solved and Unsolved Problems in Number Theory*, Chelsea Publ. Cy., New York, 1978 (second edition), pp.6-7. The corollary of Theorem 6 states that 'If a prime p divides a product of numbers, it must divide at least one of them.'
- 19 See Daniel Gorenstein, 'Classifying the Finite Simple Groups', *Bulletin of the AMS*, 14, 1986, pp.1-98.
- 20 Mark Steiner, *Mathematical Knowledge*, Cornell UP, Ithaca, London, 1975.
- 21 This short excursion into the Wittgensteinian field is not meant to take into account all the intricate details and complexities one finds in Crispin Wright, *Wittgenstein on the Foundations of Mathematics*, Duckworth, London, 1980 and Stuart G. Shanker, *Wittgenstein and the Turning-Point in the Philosophy of Mathematics*, Croom Helm, London, 1987.