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PRAGMATICS AND MATHEMATICS OR HOW DO MATHEMATICIANS TALK ?[1]

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> Investigations in the history of mathematics, which shed light on the processes by which mathematics grows and changes, investigations into "plausible reasoning" in mathematics are among the areas which invite study.
> Hilary Putnam in Putnam (1980).

0. To many the relation between pragmatics and mathematics is absolutely superfluous for isn't mathematics supposed to be a purely syntactical game where there is no room for semantics and certainly not for pragmatics. On the other hand many believe that mathematics cannot be fully understood unless a pragmatical component is introduced or, even more, unless pragmatics becomes the very basis on which to build the mathematical dome itself.

The aim of this article is twofold : one, show that indeed pragmatics is important in understanding mathematics or mathematical processes (and I apologize to those for whom this is a settled question), two, give some indications as to what elements are important in such a pragmatical account of mathematics.

1. The first problem we are facing is that of defining pragmatics : There are many (perhaps too many) definitions around : (the following list is to be found in Vandamme (1979))

(1) the study of the subjective relations of speaker and hearer related to a particular statement
(2) the study of relations between language and action
(3) the study of changes in an individual caused by speech acts

(4) the study of the relations between language used and its users
(5) study of the changes brought about by an individual in the language system.

Following Gochet in Gochet (1980), one can see that two elements are present in all the definitions :

(1') somebody uttering something in a certain language (including non-verbal language, etc.)
(2') the effect, or, more general, its relation to a context to be specified.

With this indeed very large definition in mind, let us look at the other problem we have to face : what is mathematics ? It certainly is no problem to produce as many, if not more, definitions of mathematics, as we did for pragmatics ! However, here I propose a specific definition as a sort of working definition (since for the purpose of this article it isn't necessary to have a sharp definition of mathematics) : mathematics is the union of all the different branches that can be found in the index of the "Zeitschrift für Mathematik und ihre Grenzgebiete". One can without danger state that this index is used everywhere in the mathematical community (it suffices to check the major mathematical journals, such as "The Bulletin of the American Mathematical Society", etc.). Using this definition, mathematics can be seen as a very large set of statements within domains as diverse as logic, foundations, set theory, algebra, topology, ...

The first part of our definition of pragmatics in the case of mathematics can be restated as (1") someone uttering a statement in a certain mathematical language. As to the second part of the definition, it is interesting to reflect for a moment about a mathematician's basic activity : proving theorems. In this case, a typical interaction between two mathematicians is something of the kind :

A : "I have just proved that p !"
B : "You did ? Show me".
A produces the proof of p
B : "Indeed you did, congratulations !"

Using this rough picture as a guide, (2') can be restated thus :

(2") the relation between a mathematical statement uttered and its ensuing defence in a dialogue.

Having this more specific definition at our disposal, we can face the basic question : is the domain to which the definition applies, trivial or not ? If it is, we might as well end here !

Let's see what the "syntactical" mathematician has to say about our definition. I assume (s)he will say the following :

(3) concerning (1''), that's easy : mathematics is the language par excellence that is context-free, so that point is settled
(4) concering (2''), that's easy too : A claims (s)he has a proof, produces it, -- following the well-known rules, such as modus ponens, substitution, etc. -- and B can check easily whether the proof is a good one or not. And that's all !

The reader might expect I don't agree with this point of view. In order to show this, it is necessary to have another look at the field of mathematics. If we go through the list of the various mathematical disciplines, one could make a distinction between (a) mathematics at the outside, and (b) mathematics inside. In (a) one would find some studies in the foundations of mathematics, some studies in logic, etc. A common element to this type of studies would be that it contains a large portion of some non-mathematical language. A typical example would be Abraham Robinson's "The Metaphysics of the calculus" (1967). This article contains as well a discussion on the history of the infinitesimal, as a rough sketch of his non-standard analysis. That (3) and (4) break down in (a) can be defended by the following argument. Since the bridge is made between formal and informal language, it is reasonable to suppose that the context becomes more important. One only has to look at the mass of literature on pragmatics to see that most languages under discussion are informal languages and that a pragmatical account of things is unavoidable. And the same goes for the argument that the rules governing the dialogues cease to be univoque.

If, however, one is not convinced, I think it is sufficient to look at a number of discussions in the foundations of mathematics over the past decades to see that indeed the situation is not so clear as the syntactical mathematician would like it to have[2]. Furthermore, some clear exemples can be produced :

(E1) if a mathematician says : "2 + 2 = 4", this statement expresses something completely different, whether the mathematician who said it, is an intuitionist, a constructivist, a finitist or a formalist.
(E2) if a mathematician says : "I know that this equation has a root", then the formalist would say : "Give me the proof", but the constructivist would say : "Give me a construction for the root."

Of course, one can always reply that (a) is not a part of mathematics proper, and thus the above argument, although correct, serves no purpose here. Instead of showing this view to be wrong — as I believe it is — I'll show that pragmatics is necessary in (b) as well, and (b) certainly is a part of mathematics ! In order to show this, some elements of complexity theory need to be introduced[3]. Consider a Turing machine or an equivalent machine. A problem for a Turing machine is a couple, consisting of an instance I and a question Q. A famous example — which will be discussed in more detail — is the Four Color Theorem (FCT). In this case I is a set of planar maps and Q asks for a procedure for colouring such a map such that no adjecent regions have the same colour. To solve a problem P, a machine T requires time and space. Time can be measured by the number of steps the machine has to execute and space can be measured by the amount of tape that is required to perform the calculations. These measures will of course be dependent on the length of the input, that is on the number of symbols occurring in I, say n. Thus we can write the required time and space as functions of n, time = f(n) and space = g(n). The problem complexity theory deals with, is to find out what f and g can be like. A first classification has been introduced : either f and g are polymonials in n, or they are exponential in n. If, f.i. time can be written as say $n^3 + n^2$ then it is a polynomial function and the problem is said to be solvable in polynomial time or the problem is a P-problem. If time is something like c^n, where c is some constant, then time is an exponential function and the problem is solvable in exponential time or the problem is a E-problem. It is important to realize that if a problem is solvable in polynomial time and space, however complex the polynomial might be, it would still require less space and time than a E-problem[4]. This distinction introduces a gap between problems : the class of E-problems contains no P-problems and vice versa. Besides the search for the structure for f and g, complexity theory looks for upper and lower bounds on problems. It can be shown that some problems require exponential time and space no matter what solution is proposed whether one is known or not ! This gives a lower bound to the problem. Conversely, it can be shown that some problems can never require more than a certain amount of space and time. This clearly provides an upper bound to the problem.

Within this context, it makes sense to split up (b) in two areas: (b1) polynomial theories and (b2) exponential theories. An element of (b1) contains problems of polynomial difficulty. Elementary algebra and geometry are to be found in (b1). An element of (b2) contains problems of exponential difficulty. The theory of inaccessible ordinals is an example. It is clear that the distinction on the level of theories is not so sharp as in the case of individual problems, in other words (b1) and (b2) can be seen as the extremes of a continuum : a theory close to (b1) would contain a larger part of P-problems and one close to (b2) would contain more E-problems. We have now reduced our task of showing that (3) and (4) break down in (b), to showing that it does in (b1) and in (b2).

Now, in the case of (b1) if one restricts oneself to finite combinatorial problems, I am (almost) willing to agree with the syntactical mathematician. If a mathematician claims (s)he has a proof for the statement that $1 + 2 + 3 + \ldots + n = n(n + 1)/2$ (n strictly finite), the statement itself is pretty clear and the proof can easily be checked by another mathematician. Whether he is a formalist, a constructivist, an intuitionist or a finitist is not really important, for if no agreement can be found about the proof, one can always fall back upon purely finite, combinatorial, step-by-step procedures. Unfortunately, this agreement is a meagre one, since (b1) does not constitute the interesting part of mathematics, in other words, most of mathematics takes place in (b2). Even in elementary number theory, infinities are to be found thereby making it impossible to fall back upon finite step-by-step procedures. If this is not convincing, it is sufficient to look at some mathematical literature to see that most open problems in mathematics today, are certainly not P-problems. Beautiful examples are the Riemann hypothesis, Fermat's last theorem, and FCT, although the last one is no longer an open problem.

What about (b2) ? Suppose A has found a proof of p, requiring exponential time and space. It is impossible for B to check the whole proof step-by-step since this would obviously require the same amount of time and space. B has to look for other methods that require polynomial time and space. These methods can never be absolutely safe, for if it were the case, it would be sufficient for any mathematician to write down an exponential proof and use the polynomial checking methods to see whether the proof is correct or not. But this would immediately solve the whole complexity business and this is obviously not the case. It seems very appropriate to label these methods heuristics. Suppose then that B has at his or hers disposal a list of heuristics H_1, H_2, ..., H_n. B will choose some H_i and check the proof. But why did B make this choice and not another. Obviously (s)he used some criterium or criteria. What can these criteria be like ? They can appeal to success : I have used this heuristic before in a situation of the same kind, so let me use it again. To practice : I am pretty good in working with this heuristic, so why not try it here. To one's view on the process of mathematical thinking : if I had made the proof, where could I have made mistakes. But one might go further, not only can (s)he appeal to this kind of criteria, (s)he simply has to for the simple reason that B is never sure of using the best heuristic around. If there would be such a thing as the best heuristic, this would turn the heuristic into an algorithm, and this is precisely what a heuristic is not ! This point will be taken up again in the following. Thus I believe one can conclude that some of the criteria are of a non-mathematical nature and so a pragmatical component is introduced thereby refuting (4) and to a lesser extent (3).

Having shown the first part of this article, we can now turn to the second part.

2. In the previous discussion we have already introduced some elements necessary for a pragmatical account of mathematics : there are two subjects A and B, equipped with certain computational abilities. Furthermore they must be able to discuss foundational problems and work with heuristics. How can we translate all this in one single frame-work. To tackle this question, it is important to look at previous attempts where a mathematical subject or subjects are introduced. We will briefly discuss the intuitionists, Alexander Esenin-Volpin and Paul Lorenzen.

The intuitionists were the first ones to introduce the mathematical subject, in other words, all mathematical processes are performed by someone, they are not just things that happen to float around. Their investigations into the nature of mathematical expressions led them to reject a great deal of them and with them, some rules of deduction that were believed to be evidently safe (f.i. tertium non datur)[5]. In terms of machines, this mathematical subject -- Brouwer called it the Creative Subject — was at the same time a very poor and a very powerful machine. Poor, because it was only equipped with an intuition about what numbers are, powerful because this intuition was sufficient for the machine to construct the whole of mathematics. Moreover this machine worked completely on its own, it didn't need other people around, it didn't need any information about the world in general. This is illustrated most dramatically by some statements of Brouwer himself[6]. This approach led to difficulties. There was a violent debate between Brouwer and the other intuitionists concerning the question how much the machine was able to construct. Brouwer believed it was able to construct something analogue to the cardinality of the real numbers while the others held the opposite view. The problem they faced was I think that the intuition alone was not sufficient to produce clear-cut limitations on the machine's constructions. More was needed, in other words, the machine had to be equipped with more abilities. When attempts were made to formalize the activities of the Creative Subject — as was done by Kreisel in Kreisel (1967) — Troelstra in Troelstra (1969) showed that a paradox followed. The basic activity of the machine was notated $\vdash_m A^{(m)}$, meaning "at moment m, the machine reaches conclusion A". Some of these conclusions will be about sequences of numbers and more specifically, about what intuitionists call lawlike sequences, i.e. sequences of numbers where there is a certain procedure to generate one number after the other. A Fibonacci sequence is a typical example. Suppose now we count all the conclusions of the type "$\vdash$ is a lawlike sequence" and we write $A[x]$ for "a_x is a lawlike sequence". A new sequence can be constructed : $b(x,y) = a_x(y)$, i.e. $b(x,y)$ is the yth element of the xth lawlike sequence. It is clear that b itself is a lawlike sequence since there is a procedure to generate the elements of b. But if b is lawlike, so is c, defined by $c(x) = b(x,x) + 1$. At some moment z, the machine will reach the conclusion that c is a lawlike sequence, thus for all x, $b(z,x) = c(x)$ which is equal to $b(x,x) + 1$. Since this goes for all x, it also holds for $x = z$ and we get : $b(z,z) = b(z,z) + 1$. Paradox ! The notion of the Creative Subject had to be changed but in what direction ? An obvious solution is to "open up" the machine. This approach was taken up by Alexander Esenin-Volpin in what he calls the ultra-intuitionistic or the anti-traditional program. He claimed that mathematics had to be strictly finite and that the mathematical subject is too restricted in its activities. His proposal as to how these are to be extended, is impressive. The full proposal is to be found in Esenin-Volpin (1970, p. 16). Among other things, the subject must be capable of perceptions thus providing a connection with the outside world, it must be able to accept and/or reject propositions. In contrast to Brouwer, other subjects are around that the subject can address, ask questions to and it can answer questions posed by them. The problem one faces here is that the subject becomes too real and gets difficult to handle. If Esenin-Volpin's proposal were to be translated say in terms of Turing machines, the machine must be able to calculate, perceive, understand language, etc. I do not deny that his approach is very valuable : in the end, I too believe that a theory of mathematical thinking and reasoning has to include all these elements and, even though a frame-work for expressing it all is not yet available, it is very important to defend this necessity. In the line of the intuitionists but more limited than Esenin-Volpin, we find Paul Lorenzen. He proposed to reformulate mathematics in terms of dialogues between an opponent and a proponent. In terms of machines, we now have two machines who are equipped with some procedures for attacking and defending a position. If f.i. the opponent sees a conjunction A & B, the procedure is to attack A and to attack B. A typical example is the following dialogue starting with the statement $-(a \,\&\, -a)$ put forward by the proponent :

	O		P	
(1)			$-(a \,\&\, -a)$	
(2)	$\not ?$	$a \,\&\, -a$		? 1
(3)		a		? 2
(4)		$-a$		a

In line (2) O attacks the negation by affirming what is negated or O can simply not question the statement indicated by ? ("non dubito"). In line (3) P attacks the left side of the conjunction a & $-a$. O answers by stating a. P continues by attacking the right side and O answers by stating $-a$. In line (4) P ends by stating a. O can ask P for a proof of a, but P replies that O has already accepted a before him, so O has to provide a proof. In this way P can always win the dialogue and so the statement $-(a \,\&\, -a)$ is true. A number of people — together they are now called "the Erlanger Schule" including Kuno Lorenz, Friedrich Kambartel, Oswald Schwemmer, Carl Friedrich Gethmann to name but a few — continued this line of research. One of their chief problems was to provide a justification for the rules of attack and defense : why are they just the way they are ? It soon turned out that a pragmatical account was necessary. Lorenzen's dialogues had to be embedded in a speech act theory. The following quotes are taken from Gethmann's book "Protologik". "Hat es die dialogische Logik nicht mit Propositionen, sondern mit vollständigen Sprechhandlungen zu tun, dann sind die Dialogregeln, die die Bedeutung der logischen Operatoren festlegen, als Reglementierungen für Sukzessionen von Sprechhandlungen zu verstehen." (p. 55). The first part of this statement is grounded by an analysis of the symbol "?". The question-mark can mean different things : "Zweifel", "Angriff", "Bestreiten", "die Rolle des Opponenten übernehmen", "Auffordern, zu begründen". (p. 50). Each of these meanings produces a different reaction from the other party in the dialogue. What one has to do is "die dialogische Semantik der logischen Operatoren in den weiteren Zusammenhang einer systematischen Konstruktion und Rekonstruktion argumentationsrelevanter Sprechhandlungen einzubetten". (p. 53). I don't need to mention that this approach is very close to mine, and when Gethmann remarks on p. 152 "Pragmatisch gravierender ist jedoch, dass die Mengen, mit denen es umgangssprachliches Argumentieren zu tun hat, zwer meistens finit sind, aber pragmatisch unübsersehbar", I cannot help but to see a connection with my complexity approach. An exponential proof can be seen as a proof where it is no longer possible to get an "overview".

I must add here that I have taken Gethmann as an examplary case, similar remarks and ideas can be found in the work of Kambartel, Thiel, Hegselmann, Mainzer, etc. I refer the reader to Gethmann (1980) for an excellent overview.

Is the reader to believe that my problem has been solved ? Yes and no. Yes, because several models and formal theories have been advanced by members of the Erlanger Schule to deal with this pragmatic view, no, because either these theories are too large — they include f.i. all possible speech acts — or they are not but then the problem arises what criteria are to be used to state that this or that part of pragmatics has to be included and some other part not. The remedy to this problem is to be found in empirical material, in other words let us look for specific dialogues in mathematics and analyze what elements are important for a description of these dialogues. This view certainly isn't a new one. It has been brought forward by Martin in Martin (1979) when he was discussing Montague's system : "Another disconcerting feature of Montague's work is its almost total disregard of the painstaking empirical and theoretical data that linguists have amassed over the years. He writes as though there were no such subjects as empirical or descriptive linguistics,..." (p. 171). I certainly do not argue that the one approach has to be dropped for the other, quite on the contrary. I believe that the mutual interaction between theoretical models and empirical cases is a very fruitful one promising to solve a number of never ending discussions. Moreover since we are dealing here with a very specific language, namely mathematics, this facilitates to a certain extent the choice of empirical material. In the last part of this paper I will examine two such cases, one in field (a) one in field (b2). I must mention here that this kind of approach is new in the field of pragmatics related to mathematics and that therefore my humble attempt to do it is only a first step. I do not offer the reader a procedure of how to select the relevant data obtained in an empirical study, but rather I hope to show how it can be done in two specific cases. Whenever possible, reference will be made as to how the data can be inserted in a Lorenzen-like frame-work.

The example in field (a) is the discussion between David Hilbert and Gottlob Frege. The material consists of a set of nine letters, written between 1895 and 1903. The topics discussed concern problems of foundations of geometry raised by Hilbert's essay on this subject. All the quotations used are in English. These are not translations done by me, I used the English translation of the correspondence as it can be found in McGuinness (1980). In case of doubt, the original letters were consulted in Gabriel (1976). Interesting features in this correspondence are, one, the fact that Frege and Hilbert belonged to different schools (Frege is a well-known logicist and Hilbert an equally well-known formalist), two, the fact, that the result of the discussion was negative. This is not just a side-remark. Trying to find out why a discussion failed can be as important, if not more, as finding the rules used[7] in a correct discussion. It is worth noting that concerning the failure of the discussion between Frege and Hilbert, some "theories" exist already : most of these claim that Frege misunderstood Hilbert's ideas, the idea being, that Frege was not capable of following the "new" ideas. There is however a notable exception namely Friedrich Kambartel[8], who claims that Frege correctly criticized Hilbert for his loose use of certain terms. I hope to show that part of the misunderstanding between Frege and Hilbert also had to do with the non-correct use of certain rules, thus showing the importance of a pragmatical analysis.

Although it may appear trivial, I insist on remarking that my treatment of these letters takes place in an interpretational framework that is my own. I don't claim to give the interpretation of the letters, just a plausible one. If anybody can produce a more acceptable one, I will gladly accept it !

The first two letters (Frege to Hilbert, Jena, 1 October 1895 and Hilbert to Frege, Göttingen, 4 October 1895) concern the problem of why formalize. Not much is to be learnt from these, since the second letter contains only a short and rather cryptic reply from Hilbert. Letters 3 and 4 (Frege to Hilbert, Jena, 27 December 1899, Hilbert to Frege, Göttingen, 29 December 1899) are the most interesting ones. Frege has read Hilbert's "Grundlagen der Geometrie" and writes to Hilbert his reactions, to which Hilbert replies. The fifth letter (Frege to Hilbert, Jena, 6 January 1900) is Frege's comment on Hilbert's letter, but unfortunately Hilbert replies he has no time. We will only refer to letter 5, inasfar as the analysis of 3 and 4 makes it necessary.

The first topic discussed is about the distinction between axioms and definitions. Frege states the following :

(F1) a definition contains a sign that had no meaning before and gets its meaning by that definition

(F2) axioms are built up of signs and words whose meaning must be clear and must already be laid down by some definition

(F3) axioms express fundamental facts of intuition

(F4) confusing the two are responsible "that complete anarchy and subjective caprice now prevail" (p. 36)[9].

Hilbert's position is very clear : the characteristic marks of the definition are exactly the axioms. The misunderstanding — the word used by Hilbert in the letter - has to do with the fact "that I do not want to assume anything as known in advance" (p. 39) and he adds — and here he uses the same argument Frege used – "one is looking for something one can never find because there is nothing there, and everything ... degenerates into a game of hide-and-seek" (p. 39). This short discussion contains the following arguments : Frege claims that "first you have definitions, then axioms" and if this is not the case, then "anarchy !". Hilbert turns things around. We can easily represent this in a dialogue-scheme using the following abbreviations : "!p" stands for "I say p is the case" "C" will stand for Frege's claim.

(D1)

	F	H
(1)	C	$! \neg C$
(2)	$\neg\ C \supset$ anarchy.	$!\ (\ C \supset$ anarchy)

If is probably not difficult to see that this type of dialogue cannot end. In the best of cases, after n moves, one ends up with a stack of unproved claims. Nevertheless, it is fascinating to ask whether the "!"-reply is as safe a protection as it looks. Frege's second argument is an example of a counter-attack "that doesn't work". It can be formulated thus : if the opponent claims p, derive q from p, so that he must also claim q and let q be such that the opponent will not claim it. but what does the opponent do ? He repeats the same move on the proponent. Perhaps a good name for this type of dialogue would be a "mirror-dialogue".

The discussion is continued and Hilbert (in letter 4) produces the following reasoning :
(H1) you say the definition is contained in the explaination
(H2) the definition is formed by the axioms
(H3) thus you say that the axioms are contained in the explanation
(H4) you can say this, but it is in contrast with costumary practice.
The answer – in letter 5 – proceeds thus :
(F5) the construction of an example in "Hilbert's style"[10]
(F6) showing that his definition of definition doesn't fit in.
The most amazing feature of the above reasoning seems to me the fact that the word "definition" is used with different meanings. In (H1) Hilbert speaks about definition in Frege's sense, whereas in (H2) he speaks about it in his own sense. But then of course, (H3) cannot follow from (H1) and (H2). Remarkably enough, Frege does the same thing, by using his definition in order to show that it cannot be applied to an example in Hilbert's system. Again we can try to represent the discussion in a dialogue with the aid of the following abbreviations :

Def(F), Def(H)	: definition according to Frege, idem to Hilbert
Cont(x,y)	: x is contained in y
Form(x,y)	: x is formed by y
Ex(x,y)	: x is an example of y
Appl(x,y)	: x is applied on y
WR	: you are wrong

In contrast to Lorenzen's dialogues, we allow the players to make a sequence of statements before the other can reply.

(D2)	H	F
(1)	Cont(Def(F), Expl)	
(2)	Form(Def(H),Axioms)	
(3)	$(\text{Cont(Def(F),Expl) \& Form(Def(H),Axioms)}) \supset \text{Cont(Axioms,Expl)}$	
(4)	Cont(Axioms,Expl)	
(5)	$\text{Cont(Axioms,Expl)} \supset \text{WR}$	
(6)	WR	
(7)		Ex(x,Def(H))
(8)		Appl(Def(F),x)
(9)		$\text{Appl(Def(F),x)} \supset \text{WR}$
(10)		WR

The fallacy committed by Hilbert and Frege can be seen clearly now.

(4) is a correct application of modus ponens on (1), (2) and (3) if Def(F) = Def(H), which isn't the case and without (4), conclusion (6) isn't possible. The same goes for Frege's reasoning. As in the preceding case, again we have reached a situation in the dialogue, that offers no prospect for continuation.

The second topic discussed is truth. Nothing new happens here : exactly the same type of arguments are used again. The remaining letters (Frege to Hilbert, Jena, 16 September 1900, Hilbert to Frege, Göttingen, 22 September 1900 and Hilbert to Frege, Göttingen, 7 November 1903) show clear indications that both thinkers started to question the discussion itself :

(F7) "... that the area of friction between our opinions is already large enough" (p. 49)

(F8) "I believe, I can deduce ... that my arguments failed to convince you" (p. 49)

(H5) "I did not think up this view because I had nothing better to do, but I found myself forced into it ..." (p. 51)

This dialogue takes place on the meta-level and discusses the conditions for a dialogue to happen. We will refer to this dialogue as (D3).

What can we learn from this investigation ? It is obvious that the classical dialogues will have to be extended in two directions : first, the players must have more possibilities concerning attacking and defending – they must be able to claim things as in (D1)(1) where H says "!C", they must be able not just to make one statement but a sequence of statements before the other can reply as in (D2), etc. – secondly, their language must be large enough so that they can speak about definitions, axioms and the like. More interesting is the fact that the extended dialogues will have to include predicates dependent on a context. In (D2), the predicate Def is dependent on F or H. There seems to be a very strong similarity between these context-dependent predicates and Montague's indexical expressions. F and H can be seen as indices or point of reference for Def. It is worth noting that the mixing of these indices is responsible for the fallacy committed by Frege and Hilbert. Equally interesting, is (D3). The extended dialogues will have to deal with the conditions for a dialogue to take place. This reminds one immediately of Grice's Cooperation Principle : "Make you conversational contribution such as is required, at the stage at which it occurs, by the accepted purpose or direction of the talk exchange in which you are engaged". It is clear that Frege and Hilbert bluntly violated this principle ! Grice's principle is not without problems, as Asa Kasher in Kasher (1976) has pointed out. He proposes to replace the Cooperation Principle by the Rationalization Principle : "There is no reason to assume that the speaker is not a reational agent; his ends and his beliefs regarding his state, in the context of utterance supply the justification of his behavior." (p.210). Seen from this angle, it may indeed have been very rational of Frege and Hilbert to end the discussion seeing the amount of time and energy they would need to convince one another. This short digression shows how important it is to include in the extended dialogues conditions that state what a dialogue is, when it is taking place and how the participants are behaving.

It has been pointed out to me quite rightly that all the ideas expressed above could apply equally well to any sort of linguistic interaction and that therefore there is no need for an independent domain of pragmatics in mathematics. I agree completely but since I also believe that this thesis is far from commonly accepted, I felt the necessity through this one example to show that these elements are indeed unavoidable. Moreover I believe having shown that empirical material has the advantage of investigating faulty dialogues, such as (D2). Much, if not more, can be learnt from this type of interaction that one rarely meets in theoretical investigations. On the other hand, if one looks at (b2) I think the dialogues present some features that are proper to mathematics. This will be examined by looking at the famous Four Colour Theorem. The theorem states it is possible to colour any planar map with four colours such that no two adjacent regions have the same colour (if two regions touch at a corner, they are not called adjacent). This problem has been around for quite some time, has been "proved" many times, but each time an error was found. Recently the theorem has been proven again by Appel, Haken and Koch in 1977. This time the proof seems to be final. However some mathematicians were not pleased with it. The proof consisted of two parts : first, the authors showed how to reduce the set of all maps to a large but finite set of maps; if these could be coloured with four colours only, then so could they all be, two, this set of maps was coloured by a computer and so to speak, the final word was to the computer, who said "yes". In terms of our terminology introduced before, the proof is an exponential one so we are well within (b2). It must be added that there is no evidence for a lower bound, the possibility of a polynomial proof cannot be excluded. In 1979 Thomas Tymoczko published a paper entitled "The Four-Color Problem and Its Philosophical Significance" in which the following thesis was defended :

(T1) the proof of a theorem must be surveyable
(T2) the proof of FCT is not surveyable because a computer has been used the program of which can never be checked by a human because this would require much to much time
(T3) one is forced to accept one of the following conclusions : FCT is not a theorem because the proof is not a correct proof or we must change our notion of theorem.

By change Tymoczko meant that we have to allow a proof to contain what he calls empirical evidence, such as a computer performing calculations.

Michael Detlefsen and Mark Luker replied to this paper in "The Four-Color Theorem and Mathematical Proof". Their claim was even stronger : Tymoczko was wrong in believing that FCT was the first theorem using for its proof empirical considerations, nearly all of mathematics uses empirical evidence in its proofs. Their thesis is based on the idea that whenever a proof has some calculations in it - and in practice they all do - there are four assumptions required for confidence in the result of those calculations :
(A1) the underlying algorithm to be used is mathematically sound
(A2) the particular program to be used is a correct implementation of this algorithm
(A3) the computing agent correctly executes the program
(A4) the reported result was actually obtained. (p. 808).

(A1) and (A2) can be checked in principle, but the belief in the validity of (A3) and (A4) is a matter of empirical considerations. A digression is necessary here. The reader may have remarked that the assumptions (A1)-(A4) introduce a pragmatical element in what I have called (b1) in my division of the mathematical field. But there I have stated that I (almost) agreed with the syntactical mathematician. The reason I added the "almost" between brackets, is precisely because I agree with the above assumptions (A1)-(A4). The reason for focusing on (b2) was to make my claim stronger : even if I grant you "context-freeness" in (b1), you still will have to deal with it in (b2). Even if someone believed (s)he had refuted (A3) and (A4), (s)he would still have to refute my claim. I hope this explains why complexity theory plays such a vital role because it raises extra problems besides the ones introduced by (A3)-(A4). If however one believes that the pragmatical element must be stressed in (b1) and (b2) then I must refer to two articles by Hilary Putnam, "Analyticity and Apriority" and "Truth and Necessity in Mathematics". I would do injustice to Putnam if I tried to summarize his ideas in a few lines. Let me just state that the core idea is that even when something is mathematically impossible, it isn't epistemically impossible for the simple reason that although I can know of a certain mathematical theory that it is consistent, I can still know what it means that a contradiction could be derived in it and what I should do if such a contradiction were ever found. This destroys the full certainty a proof is supposed to give. Going into all the details of Putnam's reasoning requires a separate article and as I mentioned above, my aim is in proving the stronger claim.

Going back to Detlefsen and Luker, it is interesting to see that their suggestions and proposals as to how we can know a proof is correct, are of a heuristical nature, referring to elements outside of mathematics. One heuristic is of a sociological nature : let as many mathematicians as possible check the proof or to quote Daniel Gorenstein, a group theorist : "However, there is a prevalent feeling that, with so many individuals working on simple groups over the past 15 years, and often with such different perspectives, every significant configuration will loom into view sufficiently often and so cannot remain unnoticed for long. On the other hand, it clearly indicates the strong need for continual reexamination of the existing "proofs"." (p. 812 in Detlefsen and Luker). Another heuristic is of a probabilistic nature : reject the proof if it is a long and complex one and look for probabilistic evidence. This heuristic is pragmatical in a two-fold way : it refers to the abilities of the mathematician to construct probabilistic evidence — this again referring to practice and training — and it relies on the mathematician's view on probability, i.e. will he accept a proof that is 99 % sure, or 90 % or even 60 % ? The authors mention a beautiful example about prime numbers. Say you try to find out whether a number n is prime or composite. There exists a simple algorithm that giving a number b smaller than n, checks if b is a witness or not. If b turns out to be a witness, then n is composite. Rabin showed that if n is composite, at least half of the integers between 1 and n will be witnesses to the compositeness of n. So, if you select k numbers at random, and they all fail to be witnesses, then the probability of n being composite, is less then $(1/2)^k$. And the authors add "Indeed, some mathematicians (e.g., Ronald Graham of Bell Laboratories) have said that they have more confidence in results achieved via Rabin's techniques than they have in results achieved by long and complicated traditional proofs". (p. 818). Another beautiful example is to be found in De Millo (1980) that could be labelled the exchange heuristic : "Recently, two independent groups of topologists, one American, the other Japanese, independently, announced results concerning the same kind of topological object, a thing called a homotopy group. The results turned out to be contradictory, and since both proofs involved complex symbolic and numerical calculation, it was not at all evident who had goofed. But the stakes were sufficiently high to justify pressing the issue, so the Japanese and American proofs were exchanged. Obviously, each group was highly motivated to discover an error in the other's proof; obviously, one proof or the other was incorrect. But neither the Japanese nor the American proof could be discredited."

Many other heuristics can be thought of, such as (1) select relevant subparts of the proof and check these in detail, (2) find another proof, (3) try to derive a contradiction from the theorem proved, etc.[11]

Turning back now to our original problem, namely what can be learnt from these interactions between mathematicians, it is more than obvious that the extended dialogues will include the possibility for the participants to appeal to heuristics. We call a dialogue classical if it is of the following type :

(CD)	O		P	
(1)			I claim p	
(2)	I attack p			
(3)			p stands up to the attack	p doesn't stand up to the attack
(4)	p is accepted	p is rejected		

then I would suggest to replace it by the following dialogue :

(ED)	O		P	
(1)			I claim p and I have a proof	
(2)	I pick a heuristic H_i to check the proof			
(3)			an error is found	no error is found
(4)	the proof is corrected by some method	the proof is accepted		

Several remarks have to be made :

(a) the classical dialogues are easily seen to be a subclass of the extended dialogues : in the former ones, only one heuristic is used, the attack heuristic that leads to acceptance or rejection

(b) the method used to correct the proof can lead to different results: the proof can be so changed that p can still be claimed. Suppose someone constructs a proof using transfinite induction and someone else finds an error in the use thereof; then (s)he could still reply that the use of induction wasn't necessary and replace it by another proof procedure and thus (s)he would still be able to claim p. If the elimination of the error leads to a proof not of p but of some statement p', then I would still be able to claim that p might be the case, I just haven't found a good proof. It seems very suggestive that the discussion we had about heuristics can equally be applied to the methods we use to correct a proof. A beautiful example of such a method can be found in Imre Lakatos' "Proofs and Refutations". In the pattern that he proposed for mathematical discovery, one proceeds by constructing counterexamples that refute the proof, the part of the proof that is "guilty" for the emergence of these counterexamples "is made explicit, and built into the primitive conjecture as a condition. The theorem — the improved conjecture - supersedes the primitive conjecture with the new proof-generated concept as its paramount new feature" (p. 127).

(c) there is a distinct difference between "the proof is accepted" in (CD) and in (ED) : in (CD) it is final, no more need to be said about p, in (ED) however there can only be something of a degree of acceptance. In this case it may be important to consider the history of the proof : has it been challenged before and did it survive the attacks. It seems necessary to include a logic of acceptance in the dialogues to deal with this.

(d) in contrast with (CD), two dialogues in (ED)-style can be considered different if they differ only in who O is, in other words, replacing the opponent O by some other opponent O' becomes very important. This simply means that a proof can become more accepted if it is checked by more mathematicians even if they do so in the very same way, i.e. use the same heuristic.

I hope the reader will agree that a lot can be learnt from empirical material as we have shown in these few pages. Time has come to reach some conclusions.

3. First, it has been shown that pragmatics is important if not necessary in understanding mathematical processes. The argumentation relied heavily on complexity theory, a mathematical theory about the length in time and space of computations. Secondly, agree-

ing with Putnam that mathematics is to be characterized as a style of reasoning we claim that there at least two different styles depending on whether the reasoning takes place in the foundations of mathematics or in mathematics proper. Thirdly, through the investigation of some empirical material, we have made some suggestions as to how a Lorenzen-type of dialogue can be extended to include these types of reasoning. It turned out that in one of the styles of reasoning (in mathematics proper) heuristics play a vital role

Jean Paul Van Bendegem
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NOTES

[1] I am most grateful to Leo Apostel and Asa Kasher for their constructive criticism that allowed me to improve my ideas on the relation between mathematics and pragmatics. Comments, suggestions and ideas have been made by Fernand Vandamme and Diderik Batens to whom I express my thanks.

[2] The introduction of Heyting (1971) is an imaginary discussion between an intuitionist, a formalist, etc. It shows clearly the differences in point of view.

[3] For an excellent introduction I can refer the reader to Garey and Johnson (1979).

[4] This can be illustrated by some numbers. Suppose one step executed on a "real" Turing machine takes one millionth of a second. The table shows how long it takes to perform a calculation on an input of resp. 30 and 40 symbols, in the case time is a polynomial function, i.e. time = n^2 and in the case time is an exponential function, i.e. time = 3^n

time \ input length	30	40
n^2	0.0009 sec	0.0016 sec
3^n	6.5 years	3855 centuries

[5] They didn't accept the statement $p \vee -p$. It amounts to saying that you have either a proof of p or a proof of $-p$. Take for p the state-

ment that there is a sequence of nine 7's in the number . This statement can neither be proved nor disproved.

[6]The following quote is from Brouwer (1948) : "Intuitionistic mathematics is inner architecture and research into the foundations of mathematics is inner inquiry with revealing and liberating consequences, also in non-mathematical domains of thought."

[7]I have been criticized for making a rather loose use of the expressing "using a rule". I agree completely. I do not wish to settle here the hard problem of finding out when x is using a rule p. I have choosen a certain frame-work — Lorenzen's dialogues — mainly for its simplicity.

[8]In Kambartel (1968).

[9]The page numbers refer to the English edition.

[10]The example is the following : Frege asks himself how the axioms:
1. Any number is congruent to itself according to any modulus
2. If a is congruent to be and b to c in the same modulus then a is congruent to c in that same modulus
allow him to know that $2 \equiv 8$ (mod3).

[11] A short discussion in the Mathematical Intelligencer (vol. 2, no 1, 1979) must be mentioned : it contains an informal article by Yu. I. Manin on "How convincing is a proof ?" followed by contributions of Neumann and Feferman. Manin mentions in his contribution three aspects to be considered when judging a proof :
1. reliability of principles (cfr. heuristics)
2. levels of "proofness" (cfr. complexity theory).
3. errors.

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