Proofs instead of Meaning Explanations:
Understanding Classical vs. Intuitionistic Mathematics from the Outside*

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1 Introduction

The conflict between classical and intuitionistic mathematics – henceforth, the *C-I conflict* – has been discussed at length and in depth by a number of famous scholars. Why an outside perspective? Is such a perspective interesting, or even possible?

There are in fact reasons why a somewhat detached account of this conflict might be worthwhile. First, the conflict is *prima facie* very puzzling, and even worrying. Mathematics is a discipline on which much of science, indeed much of our knowledge, rests. Moreover, it is a discipline whose practitioners are supposed to agree among each other more than in other fields about results and methods. Yet here there appears to be a conflict even about basic laws of logic, not to mention specific mathematical claims.

Second, popular accounts of this state of affairs are not very satisfactory. One may be told that the part of mathematics that matters for *practical applications* is unaffected by the C-I conflict. But that leaves the original question even more puzzling: how then can there be a conflict about basic logic? Another idea is that classical and intuitionistic mathematicians simply speak different languages, and only *seem* to contradict each other. There is something to this, of course. But again, if that were the whole explanation, why would the conflict persist?

Specialists in the field haven't paid much attention to explaining what goes on to a wider audience. That's unfortunate, especially since the 'received' view of the matter has undergone significant changes since the days of Brouwer. For example, it is now quite common for intuitionists to see classical mathematics as

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a *special case* of intuitionistic mathematics. That would have been unthinkable to Brouwer.[1]

But also from the point of view of general epistemology or philosophy of science, this conflict ought to be an ideal object of study. One would be hard put to find other cases of such clear-cut and continued disagreement, about truth and about methods, in the sciences. Discussions on the subject in the philosophy of mathematics abound, but they usually reflect the philosophical aspects of one or the other position in the conflict. What I am after here is the more detached view of the philosopher of science.[2]

For example, it might seem, *prima facie*, that the C-I conflict is a promising case for those who sustain some form of *relativism* about knowledge or truth. Here we have two communities of mathematicians who clearly disagree, but whose disagreement is not easily resolvable by giving one side an advantage over the other. Perhaps *both* are right; perhaps the disagreement is 'faultless'? I am not saying that this is actually the case, but the question could surely be raised, as it recently has been raised for discourse about other things than numbers or functions (e.g. discourse about taste, values, probabilities, knowledge, the future, etc.[3]).

Such an undertaking would not only benefit from an outside perspective, but require one. But is such a perspective really possible?[4] Won't one inevitably be influenced by one's own preferences? Surely there is a such a risk, but it shouldn't make us give up before trying. Being aware of the problem, one can try to avoid falling into the most obvious traps. And if in the end the difficulties become unsurmountable, that too would be a useful insight.

There is, however, a theoretical objection coming from the intuitionist camp, for example in Michael Dummett's version. It stems from the claim that classical mathematics, and more generally the classical notion of truth, is simply incoherent, and therefore ultimately *unintelligible*. Mustn't the lack of intelligibility transfer to any attempt at an 'objective' account of the conflict? This is a serious question. But things are not simple: the unintelligibility claim is not shared by all intuitionists, and it may even be in conflict with some other things Dummett says on this subject — Dummett is in fact one of the (few) champions of promoting *mutual understanding* between classical and intuitionistic mathematicians.

A further worry is that an outside view of the C-I conflict will be super-

[1] The inverse view is also common, that intuitionistic mathematics is just a particular kind of (classical) mathematics. Appearances notwithstanding, these two views are not incompatible; see section 5.2.

[2] [Hellman 1989] focuses on the issue of mutual understanding between the two camps, as I do in this paper. His perspective is that of classical mathematics, however, and a main claim is that the intuitionist cannot state her position clearly without resorting to classical logic. Although that issue is both interesting and relevant, I avoid it here.

[3] See, for example, [MacFarlane 2005] and references therein.

[4] Traditional relativism denies that this is possible. But the modern versions of relativism referred to in the previous footnote might very well allow it. Relativism applies, it is claimed, to certain discourses, not to all. One could be allowed to be relativist about statements of taste, say, but absolutist about semantics, in particular about the meaning of taste statements.

ficial. Mathematicians are usually (and often justifiably) suspicious of how non-specialists describe what they are up to. They feel that the mathematics should speak for itself. But that would mean that non-specialists should give up any attempt to understand what the conflict is about. And this might even be reasonable if the debate were about number theory, or topology, say. But the debate is (also) about what mathematics *is*. Then it is not enough to just *point* to existing mathematics, especially when the different camps point to different kinds of mathematics.

The structure of the paper is as follows. After some stage-setting in sections 2 and 3, I start with a rather close look in section 4 at the suggestion, mainly due to Dummett, that classical and intuitionist mathematicians should try to achieve mutual understanding by starting from a *common ground*, which is unproblematic in some important sense. My evaluation of this strategy is mostly negative: a basic *asymmetry* as to one side's ability to achieve understanding of what the other is up to will remain. In section 5, I then explore another approach: focusing on *proofs* rather than meaning explanations, and taking account of the avowed intention of most modern intuitionists to make all intuitionistic theorems classical theorems as well, appears to significantly improve the prospects of mutual understanding. Although this indeed promises to eliminate serious *conflict* between the two camps, I make some cautionary remarks at the end of that section, as well as in the concluding section 6.

2 Background

Although there are many variants of intuitionistic as well as classical mathematics, for certain basic issues these differences do not matter much. It is often enough to simply speak (as Dummett does) of *intuitionists* and *platonists*. The principled differences between these two concern the notion of mathematical *truth* and the meaning of the basic *logical constants*. The typical intuitionist takes truth to be what philosophers call an *epistemic* notion: roughly, something is true if it can be *proved*. This puts *computation* at center stage: (intuitionistic) proofs are computations, or directions for finding computations. Accordingly, the meaning of the constants are given as *proof conditions*: some form of the Brouwer-Heyting-Kolmogorov (BHK) conditions for the circumstances under which a statement of a certain form can be asserted.

The typical platonist disagrees. Truth is *not* epistemic: whether something *is* true is unrelated to our ability to *find out* if that is so. A statement for which we will in fact never find a proof might still be true. The meaning of the constants are in terms of the usual (Tarskian) truth conditions. It is a little harder to state the 'typical' platonist way of explaining *why* truth is non-epistemic, but the rough idea is that mathematical statements are *about* some reality or structure suitably independent of us. We need not assume he holds this for all of mathematics; for most of what I say it will be enough to consider *first-order number theory*, *PA*: for any sentence in *PA*, the platonist holds that it is either true or false, regardless of what we know now or will ever know. A *platonist about real numbers* holds the same for statements (in some language that needs to be specified) about the reals, but that doesn't commit him, as these terms are used here, to the same view about the whole of set theory, for example.

It may seem that an obvious weak point for the platonist is the reference to an independent (platonic) reality of abstract objects. The only abstract objects the intuitionist needs are the proofs themselves. But here the platonist counters that this is just appearance: by defining truth as provability you lose the ability to explain the *point* of proofs, which, non-trivially, is precisely to get at the truth. The intuitionist responds that the point is in fact another, having to do with *computability*. And the familiar (philosophical) debate continues. But to begin, at least, we shall ignore the 'why' of proving things, as well as the existence of platonic realities: it is enough to assume that the two parties have the different attitudes towards number-theoretic *statements* indicated above.

3 Setting the stage

A quick glance from the 'outside' seems to indicate that the C-I conflict is very serious indeed. Intuitionists refuse to assert things that platonists find trivially true, and in other cases assert things that classical mathematics outright denies. An example of the former is of course the Law of Excluded Middle, say in the form that for any PA-sentence φ,

(LEM) $\varphi \vee \neg\varphi$

is (logically) true. The intuitionist doesn't *deny* LEM (which to her would mean claiming that it is contradictory), but she certainly doesn't believe we have any reason to assert it. The second kind of conflict is exemplified by Brouwer's theorem

(CONT) Every function from [0,1] to the real line $\mathbb{R}$ interval is uniformly continuous,

something that every math student learns how to *disprove* at an early stage.

But a common explanation is that only the *words* are the same; the *statements* made are different. The intuitionist means something quite different with words like "or", "not", "real number", etc.[5] This eliminates the immediate threat of conflict. Moreover, it seems that *provided* the words are used in the intuitionist way, the platonist too can accept that LEM fails, and perhaps even that CONT holds.[6]

However, the problem doesn't go away so easily:

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[5] Thus, for example, [Bridges 1998] (p. 55): "The apparent absurdity of this statement is, however, illusory, as is suggested by the following more careful re-statement of it.

> *Every intuitionistically definable function from the intuitionistic interval* $[0, 1]$ *to the intuitionistic real line is, intuitionistically, uniformly continuous.*"

[6] For example, by following the exposition of Brouwer's theory of choice sequences in [Troelstra and van Dalen 1988], which takes place in a classical framework.

- Can the respective meaning explanations be provided in a sufficiently clear way, so that *mutual understanding* is achieved?
- Assuming this can be done, and allowing for the meaning differences, can we be sure that no *other* conflicts than those alluded to above, of the stronger or the weaker kind, exist?
- For example, can we be sure, for some principled reason, that the platonist will accept *all* 'translated' intuitionistic claims?
- Even if that were the case, what shall we do with the fact that the converse seems to fail? Intuitionists do *not* accept the classical version of LEM or of the negation of CONT. They might (nowadays) agree that these claims are *consistent*, but they would not assert them, whereas the platonist is happy to admit, for example, that the *intuitionistic version* of LEM fails.
- Thus there seems to be an *asymmetry* as regards mutual understanding, and one would like to know why.

To the specialist, these questions may seem trivial, or misguided. But I will proceed on the assumption that, at least initially, they make sense.

4 Mutual understanding

The intuitionism of Brouwer and Heyting was often presented in rather polemical form. Michael Dummett, however, is a latter-day defender of Brouwer style intuitionism who, in addition to finding support for it from a Wittgenstein-inspired account of how language works, has repeatedly stressed the need for *dialogue* between platonists and intuitionists:

> ...the desire to express the conditions for the intuitionistic truth of a mathematical statement in terms which do not presuppose an understanding of the intuitionistic logical constants as used within mathematical statements is entirely licit. Indeed, if it were impossible to do so, intuitionists would have no way of conveying to platonist mathematicians what it was that they were about: we should have a situation quite different from that which in fact obtains, namely one in which some people found it natural to extend basic computational mathematics in a classical direction, and others found it natural to extend it in an intuitionistic direction, and neither could gain a glimmering of what the other was at. That we are not in this situation is because intuitionists and platonists can find a common ground, namely statements, both mathematical and non-mathematical, which are, in the view of both, decidable and about whose meaning there is therefore no serious dispute and which both sides agree obey a classical logic. ([Dummett 1973]: 237-8)

The quote also indicates one road along which Dummett thought mutual understanding could proceed: via the common ground of decidable sentences.

4.1 Decidable sentences as a common ground?

The basic idea seems to be that decidable sentences are *unproblematic*, and therefore mutual understanding can begin with them.

We can avoid any discussion about exactly what *decidable* means here, as follows. First, restrict attention to the language of *PA*. The great advantage of this is that we can assume, without distorting things very much, that

(1) There is no conflict about the meaning of the arithmetical non-logical constants, and therefore no conflict about *atomic* sentences.

In contrast with the case of analysis, the conflict concerns only the logical vocabulary in this case. Now, let D be the set of *PA*-formulas with only *bounded quantification*.[7] Even if D is only a subset of the set of sentences Dummett has in mind, there is no unclarity about the fact that all sentences in D are decidable.

Now, in what sense are sentences in D a *common ground* for the platonist and the intuitionist?

At first sight, it might seem that Dummett holds that these sentences express "basic computational mathematics" and therefore *mean the same* for both. But this cannot be the idea. Sentences in D use the basic logical vocabulary, and Dummett points out time and again that the logical constants have different meanings for the platonist and the intuitionist. Rather, what he means is that the following holds:

(2) For all $\varphi \in D$, the intuitionist asserts φ if and only if the platonist does.

This is the sense in which decidable sentences "obey a classical logic". However, it doesn't *follow* from (2) that they only involve notions concerning which there is no dispute. That would only follow if there were nothing more to the meaning of these sentences than their assertion conditions, so that (2) would *entail* that D-sentences do mean the same to both. But Dummett doesn't favor such a crude behavioristic meaning theory. This is clear from his remarks about the logical constants, and also from his claim that what the intuitionist means can be *explained* in terms which are not in dispute. On the crude meaning theory, there would be nothing further to explain about sentences in D.

It is thus somewhat mysterious how (2) could do the work Dummett wants it to. Consider the following D-sentence:[8]

$$\varphi_0 \ = \ \mathit{prime}(2^{10540}+1) \vee \neg \mathit{prime}(2^{10540}+1)$$

The intuitionist and the platonist can both assert φ_0, but on very different grounds. For the intuitionist, φ_0 is true since there is an algorithm for determining if a number is prime, which we know in advance will terminate, even if

[7] That is, terms and D-formulas have the following forms:

terms: $0,\ S(t),\ t_1 + t_2,\ t_1 \cdot t_2$

formulas: $t_1 = t_2,\ t_1 < t_2,\ \neg\varphi,\ \varphi \wedge \psi,\ \varphi \vee \psi,\ \varphi \rightarrow \psi,\ \exists x(x < t \wedge \varphi),\ \forall x(x < t \rightarrow \varphi)$

[8] Allowing standard extensions by definition from D-formulas, such as e.g. $\mathit{prime}(x)$.

we don't know the outcome for this particular number. The platonist recognizes that this is a ground for asserting φ_0, but he has a much a simpler one: it is a trivial *logical* truth. Surely, this is a strong indication that φ_0 means a different thing for the platonist than for the intuitionist.

So the sense in which decidable sentences constitute a common ground is too weak, it seems. Nor is there a common way they are used in standard explanations of the meaning of the logical constants. For the platonist, decidable sentences play no role at all in that explanation. There is no difference for him between φ_0 and

$$\varphi_1 \vee \neg\varphi_1$$

when φ_1 is undecidable. The intuitionist, on the other hand, might use decidable sentences in a first approximation of the meaning explanations, going beyond them to deal with quantification over infinite domains. No *common* role is played by decidable sentences in these respective explanations.

4.2 A neutral metatheory?

To understand what Dummett is after we must, I think, pay less attention to the class of decidable sentences and the fact that these have the same assertion conditions for everyone. Instead, we should focus on his idea that the respective meaning explanations themselves can be given in terms which are understandable to the opponent. In [Dummett 1973], he is mostly interested in how the platonist can come to understand the intuitionist:

> It is therefore wholly legitimate, and, indeed, essential, to frame the condition for the intuitionistic truth of a mathematical statement in terms which are intelligible to a platonist and do not beg any questions, because they employ only notions which are not in dispute. ([Dummett 1973], p. 239)

Dummett goes on to say that this is most naturally done by carefully describing the intuitionistic notion of truth, in terms of the existence of a proof, to the platonist. He comments, concerning the success of such explanations, that although the other side may not accept them as legitimate, "at least the conception of meaning held by each party is not wholly opaque to the other" (ibid.. p. 238). This remark relates to the fact that the intuitionist insists that mathematical truth *cannot* be explained in the platonist manner. In the other direction no similar problem is mentioned. In fact, the rest of his discussion concerns the very notion of intuitionistic truth: e.g. whether one should require the actual possession of a proof or if it is enough to have the means (in principle) to obtain one. This leads to an intricate analysis of the role of so-called *canonical* proofs, but there is no indication that the platonist should have greater difficulties following these arguments than anyone else.

When Dummett returns to the issue of mutual understanding in [Dummett 1991], his approach is slightly more formal:

> What is needed, if the two participants to the discussion are to achieve an understanding of each other, is a semantic theory as insensitive as possible to the logic of the metalanguage. Some forms of inference must be agreed to hold in the metalanguage ... but they had better be ones that both disputants recognise as valid. ...
>
> Thus, within sentential logic, the semantics of Kripke trees or Beth trees is insensitive to whether the logic of the metalanguage is classical or intuitionistic: exactly the same forms of inference can be shown valid or invalid on that semantic theory. If both disputants propose semantic theories of this kind, there will be some hope that each can come to understand the other; there is even a possibility that they may find a common basis on which to conduct a discussion of which of them is right. ([Dummett 1991], p. 55)

Although Dummett carefully distinguishes formal semantic theories from the 'real thing', i.e. theories of meaning, he apparently thinks that if the language in which such semantic theories are expressed has a logic not in dispute, at least a road towards mutual understanding is open. He is also explicit that 'internal semantics', e.g. a semantics for an intuitionistic theory given in an intuitionistic metalanguage, is of no help here. No technical details are given, but presumably Dummett is referring to intuitionistic proofs of *completeness theorems* for intuitionistic logic. A completeness theorem says precisely that a certain formal semantics captures the notion of validity in a certain logic or theory.[9]

The point cannot be that the platonist too understands the metalanguage and the logic in which the completeness proof is carried out — if he did there would be no point of the exercise. Rather, the idea must be that there is now a formal characterization of a certain set of intuitionistic validities, whose correctness is accepted by the intuitionist, as well as (via the classical completeness proof) by the platonist. One may grant, as Dummett indicates, that this could provide some basis for a discussion between the two on the merits of that system of intuitionistic logic.

Again, this is only understanding in one direction. For truly *mutual* understanding by these means, we would also need an intuitionistically acceptable proof of the completeness of a relevant system of *classical* logic; say, first-order logic. However, it is known that such a proof doesn't exist.[10]

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[9] It was first believed that completeness theorem for Kripke or Beth semantics for intuitionistic systems could only be proved classically, but Weldman and de Swart realized that if one allows contradictory worlds (worlds in which some sentences are both true and false), completeness with respect to this class of models could be proved intuitionistically. See, for example, [Lipton 1992] for results of this kind.

[10] This was shown by Gödel and Kreisel; for stronger versions, see [McCarty 1996]. It should be noted that [Krivine 1996] shows that the fact that every consistent set of sentences (in a countable language) has a model can be proved intuitionistically; see also the exposition in [Berardi and Valentini 2004]. Classically (but not intuitionistically), the completeness of classical first-order logic follows almost immediately from this fact. So some measure of understanding can perhaps be obtained in this case too.

We thus see, following Dummett, that whether one takes the direct route of explaining the intuitionistic meaning of the logical constants, or the more indirect route via completeness theorems, an *asymmetry* appears: it seems fairly clear how the platonist could go about understanding intuitionism, but much less clear how understanding in the opposite direction would work.[11] Indeed, in several other places, Dummett says explicitly that the intuitionist cannot understand or make sense of classical logic or mathematics, because it doesn't make sense: it is *unintelligible*.

4.3 Intelligibility and translation

How seriously should one take Dummett's claims about unintelligibility? On the one hand, he continues Brouwer's antagonistic stance towards classical mathematics, saying that intuitionistic theorems "refute certain classically valid logical laws" ([Dummett 1977]: 84). One may wonder how a theorem can refute a meaningless statement. On the other hand, he takes the issue of mutual understanding and a common ground very seriously, as we have seen.

Perhaps one should take the unintelligibility claim at face value. Perhaps laws like LEM are refuted in the sense that the only meaningful way to understand them renders them invalid. And perhaps mutual understanding must always be approximate or partial.

At this point, an observer can only note that if Dummett is *right*, the prospects of mutual understanding are bleak indeed. To get any further, he would have to engage in the philosophical debate, which is not my ambition here. A remaining point, however, would be to account for the fact that classical mathematics *appears* to make sense. After all, it does so to the vast majority of mathematicians.

Intuitionists often explain this via the various *negative translations* that exist from parts of classical mathematics into corresponding constructive theories. The idea is that when the platonist asserts φ, what he really means — or alternatively, all he can be taken to mean — is φ^{neg}, where φ^{neg} is some translation of φ (in the same language) such that, if T_C and T_I are the relevant axiomatic theories, φ and φ^{neg} are equivalent in T_C, and T_C proves φ if and only if T_I proves φ^{neg}.

But there are problems with this view. First, it only concerns certain axiomatized *parts* of mathematics. Second, such translations yield (relative) *consistency* of the classical theories (since they preserve negation), and so the intuitionist can take them to indicate that classical mathematics is at least consistent, but that is a far cry from making sense of it. Of course, an extreme view would be that this is the only sense to be had. But the translation is often taken to show more, namely, that what the platonist mathematician is really after are the translated versions of his theorems. And at this point, the asymmetry in

[11] The first claim is also a standard platonist view: he can follow the intuitionistic explanations of the logical constants, as well as intuitionistic mathematical proofs (given the way the relevant intuitionistic concepts are defined); but he sees no reason to declare that these are the only acceptable proofs.

understanding shows up again. For even if φ and φ^{neg} are provably equivalent, if you take a reasonably complex classical theorem φ and tell a platonist that what he *really* means is φ^{neg}, he might just deny that that was what he had in mind when he was thinking about how to prove φ.[12]

In other words, even for these theories (like PA versus its intuitionistic version, Heyting Arithmetic, HA), the platonist and the intuitionist would not *agree* about what the classical mathematician is up to. By contrast, if the platonist 'translates' an intuitionistic statement using the BHK explanations of the logical constants, or further intuitionistic elaborations about meaning as in e.g. [Dummett 1991], they might well agree about the truth or falsity of the statement understood in *this* way.

4.4 Summing up

We started with the need for an outside view, but have so far focused on whether mutual understanding between the two camps is possible. But that's an entirely relevant issue. If we had found, for example, that each party can fully understand what the other is up to, and is willing to admit that both are doing mathematics and that no inconsistencies are likely to arise, then the conflict would only be about which kind of mathematics was most interesting or useful. This is of course highly relevant for matters of research funding or academic appointments, but has little theoretical interest. (It might interest the sociology of science, but hardly the philosophy of science.)

But that is not what we found. There is a striking asymmetry when it comes to understanding what the other side is up to, however such understanding is supposed to take place. The platonist appears to have no serious difficulties in grasping, at least not in principle, via reinterpretation of the logical vocabulary and other means, the intended content of intuitionistic mathematical claims. This is what many classical mathematicians themselves claim, but we saw that Dummett appears to reason along similar lines.

Problems arise, on the other hand, for how classical mathematics is to be understood. If the intuitionist insists that it is fundamentally flawed, she can try to make sense of at least parts of it via negative translations. But it seems unlikely to me that there could be an agreement about *meaning* along these lines. There is likely to be a recognition that what the other side is up to is consistent, but that is a very weak form of agreement.

If we don't want to delve deeply into philosophical questions about meaning, or simply take sides in the conflict, we seem to have reached an impasse.

[12]The argument hinges on notions of meaning that may themselves be controversial. My point is merely to observe that even if a translation preserves theoremhood, it does not automatically follow that it also preserves meaning.

5 Understanding in terms of proofs

The intuitionist I have so far portrayed is of the original Brouwer style, although in Dummett's version, which differs as to philosophical background but not in mathematical content. But there is a newer brand of intuitionism, that I will simply call *modern intuitionism*[13], since it is a dominating trend these days. One starting point is [Bishop 1967], whose explicit aim was to do constructive mathematics that looked just like ordinary mathematics, not even apparently contradicting any classical theorems, and not relying on more or less philosophical notions concerning the continuum or other central mathematical objects, but only paying attention to constructivity (to assert that something *exists*, you must provide an algoritm for finding it). More specifically, it proposed to approach the continuum without using Brouwer's choice sequences, or his ideas about the 'creative subject'. An independent effort with similar aims was [Martin-Löf 1970].

This line of work has been carried on by a number of mathematicians, e.g. Per Martin-Löf, Douglas Bridges, Fred Richman, Giovanni Sambin, Thierry Coquand, to mention just a few,[14] and today encompasses an impressive body of mathematics.

Some of the modern intuitionists are still concerned with philosophy and the foundations of mathematics, whereas others prefer to let the mathematics speak for itself. But one thing that separates them from the old style intuitionists is their (explicit or implicit) adherence to the slogan:

(*) Every intuitionistic theorem (proof) is a classical theorem (proof).[15]

This appears to provide a way out of the impasse mentioned above.

5.1 Truth and assertability

The impasse stemmed from the radically different notions of truth entertained by the two sides: for one it is a primitive, fundamental, and 'metaphysical' notion; for the other it is a secondary epistemic notion, defined in terms of proof. Although this difference may make mutual understanding impossible at the level of a theory of *meaning*, it is worth pointing out that in one important respect, the differences over truth don't matter. The point is that both parties have essentially the same notion of *assertion.*

Assertions in mathematics are *theorems* (or propositions, lemmas, etc.), and with some simplification (actually a lot) we can say that the main goal of mathematical scientific activity is to deliver theorems. And regardless of any difference

[13] Some of its practitioners would prefer to avoid the label "intuitionism" altogether, using "constructivism" or "constructive mathematics" instead. But it is just a label here.

[14] Again, I am ignoring the various differences concerning the nature of constructivism/intuitionism and platonism among these scholars.

[15] For example, Brouwer's CONT is not a theorem of modern intuitionistic mathematics. See also footnote 19.

over what truth is, both sides agree about the following:[16]

(a) To assert something in mathematics, you need a *proof*.

(b) Provable statements are true.[17]

That is, for the purely mathematical activity, the differences come down what proofs to accept. Certainly, a platonist might claim that there are true statements of arithmetic whose proofs we will never know, or even truths that don't have proofs. But that is not a mathematical claim.

Relying on (*), one may affirm that intuitionistic mathematics is a part of classical mathematics. But the converse affirmation is also popular.

5.2 Classical and intuitionistic mathematics as special cases of each other

The implementation of (*) (in either version) in a specific area of mathematics T often takes roughly the form:

(**) classical version of T = intuitionistic version of T + AX

where AX is a particular axiom, like some version of LEM, or the unrestricted axiom of choice, or the power set axiom. For example, HA can be formulated so that one obtains PA simply by adding LEM as an axiom. This has of course been known for a long time, but a result of the work of modern intuitionists has been to extend (**) to ever larger parts of mathematics.

Classical mathematics is then a special case of intuitionistic mathematics in the sense that it allows fewer models (having more axioms); in particular, AX disallows 'computational' models that intuitionists take a special interest in.[18] A different and perhaps clearer way to make the same point is that without AX, many mathematical notions bifurcate. For example, intuitionistic logic distinguishes between a statement's not being true and its leading to contradiction. Or consider *formal topology*, a constructive approach to topology initiated by Martin-Löf and Sambin, where the duality between closed and open sets remains, but a closed set is no longer defined as the complement of an open set; only with classical logic do these two notions collapse into one.[19] For a final example, intuitionistic analysis doesn't have access to the axiom

$$\forall x \in \mathbb{R}(x = 0 \lor x \neq 0)$$

but gets by with slightly weaker principles like

$$a > b \to \forall x \in \mathbb{R}(a > x \lor b > x)$$

[16] "...the intuitionist's view is that ...you are not entitled to assert that a theorem is true until it's proved, which sounds much like a realist's view also" ([Richman 1990], p. 124).

[17] At least if we restrict attention to number theory and analysis.

[18] See [Richman 1990] for a forceful statement of this claim.

[19] See e.g. [Sambin 2003]. Thus, (**) should not be taken to entail that both sides use the same language. Roughly, the intuitionistic language extends the classical one, but in such a way that when AX is added, the extra intuitionistic vocabulary can be eliminated.

$$\forall x \in \mathbb{R}(\neg(x > 0) \rightarrow x \leq 0)$$

(see [Bridges 1998]). With LEM, one never even thinks of these distinctions.

On the other hand, in another clear sense, intuitionistic mathematics is a special case of classical mathematics, i.e. the special case where one investigates how to get by without certain axioms. For particular axiomatized theories, (**) expresses just that. For mathematics in general, i.e. for (*), this is presumably not something one can prove (see below), but it appears to be a shared conviction. This notion too goes well with the idea that intuitionistic mathematics is the computational part of classical mathematics.[20]

Clearly, from either of these (fully compatible) perspectives, the *conflict* between platonists and (modern) intuitionists becomes less serious. Focus has shifted from what lies behind mathematical truth to what proofs to accept. Indeed, there is no necessity to take a stand, as witnessed by the fact that a number of mathematicians do *both* classical and intuitionistic mathematics. For example, a set theorist can study classical extensions of ZFC, and the status of the Continuum Hypothesis or large cardinal axioms, and at the same time be interested in constructive versions of set theory. At the extremes, there will be platonists who find the abandonment of certain obvious valid methods of proof wholly unmotivated, and intuitionists who see no justification at all in the extra axioms. In between, all kinds of positions are possible. But when the differences have been reduced to whether or not this or that axiom can be used, those interested in philosophical foundations can focus on those axioms, and the others — the majority of mathematicians — can keep studying what follows from what, which proofs are more effective, or more elegant, or more informative, etc. The threat of conflict, in the sense of proving theorems that contradict each other, seems to have disappeared.

End of story? Recall that the peaceful coexistence between classical and intuitionistic mathematics envisaged here wholly builds on (*). I will briefly consider the evidence for (*), and conclude with some remarks indicating that some problems still remain.

5.3 Evidence for (*)

If (*) holds, no inconsistency between classical and intuitionistic mathematics can ever arise. How sure can we be of (*)? As long as we restrict attention to specific theories for which (**) holds, we are safe. But everyone knows that mathematics cannot be fully captured within any formal system, and especially intuitionists have emphasized the open-endedness of the mathematical enterprise: its methods can never be laid down once and for all. This may not

[20] This statement is imprecise. For some, the computational part of mathematics is essentially recursive function theory. Intuitionists emphasize that recursive functions too must be studied with constructive methods, e.g. without assuming LEM. Also, they reject the idea that intuitionists study subclasses of classical mathematical objects, such as constructive real numbers (as opposed to all real numbers) or recursive functions (as opposed to all function among natural numbers). Instead, they maintain that if you study e.g. number-theoretic functions with constructive methods, these functions will in fact all be computable; see [Richman 1990] and [Bridges 1998].

matter much to the working mathematician, but it certainly matters for the methodological question of the validity of (*).

How could we *know* (*), *once and for all*? Note that the reformulation of intuitionistic mathematical theories in the form (**) has by no means been an easy matter, but the result of hard mathematical work. The methodological considerations underlying this work are, when they are made explicit,[21] still some form of the BHK explanations of the logical constants. However, these explanations by themselves really don't give full evidence for (*). This observation is not often made, but an exception is Dummett, who notes that the problem lies with intuitionistic *implication*:

> In some very vague intuitive sense one might say that the intuitionistic connective $\to$ was stronger than the classical $\to$. This does not mean that the intuitionistic statement $A \to B$ is stronger than the classical $A \to B$, for, intuitively, the antecedent of the intuitionistic conditional is also stronger. The classical antecedent is that A is *true*, irrespective of whether we can recognize it as such or not. Intuitionistically, this is unintelligible: the intuitionistic antecedent is that A is (intuitionistically) *provable*, and this is a stronger assumption. We have to show that we could prove B on the supposition, not merely that A happens to be the case (an intuitionistically meaningless supposition), but that we have been given a *proof* of A. Hence intuitionistic $A \to B$ and classical $A \to B$ are in principle *incomparable* in respect of strength. We may sometimes have a classical proof of $A \to B$ where we lack an intuitionistic one; but *there is no reason why the converse should not sometimes hold too.* ([Dummett 1977], p. 17, last italics mine)

To flesh out these remarks, consider the following thought experiment. Suppose $\varphi = \psi \to \theta$ were a sentence – we can even assume it is a numer-theoretic sentence – such that:

(i) there is a construction taking intuitionistic proofs of ψ into intuitionistic proofs of θ;

(ii) there is (in fact) no intuitionistic proof of ψ, but

(iii) there is a classical proof of ψ and a classical proof of $\neg\theta$.

Of course, these claims about existence and non-existence of proofs must be understood relative to some future, not yet discovered, notion of number-theoretic proof. (That's why it is a thought experiment.) Also, assumption (ii) has to be read classically: not in the sense that we can show that ψ's provability would lead to contradiction, but simply that no proof exists. So the thought experiment is only accessible to someone who can make sense of that assumption. But if you cannot do that, probably (*) makes no sense to you

[21] As in the careful meaning explanations in [Martin-Löf 1984].

either.[22] In any case, these assumptions appear consistent. An instantiation of them would be a counter-example to (*).

The existence of such a counter-example seems very unlikely. For all the known theories which satisfy (**), no such example can exist. Perhaps a more general meta-theorem can be proved, ruling out such examples for a large class of theories. And the issue whether we could give a principled argument that there isn't one, in *all* of mathematics, doesn't look like something that could be proved anyway. My point here is merely that (*) doesn't automatically follow from the standard intuitionistic account of the logical constants.

Incidentally, if there were a counter-example φ, it would not constitute a conflict with classical mathematics, at least from the platonist's viewpoint: he would happily acknowledge that $\neg\varphi$ is true, but also that the intuitionistic reading of φ is true! It would, however, show that the relation between classical and intuitionistic mathematics is not quite what it is usually taken to be.

6 Concluding remarks

6.1 The asymmetry remains

We found that the attempts to achieve mutual understanding between platonists and intuitionists via a common ground of unproblematic statements, or via a meta-theory that was not in dispute stranded — or at least were far from successful — because of the apparent *asymmetry* of understanding that resulted. The platonist could claim he has no principled problem of understanding what the intuitionist is up to. The intuitionist might even agree that this understanding is essentially correct. But if she also insists that classical mathematics is at bottom unintelligible, there can be no corresponding agreement about how to understand classical mathematics. For those who still pursue Brouwer style intuitionistic mathematics, as well as for those who base their adherence to intuitionistic logic on a Wittgenstein-inspired theory of meaning, like Dummett or Prawitz, there is no real possibility of reconciliation. Despite efforts to find a commond ground, they must in the end argue that "classical logic contains some invalid forms of reasoning, and consequently has to be rejected" ([Prawitz 1977], p. 2).

Modern versions of intuitionistic mathematics appear to allow for friendlier relations. We noted that this stance presupposed that every intuitionistic theorem is also a classical theorem, a highly nontrivial claim which does not follow automatically from the standard intuitionistic explanations of what the logical vocabulary means. But the claim has been remarkably borne out in mathematical practice. Let us assume it is true. Does it follow that peaceful coexistence is now unproblematic?

The threat of platonists and intuitionists proving theorems that contradict

[22]Note that Dummett in the quote above (a) claims that an assumption like (ii) is "intuitionistically meaningless", but (b) uses it to explain the difference between classical and intuitionistic implication.

each other has disappeared. But in an important sense, the asymmetry remains. The platonist still has no problem understanding intuitionistic mathematicians as dealing with the constructive part of mathematics in general. He could even admit that this is a useful and worthwhile enterprise. But nothing similar holds in the other direction. As far as I can see, the intuitionist's only possibility is a *formalist* understanding of classical mathematics: investigating the consequences of certain extra axioms.[23]

The appeal of formalism to mathematicians, of all kinds, should not be underestimated.[24] For one thing, it is a handy retreat position when philosophers or logicians ask too many questions about foundations: I just study what follows from these axioms. For another, it fits with the *aesthetic* aspects of proofs and theorems, aspects which no mathematician ignores.

> What are the criteria for choosing among axiom systems? Generally there are two opposing criteria: interesting models and beautiful theorems. ([Richman 1990], p. 125)

Presumably, a theorem or a proof is beautiful in much the same way as a game of chess can be beautiful. But, as Richman indicates, beauty has little to do with the truth- or knowledge-seeking aspects of mathematics.

On reflection, formalism is not a solution to the problem but a way to ignore it. Besides, I doubt that there are any formalists about number theory. There is a huge literature on axiom systems for arithmetic, and their models. But this is part of *proof theory* or *model theory*, both established mathematical-logical disciplines. To put it crudely, the object of these investigations is proofs, or models, but not numbers. By contrast, consider the immense efforts mathematicians have spent on long standing number-theoretic claims, such as Fermat's Last Theorem or Goldbach's Conjecture. Clearly, the feeling of mathematicians is that we now know that Fermat's Theorem is *true*, whereas Goldbach's conjecture is still *open*.[25]

To be sure, an intuitionist might not accept this result until she is satisfied about the constructivity of the methods. That is, without a constructive proof

[23] The claim that classical mathematics is a special case doesn't really help, if this special case results from ignoring distinctions that one feels should be upheld.

[24] Of course I don't mean Hilbert style formalism, i.e. the idea that the safety of mathematics should be guaranteed by some reduction to a small 'concrete' part of it, about which one is in no way formalist. Formalism here is roughly the view that mathematicians prove theorems in axiom systems, but the choice of axioms is unrelated to questions of truth.

[25] There is an interesting quirk concerning Fermat's Last Theorem, since the actual proof apparently uses methods from category theory not formalizable in ZFC (relying on the existence of inaccessible cardinals; see the discussion in FOM on this issue, for example Harvey Friedman's postings, such as http://cs.nyu.edu/pipermail/fom/1999-April/002992.html), although all specialists are convinced these methods are eliminable and the proof goes through in ZFC. My simple point here is just that virtually everyone agrees that it is the *truth* of Fermat's claim which is at stake here, not which axioms it follows from. The question was unresolved for 350 years, but now it is *settled*. (There are other and perhaps more interesting issues involved, such as why everyone agrees that provability in ZFC, and perhaps even in ZFC + some large cardinal axioms, would guarantee arithmetical truth, and also why no one apparently has found it worthwhile or rewarding to actually perform the elimination of inaccessibles from the proof. But the simple point is sufficient here.)

she would not think that the *truth* of Fermat's Last Theorem had been established, and would presumably be forced to take a formalist stance on the actual proof. And that would be another illustration of the asymmetry.

6.2 From the outside

What should the outside observer conclude, then, about the C-I conflict? A first impression is that the persistent asymmetry we found might not be that serious after all, at least with modern intuitionism. There is no outright conflict, and the fact that one participant in the debate has problems understanding what the other is up to doesn't mean that it *cannot* be understood. The other side claims it can.

I am not being ironic here. Without going into the philosophical debate about meaning, I think all our observer can do is to take seriously the claims of the mathematicians involved. If one group of mathematicians insist they have no problems understanding both kinds of mathematics, and another group insist they have serious problems understanding parts of classical mathematics, so be it.

But an equally strong impression is that we haven't really dealt with the heart of the matter. If the differences between platonists and intuitionists eventually boiled down to matters of *taste*, to which kind of mathematics they *liked best* (and therefore should be funded, etc.), the investigation could stop. But more seems to be involved. Consider the question of *why* modern intuitionists have gone to such lengths about asserting only theorems that the classical mathematician can also assert. There is no *a priori* reason to do so. On the contrary, although both insist on using the same logical symbols, the respective meanings they associate with these symbols are manifestly different, so *a priori* one wouldn't be surprised if some *apparent* conflict emerged (as it did with Brouwer style intuitionism). But the tendency has been to avoid even apparent conflicts. Why?

Presumably, part of the answer is that in this way intuitionistic mathematics is will attract more interest among 'traditional' mathematicians. But that can hardly be the main motivation. Surely the main motivation lies in the mathematical work itself, in the fact that it has proved possible to formulate constructive mathematics in this way. This is a striking and non-trivial fact, and it would appear to merit some principled explanation. Then, the asymmetry might come to look natural, rather than problematic. It seems to me that such an explanation has not yet been given.[26]

That much can perhaps be gleaned from the outside. Providing an explanation, however, most likely would require inside work.

Finally, what about relativism? I think that question too must await an explanation of the kind just asked for. Consider the statement

(3) The real numbers can be well-ordered.

[26] As noted, I don't think explanations via negative translations are adequate in the required sense.

This is a claim students learn to prove during a first set theory course, but which intuitionists (modern or traditional) refuse to believe in. The platonist may argue, as we have seen, that the sentence (3) can express two different claims, the second entailing that we can somehow *compute* such a well-ordering, and he may agree with the intuitionist that we have have no grounds for asserting *that*. A relativist take on this, however, is different. The relativist must argue that there is in fact only one claim, but that the *context of assessment* determines its truth value.[27] In the standard classical set theory context of assessment, (3) is true; in the intuitionist context, the very same claim or proposition is not true.

There is the issue of whether such a relativist stance is internally coherent. Many philosophers doubt that. But setting that issue aside, isn't there some plausibility in the (vague) idea that platonists and intuitionists do talk about the same things, but assess them in different ways? If they only talked about different things, or said different things that only appear similar because the same words are used, their disagreement would be somewhat trivial. But there is a strong impression that it is not trivial in that way. An explanation of the 'real' relation between classical and constructive mathematics, and of the way platonists and intuitionists understand each other, should clarify this situation too. Whether some form of relativism is involved is, I think, anybody's guess.

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[27] This would be relativism in the sense of [MacFarlane 2005].

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