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Is the language of intuitionistic mathematics adequate for intuitionistic purposes?

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Abstract

Is the language of intuitionistic mathematics adequate for intuitionistic purposes? In this paper I argue that it is, replying to several arguments by Hellman (1989) and Hossack (1990), (1992): I agree with these authors that there are classical propositions, such as the assertions of absolute unprovability, which are not expressible in a purely intuitionistic language, but I show that these propositions are not strictly needed for the development of intuitionistic logic or mathematics.

Keywords: intuitionistic mathematics; classical mathematics; mathematical language; meaning of the logical constants.

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1. INTRODUCTION

As is well known, in intuitionistic mathematics the logical connectives and quantifiers are subjected to a peculiar interpretation, on the lines of the intuitionistic philosophy of mathematics. According to this philosophy, mathematical objects are mere constructions of the human mind, and do not have an existence independently of us. Similarly, mathematical statements do not refer to 'facts' which are made true by an outside world, but to our own capabilities of performing constructions with certain properties.

Following this, for example -and roughly speaking-, the meaning of a statement of the form A ∪ B is not that either A or B is 'true', but that we are actually in a position to prove either A or B; the meaning of $A \rightarrow B$ is that we can transform any hypothetical proof of A into a proof of B; and the meaning of $\forall x\, P(x)$ is that we have a method to produce a proof of P(a) for every object a in the domain. (For a full exposition of the usual explanations, see Troelstra and van Dalen (1988), pp. 9-10, or Dummett (2000), pp. 8-9.)

The task of turning these meaning explanations into a precise definition is proving to be extremely difficult (I discuss some of the difficulties in my papers (1999) and (2000)). Meanwhile, many philosophers have claimed that these explanations are internally incoherent, so that the attempt to make them more precise -and in turn the whole project of intuitionistic mathematics- is condemned from the start.

In recent times, notably Hellman (1989) and Hossack (1990), (1992), have argued that a purely intuitionistic language (i.e., a mathematical language as intuitionistically interpreted) cannot be sufficient to express certain basic propositions which are indispensable to the motivation and development of intuitionistic mathematics, thereby making intuitionistic language inadequate to serve its own purposes. According to these authors, the intuitionistic mathematician is forced to resort, from time to time, to a classical language, reifying over mathematical objects (i.e., treating mathematical objects as existing independently of the human mind), and hence falling into the same kind of 'errors' that he notoriously criticizes.

In the present paper I shall examine the main arguments given in those papers by Hellman and Hossack, and I shall hopefully show that none of them is enough to grant the conclusion that the language of intuitionistic mathematics is essentially defective. I agree with these authors that there are some propositions (such as the assertion of absolute unprovability, as we will see immediately), which make sense only classically and not intuitionistically, and hence are not expressible in a purely intuitionistic language. But I defend that these propositions are not strictly needed for the development of intuitionistic logic or mathematics. Hence the arguments presented by Hellman and Hossack are not enough to establish that intuitionistic language is not adequate to serve its own purposes, or that the intended meanings of the intuitionistic logical constants are internally incoherent.

2. HELLMAN AND THE COMMUNICATION PROBLEM

Hellman (1989) argues that the intuitionistic logical constants (i.e., the logical connectives and quantifiers as intuitionistically interpreted), are insufficient to express certain basic facts about themselves, about their own status and behaviour, which are essential for the motivation of intuitionistic against classical mathematics. In other words: that the motivation of intuitionistic against classical mathematics cannot be conducted in a purely intuitionistic language.

In particular, he focuses on the impossibility of expressing intuitionistically the idea of absolute undecidability (undecidability per se, not within a specified formal system), i.e.: the idea that some mathematical statements might never be proved or disproved. As is well known, this is one of the common arguments of intuitionists for the rejection of the law of excluded middle: if there are mathematical statements which we shall never prove or disprove, on what grounds can we assume that they nevertheless have a fixed and independent truth value?

The reason why this idea is not expressible in a purely intuitionistic language is very simple: intuitionistically, if we can establish that it is impossible to give a proof of a given mathematical statement, we have thereby refuted that statement, i.e., we have proven its negation. Hence we can never be in a position to show (from the intuitionistic standpoint) that a given statement can neither be proved nor disproved. (A very different thing, of course, is that of provability *within a particular formal system*.)

As Hellman acknowledges, intuitionists are well aware of this fact: he refers to a quote from Dummett (1977), where Dummett points out that "a demonstration that A is not provable serves [intuitionistically] (...) as a refutation of A" (Hellman (1989), p. 60, referring to Dummett (1977), p. 17, which corresponds to p. 11 in the second edition of this work, Dummett (2000)).

Hellman goes on, however, to stress that without the notion of absolute undecidability the usual motivation for intuitionistic logic is not feasible:

> "Take for example intuitionism's stance on 'the law of excluded middle'. How does it motivate its refusal to accept this law (admittedly without having to claim to refute it)? Not by the paltry observation that there are propositions which we now can neither prove nor refute. (...) Now, I take it, what gives real force to intuitionism's stance is that this may never happen, that is, (possibly) at any stage, exceptions to (LEM)i [the law of excluded middle as intuitionistically interpreted] can be found."
> (Hellman (1989), p. 62.)

However, he writes, this possibility is not expressible intuitionistically: indeed, the corresponding universal quantification 'for every stage of knowledge, there is at least one unsolved problem' would have to be read classically, because intuitionistically it would mean that we have a method to generate a new unsolved problem in every new stage of knowledge, which is obviously false and not what was intended.

He quotes Dummett (1977) to illustrate the fact that intuitionists, in their critique to classical reasoning, adopt notions such as the absolute unsolvability of a mathematical problem:

> "Since we can be virtually certain that the supply of such unsolved problems will never dry up, we can conclude with equal certainty that the general statement will never be intuitionistically provable. Such a recognition that a universally quantified statement is unprovable does not amount to a proof of its negation (...)."
> (Dummett (1977), p. 45 (p. 32 in the second edition), quoted on Hellman (1989), pp. 63-64.)

However, as Hellman remarks, the 'never' in this statement has to be read classically, or the paragraph would not make sense. In fact the second part of the last sentence of this quotation (which Hellman omits) is also very interesting:

> "Such a recognition (...) does not amount to a proof of its negation, since the proposition is not, as it stands, a theorem or even a mathematical proposition at all."
> (Dummett (1977), p. 45, p. 32 in the second edition.)

This means that Dummett realizes immediately that the proposition in question (that there will always be unsolved problems) implies a certain reification and that strictly speaking is not intuitionistically meaningful.

Hellman's point is an interesting one, which can be found in other authors in somewhat different forms. Two things should be noticed here. First, that the type of motivation for intuitionism that Hellman criticizes is only needed in the context of a structuralist dominated ideology, but it is not essential for the *actual practice* of intuitionistic mathematics. In other words: the point of using classical language (and classical notions) to motivate the rejection of classical mathematics, is to show the internal weaknesses of the classical position as a whole. Given that the classical view is by far preponderant among philosophers and mathematicians (and indeed virtually all of us have learned mathematics at school under a classical, mostly platonistic, presentation), the significance of this type of argument can hardly be discussed.

Secondly, I think that it is quite possible to reconstruct these motivations, to a certain extent, using strict intuitionistic language. In particular, the justification of the rejection of the law of excluded middle could be based *only* on the existence of particular instances of unsolved mathematical problems, and not on the plausibility that there will *always* be some, because this possibility implies a reification which makes sense classically, but not intuitionistically. Indeed, each unsolved mathematical problem is a counterexample to the law of excluded middle as intuitionistically interpreted: if we cannot solve a problem, either positively or negatively, on what grounds do we suppose that it already has a fixed solution? (That is why the law of excluded middle is put into question.) Hence, as long as there remain problems to solve, intuitionists will have a raison d'être, and this applies to all foreseeable future.

3. HOSSACK ON THE MEANING OF NEGATION

In (1990) Hossack presents an argument against the intuitionistic definition of negation which is somewhat related to the argument by Hellman that we have just seen. Hossack's main contention in this paper is that, unlike the classical case, an intuitionist user of negation has to have thoughts which are not expressible in the corresponding intuitionistic language.

There are essentially two of these thoughts. The first one is the thought that the assertability conditions for a given statement at a particular time do not obtain, that is: the thought that we do not have a proof of a given statement. As is well known, intuitionistic semantics is generally based on the assumption that the meaning of statements should be given in terms of their assertability conditions, which in the case of mathematical statements amount to their proof conditions, i.e., the conditions for a given construction to be a proof of the statement in question. Apart from this, Hossack relies heavily on the decidability of the proof relation, an assumption which he borrows from Dummett, and which I shall take for granted here for the shake of argument. I have thoroughly discussed these two issues in my paper (2000).

Hossack reasons as follows:

> "Therefore they [the intuitionists] have to know for each A what it would be for a construction to be a proof of *A*. But (...) they cannot know what it would be for a construction to be a proof of *A*, unless they also know what it would be for a construction not to be a proof of *A*." (Hossack (1990), p. 215.)

If this happens with each particular construction, we can generalize:

> "Thus they cannot know what it is for the assertability conditions to be fulfilled, unless they also know what it is for the assertability conditions to fail to be fulfilled."
> (Hossack (1990), p. 215.)

Then:

> "The thought that the assertability conditions do obtain can in a sense be expressed by the proposition itself. But there is no sentence to express the thought that they do not obtain. The only possible candidate for such a sentence is the negation of the proposition, and that is already reserved for a different use.
>
> "This shows that there are thoughts that a competent user of L needs to be able to have to use the language correctly, but which cannot themselves be expressed in L [where L is a mathematical language to be interpreted in terms of assertability conditions]."
> (Hossack (1990), p. 215.)

I partly agree with Hossack here. In fact this observation is not new.
Heyting (1956) had already noticed it:

> "Every mathematical assertion can be expressed in the form: 'I have effected the construction *A* in my mind'. The mathematical negation of this assertion can be expressed as 'I have effected in my mind a construction *B*, which deduces a contradiction from the supposition that the construction *A* were brought to an end', which is again of the same form. On the contrary, the factual negation of the first assertion is: 'I have not effected the construction *A* in my mind'; this statement has not the form of a mathematical assertion."
> (Heyting (1956), p. 19.)

Indeed, the point here is that the statement that the assertability conditions do not obtain is not a mathematical statement proper. This does not mean that we cannot express it at all; on the contrary, the statement, for example, 'I do not have a proof of *A*' is perfectly acceptable from the constructive standpoint (provided that we agree on the decidability of the proof relation). However, it is not, strictly speaking, a mathematical statement, and hence there is no reason why we should want to have a *logical* or *mathematical* operator which corresponds to it.

According to Hossack:

> "Note the sharp distinction between intuitionist and classical negation here. The user of the classical negation also needs to be able to have the thought that the truth conditions for *A* do not obtain. But this thought is always expressible in the classical language itself, if it is equipped with a sign for classical negation. Thus the user of the classical language, unlike the user of L, does not need to have any thoughts that cannot be expressed in the language."
> (Hossack (1990), p. 216.)

However, if we consider, in the classical case, the respective thought that 'I have not proved A' or 'I do not know whether A is the case', we see that the speaker needs to be able to have these thoughts to be able to use A correctly (in particular, to assert it adequately or to refrain from asserting it), but they cannot be expressed with a mathematical symbol.

Of course these type of thoughts will not normally be needed in the mere process of understanding an utterance of a given mathematical sentence, neither in the classical nor in the intuitionistic case; but if we consider the mastery of the language as a whole, then these type of thoughts are probably needed in both cases.

The second thought to which Hossack refers, is that the statement $0 = 1$ (which often appears in the definition of $\neg$) is an unprovable statement; and relatedly, the thought that it is impossible to prove both a statement and its negation (assuming $\neg A$ is defined as $A \rightarrow 0 = 1$):

> "But we cannot express in *L* our knowledge that it is absurd to suppose that $0 = 1$ can be proved [where L is as before]. The best we can do within L is to say *not* $(0 = 1)$, which is just $0 = 1 \rightarrow 0 = 1$. (...) Thus the knowledge that A and $\neg$ A are incompatible is not something that one can learn just from the rules of *L* by reasoning within *L*. "(...)
> We can imagine someone who knows only the assertability conditions believing themselves to have proved both A and $\neg$ A, if they thought they had constructed a proof of each proposition. In thinking this, they would be breaking none of the assertability rules of *L* (...) This is again in sharp contrast with the classical case. To be credited with a grasp of the truth conditions of negation, the user of the classical negation must treat a proposition and its negation as incompatible. If someone seriously asserted both *A* and $\neg A$ we would simply conclude that they had not grasped the truth conditions concerned."
> (Hossack (1990), p. 217.)

However, this is not fair, because it is impossible to know the constructive meaning of 0 = 1 (that is: in terms of basic calculations) without realizing that it is false, i.e. that there can be no proof of it. That is precisely the reason for choosing such a simple statement for the definition of negation. When we assert that $0 = 1$ is unprovable, i.e., that there can never be a proof of it, we commit a certain amount of reification, since such an assertion ranges over all possible mathematical constructions ever. But the scope of this reification is extremely small, since it applies only to the hypothetical proofs of a proposed construction, that corresponding to $0 = 1$, whose impossibility is absolutely obvious.

Even this little amount of reification could be avoided, simply by refraining from explicitly asserting the unprovability of $0 = 1$. The actual practice of intuitionistic mathematics would remain the same, and anyone who understands the constructive meaning of *0 = 1* will appreciate the vanity of searching for a proof of it, or of any other statement implying it. But as I have just said, I do not think that the reification contained in the explicit assertion that *0 = 1* is unprovable constitutes a serious deviation from the intuitionistic doctrine.

4. HOSSACK ON THE MEANING OF THE QUANTIFIERS

In a second paper, (1992), Hossack articulates similar arguments against the intuitionistic quantifiers. In particular, he claims that given the clause for the intuitionistic $\forall$, the speaker should necessarily have an explicit knowledge of the metalanguage:

> "The constructivist theory (...) demands that object language speakers who grasp the meaning of 'all n are *P*' should recognize of a construction that for all n it yields a proof of *P(n)*. (...)
>
> "What has to be recognized here is the obtaining of a certain state of affairs described by a sentence of the metalanguage."
> (Hossack (1992), p. 85.)

Then, Hossack argues that if this knowledge is to be explicit knowledge at the level of the metalanguage, another metalanguage would be required for the metalanguage, and we would end up in an infinite regress. However, he writes:

> "It may be replied that constructivism does not need the assumption that the ability to recognize that a construction is a proof should take the form of explicitly thinking this thought in an appropriate language."
> (Hossack (1992), p. 86.)

In fact, previously he himself had referred to a theory of propositional attitudes to dissolve the circularity of a classical theory and complement it:

> "The success of a classicist theory of meaning will then turn on its containing a second component, a theory of propositional attitudes, which explains what it is for a person to grasp an abstract object of this sort."
> (Hossack (1992), p. 85.)

However, this, alleges Hossack, is not available in the case of the constructivist quantifiers:

> "Thus the explanatory force of the constructivist truth theory can turn on its appeal to recognitional capacities possessed by speakers quite independently of any linguistic competence. "This defence is plausible in the case of the connectives of sentential logic, which is decidable both classically and constructively. (...)
> But there can be no hope of a similar account in the case of quantification, for no mechanical device can capture our conception of generality."
> (Hossack (1992), p. 86.)

Here Hossack is simply making a fallacy of equivocation. Because the 'fact' to be recognized here is not the universal fact itself, but the fact that a given construction *proves* the universal fact. These are two very different things. Then he writes:

> "Suppose we had a device with a detector which checked items for some property, and another detector causally sensitive to items that are still unexamined. The device could tell us if $\forall x\ P(x)$ were false. If $\forall x\ P(x)$ were true it could tell us so if there were only finitely many cases for it to examine. But our judgements about the truth of x P(x) go beyond the deliverances of the device. For we will say $\forall x\ P(x)$ is true even if the device itself never returns an answer. For if it never will, that can only be because $\forall x\ P(x)$ is true. It is therefore clear that what the device says does not capture our conception of generality. Nor is there a superior device which could fill this role, as the recursive unsolvability of the halting problem shows."
> (Hossack (1992), p. 87.)

However, the point was not detecting $\forall x\ P(x)$, but detecting whether a given construction was a proof of it; and for that reason Hossack has simply not shown that we cannot have a theory of propositional attitudes which correspond to and explain it.

5. CONCLUSION

In sum, neither of the arguments of Hellman (1989) and Hossack (1990), (1992), the most important of which I have examined in this paper, are powerful enough to grant the conclusion that the language of intuitionistic mathematics is not adequate to serve its own purposes.

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