Absolute and relative concepts in logic

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Incompleteness of arithmetic

It is a common wisdom that whereas *consequence* or *entailment* is a *semantic* concept, *provability* is a *syntactic* concept. However, what exactly does this mean?

What is provability? In the traditional, intuitive sense, to prove something is to demonstrate its truth, and indeed the Latin word for proof is *demonstratio*. Hence in this sense, we cannot prove something unless it is true. Now in the course of his well known proof of the incompleteness of arithmetic, Gödel showed that provability within the axiomatic system of Peano arithmetic (PA) can be reconstructed as a number-theoretic predicate **Pr** and he showed that within the system there exists a statement **G** so that

(*) $\mathbf{G} \Leftrightarrow \neg\mathbf{Pr}(\ulcorner\mathbf{G}\urcorner)$.

Now if we assume that for every statement X of PA it is the case that X is true whenever $\mathbf{Pr}(\ulcorner X\urcorner)$, we have the following argument:

$\mathbf{Pr}(\ulcorner\mathbf{G}\urcorner) \Rightarrow \mathbf{G}$ is true $\Rightarrow \mathbf{G} \Rightarrow \neg\mathbf{Pr}(\ulcorner\mathbf{G}\urcorner)$.

This means that, in pain of contradiction, it cannot be the case that $\mathbf{Pr}(\ulcorner\mathbf{G}\urcorner)$. Hence $\mathbf{Pr}(\ulcorner\mathbf{G}\urcorner)$ is bound to be false, and hence $\neg\mathbf{Pr}(\ulcorner\mathbf{G}\urcorner)$ is bound to be true. However, this means that, by (*), **G** is bound to be true. Hence **G** is true (although it is not provable).

Now is what we have just displayed not a (sketch of a) *proof* of **G**? The answer of Gödel and his followers is *No; it is not a proof in the strict* (sic!) *sense of the word*. (For if **G** were provable, it would follow that it is unprovable, and the whole system of PA would be in ruins – whereas we take for granted that PA is consistent, for what it spells out are our most basic intuitions regarding the manipulatability of natural numbers; intuitions which are *axiomatic* in the primordial sense of the word[1].) Nevertheless, it is hard to deny that it *is* a *demonstration* of the truth of **G** (everybody who reads it – and has basic skills in mathematics

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[1] Hence **G** is valid not merely in force of the validity of the axioms of PA, but also of the consistency of PA, which may be seen as warranted by what Gaifman (2000, p. 467) calls the „recognizable validity“ of PA. And, as Gaifman points out, „a recognizably valid T cannot capture our full mathematical reasoning; because we shall reason that T is consistent, and *this* reasoning is something T cannot deliver.“ Thus, according to Gaifman, it is not the case that there are statements which are true but unprovable, but rather that there are statements which are provable only within a theory we cannot ‚get outside of‘. Hence the moral we should draw is not the one drawn by Penrose (1990) and others, namely that we are not machines, but rather that „we (qua mathematicians) are machines that are unable to recognize the fact that they are machines“. I think that Gaifman is fully right (see also Peregrin, 1998).

– *sees* that **G** has to be true), hence that it is a proof in the intuitive sense – so why do we introduce an unnatural, ‘strict’ sense of “proof”? Well, the reason appears to be that we cannot make a mathematically rigorous sense of the intuitive concept, and so we have to make do with its relativist ‘circumscriptions‘. In particular, the concept of proof as introduced by Hilbert (amounting to derivability from axioms by means of rules) turns out to be incapable of accommodating intuitive proofs of the kind shown above.

Hence we seem to be committed to accepting that not every statement of PA which is intuitively provable falls within the range of **Pr**; and we must see **Pr** as the direct regimentation not of the intuitive, absolute concept of *provability*, but rather of a relative concept of *provability-in-PA* (which is *not* the partialization of the absolute concept of provability to statements expressible in **PA**!). This leads us to the concept of *provability relative to an axiomatic system*, which now appears to be an imperfect, but the only mathematically tractable, counterpart of the intuitive concept of *provability* (*simpliciter*).

However, note that if this is the case, then we must condone the fact that we can never conclude that something is ultimately *unprovable* in the absolutist sense. In particular, we cannot interpret Gödel’s result as saying that there are ultimately unprovable truths; and we cannot use Gödel’s result to argue against the coincidence of the concepts of truth and provability (in the intuitive sense). Viewed from this angle, Gödel’s result does not entail that there are unprovable truths, but rather that the concept of proof escapes rendering in Hilbertian terms. As Dummett (1963, p. 199) puts it: “Gödel’s discovery amounted to the demonstration that the class of [principles for recognizing as true a statement which involves quantification] cannot be specified exactly once and for all, but must be acknowledged to be an indefinitely extensible class.“[2]

In fact, the idea that we could recognize a mathematical statement as true without having proved it, is odd – how else could we come to know a truth of mathematics than by means of acknowledging its demonstrations? (There are, to be sure, philosophers and mathematicians who would insist that we can *perceive* mathematical reality in the way we perceive the outer world – but to claim we can establish the truth of **G** by observing natural numbers seems, at the very least, implausible.) Should we then maintain that truth and proof, within mathematics, remain inseparably connected?

It is clear that if we do so, i.e. if we accept that Gödel’s incompleteness proof yields us a proof of **G**, then we have to see Peano arithmetic as contradictory. The point is that as **G** can be seen to claim its own unprovability, if it is in fact provable and hence true, we have a variant of the liar paradox. If we erase the gap between truth and proof, we no longer have any reason to see truth in **PA** as more than provability, and hence we are presented with **G** as simply a liar sentence. (This is how the situation was assessed by Finsler, 1926; 1944).

Is there a set of everything?

Gödel’s reasoning suggests that the intuitive concept of proof should be replaced by a formal concept which is narrower. There is clearly a sense in which we *have* proved **G** above; however, as accepting this proof at face value would render PA inconsistent, we conclude that it is not a proof in the regimented sense. Is this a feasible solution?

Consider a different, but parallel problem: the problem of the relation between the intuitive concept of set and its regimented variant as yielded by formal set theory. One of the arguments constitutive of the latter was put forward by Georg Cantor, and it aims at showing

[2] See also Gaifman‘s paper quoted above in footnote 1; and cf. also Grim (1991).

that the cardinality of any set is necessarily smaller that that of its power set. We can reconstruct this well known argument as follows (see, e.g. Lavine, 1994): Let S be a set and f an injection from S to Pow(S). Let $s^* = \{x \in S : x \notin f(x)\}$. Then clearly $f(y)=s^*$ for no $y \in S$. (For if this were the case for a y, then $y \in s^*$ would entail $y \notin s^*$ and vice versa). Hence there is no bijective mapping of S on Pow(S), and hence Pow(S) is always larger than S.

Russell's objection to this line of reasoning was that it cannot hold if S is the set of everything. If U is the set, then, by definition, U contains everything, including all its own subsets; hence Pow(U) $\subseteq$U. This means that there *is* a bijective mapping of a part of U on Pow(U) (namely the identity mapping), and hence the cardinality of Pow(U) cannot exceed that of U.

The response to this objection, which has now become so commonplace that it is not easy to see how it could be challenged, is that it only shows that everything does not constitute a set. However, consider the following anecdotic dispute between two persons called PseudoCantor and PseudoRussell:

PseudoCantor: Every group of cars consists of black cars.

PseudoRussell: But on the parking lot in front of my office there is a group of cars not all of which are black.

PseudoCantor: But this only proves that what is in front of your office is not a *group* of cars.

One of the basic intuitions behind the concept of set is that *anything* can be grouped together into a set[3]. My pencil, my left shoe and the Empire State Building constitute a set, and so do all chipmunks born in America or all natural numbers. And similarly everything whatsoever would appear to constitute a set. If someone devises a sense of "set" in which everything does not make up a set, then so be it – but this sense seems incapable of doing justice to the underlying intuition.

The fact that there should be the set of everything is especially embarrassing from the viewpoint of the standard logical analysis of language. Such analysis usually counts with a universe of discourse, a set comprising all objects about which we can talk by means of the language in question; hence if there is no set of everything, it would seem there are things we cannot talk about – things which for some peculiar reason escape our linguistic snares. Obviously, though, any thing is an element of myriads of sets, and hence of potential universes of discourse, so we *can* talk about everything – but can do so only by somehow switching among different universes of discourses and thereby between languages.

Thus, what is common to the Hilbertian concept of proof and the Cantorian concept of set is that they both apparently fall somewhat short of doing justice to our underlying intuitions. Both have been defending by claiming that our intuitions are inconsistent and that hence the regimented variants of the respective concepts necessarily cannot do justice to all these intuitions at once. Also, both of them lead to a kind of relativism. In the first case, we have no proofs *simpliciter*, but only various proofs-relative-to-systems; in the second we have no universe *simpliciter* but only various universes-relative-to-discourses.

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[3] More precisely, it is one of the basic intuitions underlying the *Cantorian* concept of set: for there is also an alternative conception (which might be called *Fregean*) and which takes sets as parasitic upon concepts. According to the latter, only such things which constitute the extension of a concept constitute a set.

Two approaches to logic

The above considerations lead to the conclusion that we can distinguish two kinds of logical pursuit. The first is the pursuit of the explication of the absolute concepts of truth, proof, consequence etc.[4]; the second is the pursuit of their relativist counterparts. These two pursuits are, of course, not unrelated (the latter may be taken as a way of carrying out the former); however, many logicians appear to simply identify logic with one of them. And while it is feasible to identify logic with the first of them (possibly taking the second as a means of accomplishing it), it is not so feasible to identify it with the second.

According to what we can call the *absolutist* conception of logic (Frege, Russell, early Carnap), logic provides the ultimate language for spelling out the facts of the world (improving on the inherently mutilating natural language). Hence we cannot get 'outside' of it, semantics is ineffable. In contrast to this, according to what can be called the *relativist* conception of logic (Hilbert, Tarski, late Carnap), logic provides multiple calculi (which can be studied either 'in their own right', or as applied to something). Hence to every language there exists a metalanguage in which we can speak about it and about its semantics

In one of the most discussed papers published in the philosophy of logic, van Heijenoort (1967) referred to the two kinds of logical pursuits as *logic as a language* and *logic as a calculus*, respectively. He described how the absolutist pursuit of truth and consequence initiated by Frege and continued by Russell slowly gave way to the study of logical calculi and their model theories, started before Frege by Boole and Peirce and revived by Löwenheim, Skolem, Hilbert and Tarski[5].

My point is that it is important to beware mistaking the study of the mathematical properties of formally defined calculi for the study of the consequence relation in the ('absolutist') sense relevant to our factual reasoning (Peregrin, 2000). It may be that since the publication of van Heijenoort's seminal analysis, the majority of logicians now realize the importance of the distinction, and perhaps even the inability of the relativist conception to stand on its own feet (save as a piece of pure mathematics); but I think it is still not fully appreciated that the majority of logical results constituting the commonly accepted core of logic are simply not intelligible outside of the relativist conception. To illustrate this, let us consider the completeness proofs which are nowadays usually considered to lie within the very hearth of logic[6].

Within the framework of the absolutist conception of logic, we cannot show that there exists a model of a statement of the (*sic!*) language save within the very language (for the

[4] Of these three, consequence appears most fundamental. Truth is too wide for logic – the truth value of a sentence such as ‚The Earth is round' is surely not a matter of logic. (Logic can at most aspire to capture what other sentences must be true in order for this sentence to be true or vice versa – i.e. to capture the instances of consequence featuring this sentence.) Proof is usually taken to be too narrow, for it cannot cover all instances of consequence (but in the first section of this paper we indicated that this is a tricky matter).

[5] The role of Hilbert in this is rather controversial. (See also Hintikka, 1988.) He is usually understood as the augur of formalism and hence of the relativist conception of logic; but the fact is that his divorce of the absolute and relative concepts was purely methodological. He realized that if we want to study consequence effectively, we may profit from rendering it in mathematical terms so that mathematical methods can be applied to it, disregarding that it is a formal reconstruction of something factual. It was only later that some logicians began to forget that the study of formal calculi is not a goal in itself.

[6] For details see Coffa (1991, Chapter 15).

language has ‘no outside’[7]), hence every proof of the existence of a model of a statement is *eo ipso* a proof of the statement (perhaps a specific, ‘constructive’ case of one, as Carnap, 1928, has it). Hence there is no room for anything like model-theoretic completeness proofs. It is instructive to see how the phenomenon of completeness is handled by Carnap before he converted from the absolutist conception to the relativist one (whose champion he later became). Carnap (1928, p. 98-99) gives, in effect, the following ‘completeness proof’:

1. Let T have no model.
2. Then T $\Rightarrow \bot$ (for $\bot$ is true in every model in which T is, namely none)
3. But then T is inconsistent (for ‘T $\Rightarrow \bot$’ is Carnap’s definition of inconsistency of T).
4. Hence every consistent theory has a model.

Obviously this is utterly trivial; and the reason is that within Carnap’s absolutist setting, there was no room for the distinction between $\Rightarrow$ (consequence) and $\rightarrow$ (derivability).

In contrast to this, if we stick to the relativist conception, showing that there exists a model of a statement of a language becomes a matter of a metalanguage of the language, and hence need not yield a proof of the statement (within the object language). We have the possibility of defining the concepts of *consistency* and *inconsistency* independently of the concept of consequence (‘T is inconsistent’ no longer abbreviates ‘T $\Rightarrow \bot$’, but rather ‘T $\rightarrow \bot$’, where “$\rightarrow$” now represents the relation of derivability by means of the deductive machinery provided by the specific formal system underlying T).

The gap between the model-theoretic notions and the proof-theoretic ones opened by the relativist conception of logic has proved to be fruitful in the sense of creating space for plenty of interesting mathematics. However, it has also created problems, problems which many logicians apparently overlook. (To put it crudely: if we are studying, say arithmetic, we want to know which of its statements are provable and hence true – who cares which of them are derivable within a peculiar formal system?) I think that one of the most important task for logic now is to improve its self-consciousness: to achieve a more distinct understanding of what the results it has achieved, especially since the Gödel’s upheaval, really mean.

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[7] See also Peregrin (1995,§11.5).

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