Chapter 14
Wittgenstein on Incompleteness Makes Paraconsistent Sense

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14.1 "A Completely Trivial and Uninteresting Misinterpretation"

Wittgenstein's comments on Gödel's First Incompleteness Theorem in the *Remarks on the Foundations of Mathematics* were dismissed by early commentators, such as Kreisel, Anderson, Dummett, and Bernays, as an unfortunate episode in the career of a great philosopher. It appears that Wittgenstein had in his sights only the informal account of the Theorem, presented by Gödel in the introduction of his celebrated 1931 paper, and was misguided by it (not that he was the only one: because of the misunderstandings it originated, Helmer said that exposition "without any claim to complete precision"—see Gödel (1931, p. 597)—is the only mistake in Gödel's paper). It is claimed that Wittgenstein erroneously considered essential the natural language interpretation of the Gödel sentence, whose undecidability within (the modified system considered by Gödel, taken from) Russell and Whitehead's *Principia mathematica* is at the core of the First Theorem, as claiming "I am not provable". On the contrary, Gödel's proof can be phrased in syntactic terms in which no such interpretation of the formulas is required.

Commentators were particularly struck by the fact that Wittgenstein seems to take the Gödel formula as a paradoxical sentence, not too different from the usual Liar—and Gödel's proof itself, therefore, as the deduction of an inconsistency:

> 11. Let us suppose I prove the unprovability (in Russell's system) of P; then by this proof I have proved P. Now if this proof were one in Russell's system – I should in this case have proved at once that it belonged and did not belong to Russell's system. – That is what comes of making up such sentences. But there is a contradiction here! – Well, then there is a contradiction here. Does it do any harm here? (Wittgenstein 1953, p. 51e)

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Zermelo, Perelman, and probably Russell himself made similar mistakes in the interpretation of the First Theorem, in the years following the publication of Gödel's results. It is usually maintained that the error rests on a confusion between a theory and its metatheory, or between syntax and semantics (see Perelman (1936), who claimed that Gödel had just discovered a new logical paradox; see also Dawson (1984) on Russell, and on Zermelo's letter to Gödel on this issue[1]), which makes it impossible to understand the difference between the truth predicate, inexpressible (by Tarski's theorem) within the theory to which the First Theorem applies, and the provability predicate, which, on the contrary, is (weakly) expressible (Anderson explicitly charges Wittgenstein with such confusion: Anderson 1958, p. 486). Until a few years ago, the discussion on Wittgenstein's remarks seemed to be concluded by the trustworthy verdict of Gödel himself, who, in a letter to Abraham Robinson, stated that Wittgenstein "advance[d] a completely trivial and uninteresting misinterpretation" (see Dawson 1984, p. 89) of the First Theorem.

However, in recent years some commentators have argued that it is possible to extract interesting philosophical theses from the comments of the *Bemerkungen*. Floyd and Putnam (2000) have claimed that Wittgenstein's intuitions anticipate some metamathematical acquisitions concerning the non-standard models of arithmetic. Wittgenstein's further remarks on Gödel, recently published in cd-rom format within the Bergen project, according to Rodych (2002), show that he did not consider the self-referential natural language interpretation of the Gödel sentence essential to the proof of the First Theorem—on the contrary, he "correctly understood the number-theoretic nature of Gödel's proposition" (see Rodych 2002, p. 380). And the debate is nowadays lively and rapidly evolving, with authoritative commentators taking a stance on Wittgenstein's "real thoughts" in the most important international reviews—from the *Journal of Philosophy* to *Dialectica* and *Erkenntnis*; see also Hintikka (1999), Rodych (1999), Rodych (2003), and Floyd (2001).

I also believe that no significant philosophical idea is past its use-by date. And in this paper I will show that it is possible to provide an interpretation of Wittgenstein's position on Gödel's results in the light of contemporary mathematical logic—that is to say, precisely from the point of view from which the comments of the *Bemerkungen* were most severely attacked. My interpretation, however, shall not follow the line of the latest commentators. In particular, I will not take the direction of non-standard models, suggested by Floyd and Putnam—although I will deal with other models of arithmetics, which definitely deserve to be called "non-standard". If we read Wittgenstein's stance on Gödel's First Theorem as conforming to the single, simple argument to be exposed below, then interesting facts will follow for the philosophical significance of the incompleteness results. The "single argument" will also allow me to vindicate and support two other ideas which harmed Wittgenstein's reputation among mathematicians and logicians: (1) his plain rejection of Hilbert's very idea of a *metamathematics*; and (2) the view that we should not dramatise

[1]In the correspondence between the two mathematicians, as Dawson points out, Zermelo "failed utterly to appreciate Gödel's distinctions between syntax and semantics" (Dawson 1984, p. 80).

the possibility that a calculus turns out to be inconsistent (a dramatisation which, according to some, puzzled Wittgenstein precisely since he began to pay attention to the role of consistency proofs within Hilbert's strategy, see Marconi (1984)). Wittgenstein's ideas on contradiction and consistency proofs were dismissed as absurdities by the same commentators who found his remarks on the First Theorem outrageous, see Anderson (1958) and Bernays (1959).

The "single argument", therefore, will capture several fundamental intuitions at the core of Wittgenstein's philosophy of mathematics—although, eventually, it will not capture them *all*. For instance, I will have to exploit some ideas on the formalisation of deductive theories, and some notions of model theory, which constitute established acquisitions of the current logico-mathematical practice, but which Wittgenstein would probably have rejected. Furthermore, I will not trust Wittgenstein's own declarations, according to which his remarks should not have any strictly mathematical import. On the contrary, my interpretation will entail a strong revisionism with respect to classical logic and classical mathematics.

14.2 Metamathematics Is Just Mathematics

At the core of the "single argument" is the idea that, in maintaining an interpretation of Gödel's proof that made of it a paradoxical derivation, Wittgenstein was consequent upon his bold move of rejecting the standard distinction between theory and metatheory (therefore, between formalised arithmetic and metamathematics).

Logicians have learned precisely from Gödel's results (and from Tarski's, on the undefinability of truth) to be much more careful than they had been before in distinguishing between theory and metatheory and between syntax and semantics; we may therefore forgive Gödel's contemporaries for being careless on this. Unlike Zermelo and Perelman, however, Wittgenstein knowingly refused several aspects of such distinctions. During his entire philosophical career he never had second thoughts on his rejection of Hilbert's metamathematics. This is expressed in the Philosophical Remarks and, most explicitly, in a paragraph of the Philosophical Grammar whose title is precisely "There is no metamathematics":

> I said earlier "calculus is not a mathematical concept"; in other words, the word "calculus" is not a chess piece that belongs to mathematics.
>
> There is no need for it to occur in mathematics. – If it is used in a calculus nonetheless, that doesn't make the calculus into a metacalculus; in such a case the word is just a chessman like all the others. Logic isn't metamathematics either; that is, work within the logical calculus can't bring to light essential truths about mathematics. Cf. here the "decision problem" and similar topics in modern mathematical logic. [. . .]
>
> (Hilbert sets up rules of a particular calculus as rules of metamathematics) (Wittgenstein 1953, pp. 296–297.)

That is to say: Hilbert's metamathematics is, in fact, nothing but mathematics. It is not a metacalculus, because there are no metacalculi: it is just one more calculus.

It would take too much space here to discuss Wittgenstein's motivations for discarding Hilbert's conception of metamathematics. Roughly, they are closely connected to a rejection of the Platonic idea that mathematical sentences describe an independently existing domain—the "realm of numbers". If we follow this line, the claim that Gödel's proof actually is the derivation of a paradox follows ineluctably. Contrary to what Bernays claimed, the discussion of Gödel's results in the *Bemerkungen* does *not* "suffer from the defect that Gödel's quite explicit premises of the consistency of the considered formal system is ignored" (Bernays 1959, p. 523). Bernays' charge just begs the question against Wittgenstein, for the consistency of the relevant system is precisely what is called into question by Wittgenstein's reasoning. For a particularly clear statement of this issue, see Rodych (2002, pp. 384–385). Let us see why.

14.3 Prose vs. Proof

Here is, to begin with, a standard exposition of the First Incompleteness Theorem, which will help us in the following. An exemplary case of a theory to which Gödel's Theorems apply is provided by Peano arithmetic, PA.[2] This can be obtained by simply adding to the ordinary axioms of first-order predicate logic with identity the following principles:

(PA1) $\forall x(Succ(x) \neq 0)$
(PA2) $\forall xy(Succ(x) = Succ(y) \to x = y)$
(PA3) $\forall x(x + 0 = x)$
(PA4) $\forall xy(x + Succ(y) = Succ(x + y))$
(PA5) $\forall x(x \times 0 = 0)$
(PA6) $\forall xy(x \times Succ(y) = (x \times y) + x)$
(PA7) $\alpha[x/0] \to (\forall x(\alpha[x] \to \alpha[x/Succ(x)]) \to \forall x\alpha[x])$.

The theory holds in the so-called standard model of arithmetic (be it $\mathbb{N}$), i.e., the model constituted by natural numbers and the operations on them we know since we were children. Variables are therefore supposed to range on natural numbers,[3] and "0" is the name of number zero. The intended reading of the one-place functor $Succ(x)$ is "the (immediate) successor of x". Therefore, $Succ(0)$ is 1, that is, the (immediate) successor of zero in the series of natural numbers; $Succ(Succ(0))$ is 2, that is, the successor of 1; etc. $+$ and $\times$, of course, are read as addition and multiplication. Therefore, (PA1) claims that zero is the successor of no

[2]Wittgenstein's remarks had as their background system the one of Russell and Whitehead's *Principia mathematica* (with slight modifications). However, this is a minor point, and sticking to PA allows us to follow a standard way of presenting Gödel's Theorems.

[3]Or, at least, these are our bona fide intuitions when we formulate the theory. The existence of non-standard models shows that things are not so straightforward. I will come to this in a subsequent note.

number; (PA2) claims that if x and y have the same successor, they are the same number. (PA3)–(PA6) represent recursive equations characterizing addition and multiplication. (PA7) is the schematic formulation of the (mathematical) Induction Principle, which claims that if some $\alpha[x]$ holds for the zero and for the successor of a given number x for which it holds, then $\alpha[x]$ holds for all numbers.

Now the Gödelisation procedure allows one to associate a natural number to each symbol, formula and sequence of formulas of PA, so that one can always effectively move back and forth between an expression of the language of PA and the number to which it has been paired (its Gödel number). A k-ary relation (whose extension consists in a set of ordered k-ples) R can be said to be representable in PA iff there is a formula $\alpha[x_1, \ldots, x_k]$, such that, for any ordered k-ple of numbers $\langle n_1, \ldots, n_k\rangle$, we have that:

(a) If $\langle n_1, \ldots, n_k\rangle \in R$, then $\vdash_{PA} \alpha[x_1/\mathbf{n}_1, \ldots, x_k/\mathbf{n}_k]$
(b) If $\langle n_1, \ldots, n_k\rangle \notin R$, then $\vdash_{PA} \neg\alpha[x_1/\mathbf{n}_1, \ldots, x_k/\mathbf{n}_k]$,

where $\mathbf{n}$ is the numeral of number n, see Gödel (1931, p. 607). Now, PA is the typical case of a sufficiently strong theory, that is, it is capable of representing the (primitive) recursive functions. Recursive functions-relations have the role of codifying the syntax of the theory. Metalinguistic claims on PA are mirrored within the officially arithmetic language of PA. As is usually claimed, PA "can talk about" (Boolos et al. 2002, p. 187) some of its syntactic properties. In particular, the property of being a theorem of the theory can be (weakly) represented within the theory itself. The arithmetic predicate no. 45 in Gödel's paper corresponds to something like:

$$Prf(x, y)$$

whose reading via arithmetisation is: "x is (the Gödel number of) a proof of the formula (whose Gödel number is) y". *Prf* is a recursive relation that holds between those pairs of numbers which are, respectively, the Gödel number of a sequence of formulas of PA, and the Gödel number of a formula of PA, such that the former is a proof of the latter. Predicate no. 46 is defined by means of no. 45, thus:

$$Th(y) =_{df} \exists x\ Prf(x, y);$$

therefore, it holds of those numbers which are the Gödel numbers of formulae of PA for which there is a proof in PA.[4] The fundamental condition to prove Gödel's First Theorem concerns provability within PA:

$$\text{(P)}\ \vdash_{PA} \alpha \Rightarrow \vdash_{PA} Th(\ulcorner\alpha\urcorner),$$ [5]

that is, if α is a theorem of PA, then the formula mirroring this fact within PA is itself a theorem of PA. Now, before the Fixed Point Lemma was employed to obtain

[4] *Th* is not recursive, but semirecursive (see Gödel 1931, p. 606); however, this is of no importance here.

[5] $\ulcorner\alpha\urcorner$ is the numeral of the Gödel number of α.

fully formalised Liar sentences, Gödel used it to build a sentence (be it γ) attributing to itself not falsity, but non-theoremhood:

$$(\text{FP}_\gamma)\gamma \leftrightarrow \neg Th(\ulcorner\gamma\urcorner).$$

γ is a purely arithmetic sentence, but its informal reading via Gödelisation is: "I am not a theorem". Given the definition above, it is equivalent to $\neg\exists x \ \ Prf(x, \ulcorner\gamma\urcorner)$, that is, "I am not provable".

We have to assume, then, that PA is both consistent and ω-consistent (a system is called ω-consistent iff for no formula $\alpha[x]$ of its language it is possible to prove both $\neg\alpha[x/\mathbf{n}]$ for each natural n, and $\exists x \ \alpha[x]$). Gödel demonstrated that:

1. If PA is consistent, then $\nvdash_{PA} \gamma$;
2. If PA is ω-consistent, then $\nvdash_{PA} \neg\gamma$.

As for (1): if γ were a theorem of PA then, given (P), also $Th(\ulcorner\gamma\urcorner)$ would be. Hence, given (FP_γ), the provability of $\neg\gamma$ would follow. We would have, then, $\vdash_{PA} \gamma$ and $\vdash_{PA} \neg\gamma$, against the assumption that PA is consistent. As for (2): since the proof relation of PA is (primitive) recursive, we have that for each n either $\vdash_{PA}$ $Prf(\mathbf{n}, \ulcorner\gamma\urcorner)$, or $\vdash_{PA} \neg Prf(\mathbf{n}, \ulcorner\gamma\urcorner)$. The former case is ruled out by the fact that, as (1) claims, γ is not provable—therefore, for each n it is not the case that n is the code of a proof of γ in PA. Hence, the latter case holds. It follows, given the assumption that PA is ω-consistent, that $\exists x \ Prf(x, \ulcorner\gamma\urcorner)$ is not a theorem. But $\exists x \ Prf(x, \ulcorner\gamma\urcorner)$ is nothing but $\neg\gamma$ (see e.g., Smullyan 1992, Chap. V; Boolos et al. 2002, pp. 225–227). The conjunction of (1) and (2) gives us Gödel's First Incompleteness Theorem. This tells us that Peano arithmetic includes a sentence, γ (its own Gödel sentence), which is undecidable within PA, that is, not provable and not refutable.[6]

[6]One of the consequences of Gödel's First Theorem is that (first-order) PA is not, as model theorists say, categorical. This means that from Gödel's results follows the existence of non-standard models of PA, structurally different from $\mathbb{N}$. In particular, there is no way to constrain the variables of the theory so that they range exclusively on ordinary natural numbers. In 1957, Goodstein had already claimed that "Wittgenstein with remarkable insight said in the early thirties that Gödel's results showed that the notion of a finite cardinal could not be expressed in an axiomatic system and that formal number variables must necessarily take values other than natural numbers" (Goodstein 1957, p. 551). More recently, Floyd and Putnam have credited the "notorious paragraph" 8 of the Appendix 1 to Part I of Wittgenstein's *Bemerkungen* with a "philosophical claim of great interest" precisely on the role of non-standard models and ω-inconsistency. The claim is to the effect that "if one assumes (and, a fortiori, if one actually finds out) that $\neg P$ [where P is assumed to be the Gödel sentence of the relevant system] is provable in Russell's system one should (or, as Wittgenstein actually writes, one 'will now presumably') give up the 'translation' of P by the English sentence 'P is not provable'" (Floyd and Putnam 2000, p. 625). The point is that if a theory proves $\neg P$ (which may be obtained simply by adding it as an axiom), then it is ω-inconsistent, but consistent. Being consistent, it is supposed to have a model. However, being ω-inconsistent, its model has to be structurally different from the standard model of arithmetics, $\mathbb{N}$. It is a non-standard model, and the "translation" of P as "P is not provable" becomes untenable in this context.

So far, semantics and truth have not poked their nose in the proof[7] which, assuming only the consistency (and ω-consistency) of the system, counts as what logicians usually call a standard "syntactic" one. However, the exposition of the First Theorem usually goes hand in hand with the following short story, which Wittgenstein would probably have labelled as the "prose": since γ claims (via arithmetisation) to be not provable, and we have just proved that it is not provable, then γ just is what it claims to be; hence, it is true. However, this simple reasoning cannot be performed within the theory: the truth predicate for PA, were it expressible within PA, under the usual conditions would originate the Liar paradox; whereas the provability predicate is expressible. Gödel himself pointed at the analogies between his undecidable sentence and such paradoxes as Richard's, or the Liar, see Gödel (1931, p. 598). However, it seems clear that, whereas the Liar sentence, "This sentence is false", produces an antinomy, with the Gödel sentence, metamathematically read as "This sentence is not provable", no contradiction is forthcoming. Or so the usual story goes.[8]

It is often concluded, then, that Gödel's First Theorem establishes a fundamental gap between provability and truth (if a formal system has to be correct, i.e., it must capture only arithmetical truths, then it cannot capture them all). Precisely because of this, it has been taken by some as a keystone of mathematical realism, on the basis of an interpretation encouraged by Gödel himself. Gödel's realist stance emerged, as is well known, only several years after his 1931 paper, mainly in *What is Cantor's Continuum Problem?* (Gödel 1947). Nevertheless, he declared that his mathematical Platonism had been the heuristic key for the discovery of incompleteness, see Feferman (1983). And this is how the Gödelian results have become "one of the great moving forces behind the modern resurgence of Platonism" (Shanker 1988, p. 171).

Now, it is precisely this semantic outcome of Gödel's proof that Wittgenstein challenged as the "prose" (as opposed to the real "proof"). However, this should not be understood as the thesis that the First Theorem shows only the fact that γ is not a theorem of PA, whereas the further semantic conclusion that (if the system is consistent, then) γ is also true would be a "metaphysical claim" (Floyd and Putnam 2000, p. 632). As a matter of fact, quite legitimate and respectable semantic versions of Gödel's result are available, see e.g., Smullyan (1992, Chaps. 3 and 4). This is a minor point with respect to our discussion, though, because two other and quite different aspects of the semantic prose were unacceptable to Wittgenstein: (1) the idea that sentence γ, which is syntactically undecidable within PA, can nevertheless—as is usually said, "with a wave of hands" (Priest 1979, p. 222)—be shown to be true (to be sure, under the hypothesis of the consistency of PA) on the

[7]An anonymous referee has appropriately pointed out to me.

[8]In Kleene's words: "Gödel's sentence 'I am unprovable' is not paradoxical. We escape paradox because (whatever Hilbert may have hoped) there is no *a priori* reason why every true sentence must be provable [...]. The sentence $A_p(\mathbf{p})$, which says 'I am unprovable', is simply unprovable and true" (Kleene 1976, p. 54).

basis of a *meta*theoretic argument conducted "outside" the formal system PA; and (2) the consequent, aforementioned discrepancy between provability in any system capable of expressing elementary arithmetic, and arithmetical truth.

1. As for the first point: the semantic prose is sometimes to the effect that γ is proved by means of an informal or "intuitively correct" argument. One may say, with a little more precision, that it is provable within a theory that can deal with the semantics via the notion of truth (for the language of PA), which is not definable, given Tarski's theorem, within PA. If one asks "how is [γ]'s truth established? The answer is: by a metamathematical *proof* of [γ]" (Routley 1979, p. 325), that is, by means of a *detour* through the metatheory. This was stated by Gödel in the opening paragraphs of his paper, where he declared that "the proposition that is undecidable *in the system PM* still was decided by metamathematical considerations" (Gödel 1931, p. 599).

It was probably this claim that initially perplexed Wittgenstein, for in the *Philosophical Remarks* he had already observed:

> What is a proof of provability? It's different from the proof of proposition.
>
> And is a proof of provability perhaps the proof that a proposition makes sense? But then, such a proof would have to rest on *entirely different* principles from those on which the proof of the proposition rests. There cannot be a hierarchy of proofs!
>
> On the other hand there can't in any fundamental sense be such a thing as meta-mathematics. Everything must be of one type (or, what comes to the same thing, not of a type). [...]
>
> Thus, it isn't enough to say that p is provable, what we must say is: provable according to a particular system.
>
> Further, the proposition doesn't assert that p is provable in the system S, but in *its own* system, the system of p. That p belongs to the system S cannot be asserted, but must show itself.
>
> You can't say p belongs to the system S; you can't ask which system p belongs to; you can't search for the system of p. Understanding p means understanding its system. If p appears to go over from one system into another, then p has, in reality, changed its sense. (Wittgenstein 1953, p. 180).

Within this framework, it is not possible that the very same sentence (say, γ), turns out to be expressible, but undecidable, in a formal system (say, PA), and demonstrably true (under the aforementioned consistency hypothesis) in a different system (the meta-system). If, as Wittgenstein maintained, the proof establishes the very meaning of the proved sentence, then it is not possible for *the same* sentence (that is, for a sentence with the same meaning) to be undecidable in a formal system, but decided in a different system (the meta-system).

2. As for the second point: following this general doctrine, Wittgenstein had to reject both the idea that a formal system can be syntactically incomplete, and the Platonic consequence that no formal system proving only arithmetical truths can prove all arithmetical truths. If proofs establish the meaning of mathematical sentences, then there cannot be incomplete systems, just as there cannot be incomplete meanings:

> The edifice of rules must be *complete*, if we are to work with a concept at all – *we cannot make any discoveries in syntax*. – For, only the group of rules *defines* the sense of our signs, and any alteration (e.g., supplementation) of the rules means an alteration of the sense. [. . .]
>
> Mathematics cannot be incomplete; any more than a *sense* can be incomplete. (Wittgenstein 1953, pp. 182, 188).

One may object that Wittgenstein here is collapsing different levels again: he is confusing a theory with what the theory describes. According to the Platonic interpretation of the incompleteness results, it is not arithmetic, in the sense of the "realm of natural numbers", which is incomplete. If we are Platonists, as Gödel certainly was, we will take the "realm of numbers" as perfectly complete, with its properties distributed in a maximal and consistent way among numbers. It is just that this realm cannot be fully captured by any formal system. Formalised arithmetic is incomplete; not the arithmetic reality (say, the standard model $\mathbb{N}$), which the theory was supposed to describe.

However, Wittgenstein intentionally opposed precisely this referential picture of mathematics, according to which the meaning of mathematical sentences consists in their referring to, and describing, an independently existing reality—the picture of "arithmetic as the natural history (mineralogy) of numbers", of which "our whole thinking is penetrated" (Wittgenstein 1953, p. 116e). According to him, the meaning of a mathematical sentence is determined by the rules that govern its use in the calculus and in particular by its own proof (which is why an incompleteness in the theory would become *eo ipso* an incompleteness of meaning):

> A psychological disadvantage of proofs that construct *propositions* is that they easily make us forget that the sense of the result is not to be read off from this by itself, but from the *proof*. [. . .] I am trying to say something like this: even if the proved mathematical proposition seems to point to a reality outside itself, still it is only the expression of acceptance of a new measure (of reality). (Wittgenstein 1953, pp. 76e–77e).

Consequently, also the Platonic separation between provability and truth has to go. The remarks on the First Incompleteness Theorem in the *Bemerkungen* are resolute on this point:

> 5. Are there true propositions in Russell's system, which cannot be proved in his system? – What is called a true proposition in Russell's system, then?
>
> 6. For what does a proposition's '*being true*' mean? *'p' is true* = *p*. (that is the answer). (Wittgenstein 1953, p. 50e)

Here Wittgenstein seems to be identifying (mathematical) truth with assertability (see Rodych 1999, pp. 178–179). Therefore, he concludes:

> If, then, we ask in this sense: "Under what circumstances is a proposition asserted in Russell's game" the answer is: at the end of one of his proofs [i.e., as a theorem], or as a 'fundamental law' (Pp.) [i.e., as an axiom – and, of course, axioms are theorems]. There is no other way in this system of employing asserted propositions in Russell's symbolism.
>
> 7. "But may there not be true propositions which are written in this symbolism, but are not provable in Russell's system?" – "True propositions", hence propositions which are true in *another* system, i.e., can rightly be asserted in another game. [. . .] [A] proposition which cannot be proved in Russell's system is "true" or "false" in a different sense from a proposition of *Principia mathematica*. (Wittgenstein 1953, p. 50e)

In the end, "'True in Russell's system' means, as was said: proved in Russell's system; and 'false in Russell's system' means: the opposite has been proved in Russell's system" (Wittgenstein 1953, p. 51e).[9] By identifying truth and provability, and by rejecting the very idea of metamathematics, Wittgenstein was opposing some established results of contemporary logic—or, better, of contemporary *classical* mathematics and *classical* logic (whereas his position has often been connected, e.g., by Dummett (1959, pp. 504–505), Bernays (1959, p. 519) and Kielkopf (1970), and others, to a strong mathematical constructivism and to the so-called "strict finitism"). This speaks against Wittgenstein's own claim, according to which "it is my task, not to attack Russell's logic from within, but from without", and "my task is not to talk about (e.g.) Gödel's proof, but to pass it by" (Wittgenstein 1953, p. 174e). As I hinted at, however, it is possible to introduce a single argument that, by reinterpreting Gödel's results in the light of Wittgenstein's general standpoint, gives to the latter an unexpected plausibility precisely from the point of view of modern *non*-classical mathematical logic. Let's have a look.

14.4 Paraconsistency to the Rescue

My strategy exploits an argument proposed by Richard Routley and Graham Priest's various influential essays (Routley 1979; Priest 1979, 1984, 1987). It has not been developed having Wittgenstein in mind,[10] but it allows us to interpret Gödel's proof precisely as a paradoxical derivation. The core idea is to see what happens when one tries to apply the First Incompleteness Theorem to the theory that captures *our intuitive, or naïve, notion of proof.*

By "naïve notion of proof" Routley and Priest apparently mean the one underlying ordinary mathematical activity: "proof, as understood by mathematicians (not logicians), is that process of deductive argumentation by which we establish certain mathematical claims to be true" (Priest 1987, p. 40). Since Hilbert, formal logicians have learned to treat proofs as purely syntactic objects: sequences of strings of symbols, manipulated via transformation rules, etc. However, *proving* something, for a working mathematician, amounts to establishing that some sentence is *true*.

Now, when we want to settle the question whether some mathematical sentence is true or false, we try to deduce it, or its negation, from other mathematical sentences which are already known to be true. The process cannot go backwards *in infinitum*, though. We should therefore reach, eventually, mathematical sentences known to be true without having to be proved—e.g., because they are "self-evident". However,

[9]That at the core of Wittgenstein's rejection of the Platonistic "prose" associated to Gödel's proof is his identification of truth with provability, has been argued in detail by Rodych and Shanker in various essays (see Rodych 1999, 2003; Shanker 1988).

[10]In particular, Priest may disagree with the picture of Wittgenstein's attitude towards Gödel proposed here (see Priest 2004).

this is not important (nor is it important to establish *which* are the primal truths; concerning arithmetic, they may be, for instance, principles such as those of Peano, that is, claims according to which every number has a successor, etc.).

Given this characterisation, it is clear that the naïve-intuitive theory Routley and Priest link to the naïve-intuitive notion of proof is rather informal. However, "it is accepted by mathematicians that informal mathematics could be formalised if there were ever a point to doing so, and the belief seems quite legitimate" (Priest 1987, p. 41). Admittedly, this is a step the so-called second Wittgenstein, who disliked formalisations, may have questioned:

> The curse of the invasion of mathematics by mathematical logic is that now any proposition can be represented in a mathematical symbolism, and this makes us feel obliged to understand it. Although of course this method of writing is nothing but the translation of vague ordinary prose. (Wittgenstein 1953, p. 155e)

However, we may reasonably assume that, when Wittgenstein made such claims, he was not questioning formalisation itself, but the overwhelming importance attributed to it by philosophers and logicians looking for the "ideal language". On the contrary, we are now assuming precisely that formalisation is nothing but the "translation of vague ordinary prose": one may regiment the fragment of English in which the naïve theory is expressed, and turn it into a formal language. Then, the primal truths may be written down in the (now) formalised language and taken, say, as axioms; and proofs may be expressed as formal arguments. Priest also claims that, after having been so translated, the naïve theory would certainly be sufficiently strong in the sense explained above, i.e., capable of representing all the (primitive) recursive functions.

Is the naïve notion of proof decidable? This is much less straightforward, and it is likely that the crux of the argument lies here. To assume that the proof relation of naïve arithmetic is decidable challenges the standard perspective, taken as established precisely by Gödel's results. I will come back to this point, though, after exposing the paraconsistent argument, which goes as follows.

Let T be the formalisation of our naïve, intuitive mathematical theory. Assuming that T, just like PA, is sufficiently strong, *if* T is consistent, then Gödel's First Theorem applies: so there is a sentence ϕ which is not a theorem of T, but which can be established as true via a naïve proof, and therefore *is* a theorem of T. Of course, anything that is naïvely-intuitively provable is provable within the naïve-intuitive theory. So "assuming its consistency, it would, therefore, seem to be both complete and incomplete in the relevant sense" (Priest 1984, p. 165). Now we have no way to avoid a paradox: either we accept this one, i.e., $\vdash_T \phi$ and $\nvdash_T \phi$ (which is quite close to Wittgenstein's remark, quoted at the beginning of this paper: "let us suppose I prove the unprovability (in Russell's system) of P; then by this proof I have proved P. Now if this proof were one in Russell's system—I should in this case have proved at once that it belonged and did not belong to Russell's system"); or we have to admit that our naïve mathematical theory, with its naïve notion of proof, is such that the Gödel sentence ϕ for the (formalisation of the) naïve theory can be proved within T itself, together with its negation—so one of the inconsistencies hosted by T is to the effect that $\vdash_T \phi$ and $\vdash_T \neg\phi$.

The philosophical point is that "This sentence is not provable" now has its "provable" understood as meaning "demonstrably true", and, as Wittgenstein conjectured, Gödel's proof becomes the derivation of a real paradox:

> In fact, in this context the Gödel sentence becomes a recognisably paradoxical sentence. In informal terms, the paradox is this. Consider the sentence "This sentence is not provably true". Suppose the sentence is false. Then it is provably true, and hence true. By *reductio* it is true. Moreover, we have just proved this. Hence it is provably true. And since it is true, it is not provably true. Contradiction. This paradox is not the only one forthcoming in the theory. For, as the theory can prove its own soundness, it must be capable of giving its own semantics. In particular, [every instance of] the T-scheme for the language of the theory is provable in the theory. Hence [. . .] the semantic paradoxes will all be provable in the theory. Gödel's "paradox" is just a special case of this (Priest 1987, pp. 46–47; see also Priest 1984, p. 172).

Therefore, Anderson's comment on Wittgenstein, according to which "the conclusion to draw would not be that P at once 'belonged and did not belong' to Russell's system, but rather that Russell's system was inconsistent" (Anderson 1958, p. 458), is really of little importance: either horn of the dilemma makes us end up in a contradiction; and, as we shall see very soon, both contradictions (i.e., a system proving both its Gödel sentence and its negation, and a system both proving and not proving something) are expected in a thoroughly paraconsistent framework, as is shown in (Priest 1987, pp. 239–243).

I claimed that the "semantic prose" on the First Theorem attacked by Wittgenstein has it that the truth of the Gödel sentence is established in the metatheory (under the assumption that the theory is consistent): it can be proved in a metatheoretic context in which we can deal with the semantics of the object theory, i.e., with the truth predicate for (the language of) the object theory. However, T, formalizing as it does our naïve notion of proof, should absorb the metatheory within the theory. After all, as Wittgenstein might have added, mathematicians use ordinary English, and ordinary English may well be (and, according to many philosophers of language, actually is) semantically closed. As Routley has stressed, "everyday arithmetic as presented within a natural language like English appears, unlike say first-order Peano arithmetic, appropriately closed". And "is provable in arithmetic" and "is arithmetically true" are "English, and in a good sense arithmetical, predicates" (Routley 1979, p. 326). So T is semantically closed in the Tarskian sense, and inconsistent. The reasoning behind the proof of the truth of the Gödel sentence is now performed *within* the formal system itself—which is what we should expect in a Wittgensteinian framework that collapses, in the aforementioned sense, the distinction between theory and metatheory. There is no metasystem in which one establishes that (if the object system is consistent, then) the Gödel sentence is true: there are no metasystems. Consequently, one cannot "get out" of a system and solve, in its metasystem, problems that were meaningfully expressible but undecidable within the system.

Now back to the key assumption that the naïve notion of proof is effectively decidable (thus, given Church's Thesis, recursive). The first thing to notice in this respect is that this may well have been Wittgenstein's assumption, too. As we have already hinted at, Wittgenstein believed that the naïve (i.e., the working mathematician's) notion of proof had to be decidable, for lack of decidability meant to him simply lack of mathematical meaning: Wittgenstein believed that everything had to be decidable in mathematics, so the argument coheres with Wittgenstein's position on this point, too. But Routley and Priest also have positive arguments for the view. That the naïve notion of proof is decidable means that we can in principle effectively recognise a naïve proof when we see one. Now, Priest stresses, "it is part of the very notion of proof that a proof should be effectively recognizable as such" (Priest 1987, p. 41)— for the point of a naïve proof is that it is a way of settling the issue whether a given mathematical claim is true or not. As Alonzo Church claims:

> Consider the situation which arises if the notion of proof is non-effective. There is then no certain means by which, when a sequence of formulas has been put forward as a proof, the auditor may determine whether it is in fact a proof. Therefore he may fairly demand a proof, in any given case, that the sequence of formulas put forward is a proof; and until the supplementary proof is provided, he may refuse to be convinced that the alleged theorem is proved. This supplementary proof ought to be regarded, it seems, as part of the whole proof of the theorem... (Church 1956, p. 53)

Besides, by acknowledging that the naïve proof relation is decidable we can explain how we *learn* arithmetic—that is, via an effective procedure:

> We appear to obtain our grasp of arithmetic by learning a set of basic and effective procedures for counting, adding, etc.; in other words, by knowledge encoded in a decidable set of axioms. If this is right, then arithmetic truth would seem to be just what is determined by these procedures. It must therefore be axiomatic. If it is not, the situation is very puzzling. The only real alternative seems to be Platonism, together with the possession of some kind of sixth sense, "mathematical intuition". (Priest 1994, p. 343)

This point, too, meets some Wittgensteinian concerns on teaching and learning mathematical calculi as a public, social phenomenon. Perhaps the most amazing fact about mathematics as a discipline is the unanimity (generally speaking) of mathematicians on what counts as a proof. As Wittgenstein remarked, the whole "language game" of mathematical proofs would be rendered impossible by lack of consensus among mathematicians. If the notion of arithmetic proof were not effectively recognizable, then the process whereby mathematics is learnt, and the general agreement of working mathematicians on what counts as a mathematical proof, would turn out to be a mystery (of course, this is but a particular case of a famous, more general argument to the effect that *language* can only be learnt recursively, and so the grammar of a learnable language must be generated by a decidable set of rules), on which see, famously, Davidson (1984, Chap. 1). On the contrary, as Routley claims, if the truths of mathematics are effective or effectively enumerable we can understand "how one generation of mathematicians learns what counts as true from the previous generation, namely they learn certain basic mathematical truths and how to prove others by making deductions" (Routley 1979, p. 327).

Of course, one can speak against the decidability of the naïve notion of proof on the basis of Gödel's results themselves. But one may argue that, in the context, this would beg the question against paraconsistentists—and against Wittgenstein, too. Both Wittgenstein and the paraconsistentists, on one side, and the followers of the standard view on the other, agree on the following thesis: the decidability of the notion of proof and its consistency are incompatible. But to infer from this that the naïve notion of proof is not decidable invokes the indispensability of consistency, which is exactly what Wittgenstein and the paraconsistent argument call into question. Contrary to what Bernays claimed, the discussion in the *Bemerkungen* does not "suffer from the defect that Gödel's quite explicit premise of the consistency of the considered formal system is ignored" (Bernays 1959). Bernays' charge just begs the question against Wittgenstein, for, as Victor Rodych has forcefully argued, the consistency of the relevant system is precisely what is called into question by Wittgenstein's reasoning (see Rodych 2002, pp. 384–385).

14.5 Paraconsistent Arithmetic

One may wonder how can Wittgenstein's position be made more palatable from a logical point of view by referring it to an inconsistent theory. It is easy to see how audacious the argument itself is: by turning Gödel's proof into a paradox, it places inconsistencies at the very core of (the theory which, supposedly, captures) our mathematical practice. This is not so straightforward, though, if one does not believe, unlike Wittgenstein's early commentators, that contradictions immediately make formal systems uninteresting. Here comes into play the aspect of Wittgenstein's philosophy of mathematics, mentioned at the beginning of this paper, which my interpretation can recapture: his attitude towards contradictions.

That Wittgenstein did not consider the surfacing of contradictions within formal systems as a terrible crisis is well known and testified, for instance, by his discussions with Turing on this point, as reported in the *Lectures on the Foundations of Mathematics*. It is true that Wittgenstein did not comment directly on Gödel's Second Incompleteness Theorem. However, he often commented on the role and the importance of consistency proofs; and his position was clear-cut—he considered this kind of proof as a symptom of "the superstitious fear and awe of mathematicians in face of contradiction" (Wittgenstein 1953, p. 53e)

> And if they now demand a proof of consistency, because otherwise they would be in danger of falling into the bog at every step – what are they demanding? Well, they are demanding a kind of *order*. But was there *no* order before? – Well, they are asking for an order which appeases them now. – But are they like small children, that merely have to be lulled asleep? (Wittgenstein 1953, p. 101e)

After interpreting Gödel's proof as a paradox closely related to the Liar, Wittgenstein asks, rhetorically: "but there is a contradiction here!—Well, then there is a contradiction here. Does it do any harm here?"; "'perhaps', Wittgenstein might say, 'all calculi that admit such sentence-constructions are syntactically inconsistent"' (Rodych 1999, p. 190), but he believed that a calculus within which one can derive a contradiction does not thereby become useless:

> Can we say: "Contradiction is harmless if it can be sealed off"? But what prevents us from sealing it off? [. . .]
>
> Let us imagine having been taught Frege's calculus, contradiction and all. But the contradiction is not presented as a disease. It is, rather, an accepted part of the calculus, and we calculate with it. [. . .]
>
> For might we not possibly have wanted to produce a contradiction? Have said – with pride in a mathematical discovery: "Look, this is how we produce a contradiction"? [. . .]
>
> My aim is to alter the attitude to contradiction and to consistency proofs. (Not to show that this proof shows something unimportant. How could that be so?). (Wittgenstein 1953, pp. 104e–106e)

Because of these insights, Wittgenstein has been considered a precursor of paraconsistent logics. He anticipated the intuition that an inconsistent calculus does not thereby become trivial and uninteresting; on this point, see Marconi (1984):

> "Contradiction destroys the calculus" – what gives it this special position? With a little imagination, I believe, it can certainly be demolished. [. . .]
>
> And suppose the contradiction [i.e., Russell's paradox] had been discovered but we were not excited about it, and had settled e.g., that no conclusions were to be drawn from it. (Wittgenstein 1953, p. 170e)

Now, if we adopt a paraconsistent logic the theory T mentioned above, which is claimed to capture our naïve-intuitive notion of proof, is not just an argumentative trick anymore. It is possible to provide a respectable logical framework for Wittgenstein's idea according to which Gödel's proof is paradoxical, and nevertheless the derivation of such paradoxes does not render the relevant system(s) useless. Inconsistent arithmetics, i.e., non-classical arithmetics based on a paraconsistent logic, are nowadays a reality. What is more important, the theoretical features of such theories match precisely with some of the aforementioned Wittgensteinian intuitions. Let us see some examples.

First, paraconsistent arithmetics do not fulfil precisely the consistency requisite. This suggests that such theories could emancipate themselves from Gödel's Theorems, and from other limitative results afflicting their consistent cousins based upon a more traditional (classical, or intuitionistic) logic. To be sure, consistency proofs are not at issue, since we are dealing with inconsistent theories. What the theory may hopefully prove, though, is its own non-triviality, which in these contexts is more often called absolute consistency.

Paraconsistent authors have begun to show that this is the case since the 1970s, by building inconsistent but non-trivial theories, whose non-triviality proof can be represented within the very theories and that, in this sense, circumvent Gödel's Second Theorem. Their inconsistency allows them to escape also from Gödel's First Theorem, and from Church's undecidability result: they are, that is, demonstrably complete and decidable.[11] They therefore fulfil precisely Wittgenstein's request, according to which there should not be mathematical problems that canbe

[11] For a quick review, see Bremer (2005, Chap. 13).

meaningfully formulated within the system, but which the rules of the system cannot decide. Hence, the decidability of paraconsistent arithmetics harmonises with an opinion Wittgenstein maintained throughout his philosophical career.

Besides, the perspective of inconsistent arithmetics is (typically, though not necessarily) involved in a form of strict *finitism*. The underlying intuition would be that there is a finite (albeit hardly imaginable and unknown to us) number of things in the world. Although we cannot specify the number, we know that it must be "a number larger than the number of combinations of fundamental particles in the cosmos, larger than any number that could be sensibly specified in a lifetime" (Priest 1994, p. 338) (which should explain why our intuitions on it are rather unconfident); and this largest number is an inconsistent number.[12]

We can get into the details by considering a simple case of inconsistent arithmetic. Suppose n is our largest-inconsistent number. Let N be the theory of $\mathbb{N}$, that is, the set of arithmetic sentences true in the standard model $\mathbb{N}$; and let M_n be the set of sentences true in the paraconsistent model with the inconsistent number n. We may take as the underlying logic of M_n some mainstream paraconsistent logic, such as LP (Priest's logic of paradox), or FDE (Belnap and Dunn's First Degree Entailment). Now, according to Priest (1994) such a theory as M_n has the following enjoyable properties: it is, of course, inconsistent (including, among other things, both its own Gödel sentence and its negation), but provably non-trivial—and its non-triviality proof can be formalised within it. It fully contains N, that is, it includes all the sentences true in the standard model. Finally, M_n includes its own truth predicate. Therefore, the inconsistent arithmetic avoids Gödel's First Incompleteness Theorem; it also avoids the Second Theorem, in the sense that its non-triviality can be established within the theory; and Tarski's Theorem, too—including its own predicate is not a problem for an inconsistent theory.[13]

This is more than enough to get interested in the paraconsistent model of M_n. How is it like? The model can be obtained by applying to $\mathbb{N}$ an appropriate filter that reduces its cardinality. Meyer and Mortensen have initially developed the technique, and some of their main results are summarised in Meyer and Mortensen (1984) that appeared in the *Journal of Symbolic Logic*, in which different finite models are considered. The filter works as follows: let D be the domain of a given model $\mathbb{M}$, and $\approx$ an equivalence relation defined on D, which is also a congruence with respect to the denotations of the function symbols of the language. Given the objects $o_1, \ldots, o_n$ belonging to D, $|o_1|, \ldots, |o_n|$ are the corresponding equivalence classes under $\approx$. Now, let $\mathbb{M}^{\approx}$ be the new model, called the collapsed model, whose domain is $\mathrm{D}^{\approx} = \{|o| \mid o \in D\}$. The role of $\mathbb{M}^{\approx}$ is to provide substitutes for the initial objects, and particularly to identify the members of D in each equivalence class, thereby

[12] Such a strict finitism is not unavoidably tied to inconsistency, nonetheless: van Bendegem (1994, 1999) has exploited the properties of paraconsistent arithmetical models to argue for a greatest number, which is not an inconsistent one.

[13] For a detailed account of these facts, see Priest (1994, pp. 337–338) and Priest (1987, pp. 234–237).

producing a composite object that "inherits" the properties of its components: the predicates that were true of the initial objects now apply to the substitute. Now, by induction over the complexity of formulas it is possible to prove the following lemma, called the Collapsing Lemma:

(CL) Given any formula α which has the truth value v in $\mathbb{M}$, α has the truth value v also in $\mathbb{M}^{\approx}$.[14]

Therefore, if the original model satisfied some set of formulas, the collapsed model also satisfies it: when the initial model $\mathbb{M}$ is collapsed into $\mathbb{M}^{\approx}$, no sentence loses a truth value—it can only gain them. Of course, when we begin with the model of a standard theory, the only values around are true and false. But in the collapsed model it may be the case that a formula, which was initially true only, or false only, becomes both true and false (and this, of course, is not a problem within such paraconsistent logics as LP or FDE). This happens when the collapsing filter produces an inconsistent object: for instance, it may identify in an equivalence class two initial objects, one of which had, whereas the other did not have, the very same property. The procedure works even if among the relevant sentences we have formulas that seem to put constraints on cardinality, such as $\exists xy(x \neq y)$, precisely because they can become paradoxical.

In the particular case of M_n, the trick consists in choosing for $\mathbb{N}$ a filter that (a) given a number $x < n$, puts x and nothing else in the corresponding equivalence class, so that $|x|$ inherits all and only the properties of x; and (b) puts every number $y \geq n$ in a single equivalence class. Consequently, all the true/false equations involving any number smaller than n in the standard model are now true only/false only of the substitute. Because of this, the initial segment in the succession (which is sometimes called the *tail*) behaves as usual. Roughly, "up to n" things work like in ordinary arithmetic. Nevertheless, anything that could be truly/falsely claimed of anything bigger than n is now true/false of the inconsistent number. Many things concerning it are therefore paradoxical now (both true and false), and "of course, n is [now] an inconsistent object [...]. In particular, in the model $\mathbf{n} = \mathbf{n} + 1$ is true even though it is also false" (Priest 1994, p. 338), so n is the successor of itself.

Priest has declared that (CL) is "the ultimate downwards Löwenheim-Skolem Theorem" (Priest 1994, p. 339), which is easy to understand. The downward half of the Löwenheim-Skolem Theorem claims that any first-order theory, with a model with an infinite domain has a model with a denumerably infinite domain, too.[15] The filter and the Collapsing Lemma allow us to "shrink" even more, since one can reduce a model with a denumerably infinite domain into one of any smaller size. We can have a collapsed model, $\mathbb{M}^{\approx}$, whose domain, $D^{\approx}$, has cardinality k (smaller than that of the initial model), by choosing an appropriate equivalence relation

[14] See Priest (1994, pp. 346–347); the result was anticipated in Dunn (1979).

[15] One of the consequences of the downward Theorem is the so-called Skolem paradox. Since set theory can be expressed in a first-order language, it has a model whose domain has the cardinality of the set of natural numbers. However, within set theory we can prove the existence of sets whose cardinality is more than denumerable.

that produces precisely k equivalence classes. Bremer has therefore suggested the following Paraconsistent Löwenheim-Skolem Theorem: "Any mathematical theory presented in first order logic has a *finite* paraconsistent model" (Bremer 2005, p. 155).

Now this strong finitism also meets a persistent tendency in Wittgenstein's philosophy of mathematics. Wittgenstein always showed a suspicious attitude towards Cantor's paradise and the non-denumerable infinities which, in Cantor's Platonistic view, were to be discovered by the diagonal argument. Of course, strict finitism and the insistence on the decidability of any meaningful mathematical question go hand in hand. As Rodych has remarked, the intermediate Wittgenstein's view is dominated by "his finitism and his [...] view of mathematical meaningfulness as algorithmic decidability", according to which "[only] finite logical sums and products (containing only decidable arithmetic predicates) *are* meaningful because they are *algorithmically decidable*". But this tendency remains also in the later phase: "as in the middle period, the later Wittgenstein seems to maintain that an expression is a meaningful proposition only *within* a given calculus, and *iff* we knowingly have in hand an applicable and effective DP [decision procedure] by means of which we can decide it" (Rodych 1999, pp.174–176).

14.6 Conclusion: The Costs and Benefits of Making Wittgenstein Plausible

The cost of accepting paraconsistent arithmetics is clear: we have to revise some well-established acquisitions of classical mathematical logic. As I claimed before, by subscribing to such a way of bringing up-to-date Wittgenstein's philosophy of mathematics one will not be allowed to claim—as many commentators did—that such a philosophy does not require any logico-mathematical revisionism, being directed only against the foundational demands of philosophers.

On the other hand, Wittgenstein might have found the situation produced by paraconsistent arithmetics quite plausible. Surprising and (in a broad sense) paradoxical innovations in the history of mathematics— this "motley of techniques of proof" (Wittgenstein 1953, p. 84e)—led to the invention of new kinds of numbers: from Hyppasus' irrational numbers refuting Pythagorism, to infinitesimals, Cantor's transfinite numbers, and all that. The early reception of such new entities among mathematicians has always been controversial, from the Pythagoreans condemning and expelling Hyppasus, to Kronecker making Cantor's life impossible. A process of rethinking mathematics in order to come to grips with the new domain has usually followed. And, as we have seen, such an audacious rethinking in a paraconsistent framework may nowadays vindicate some of Wittgenstein's "outrageous claims", which were dismissed too swiftly by commentators who dogmatically took the logic of Russell and Frege as the One True Logic.

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