What do Gödel Theorems Tell us about Hilbert's Solvability Thesis?

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When dealing with the foundational questions of elementary arithmetic, we find ourselves standing in the shadow of Gödel, just as our predecessors stood in the shadow of Kant, to the extent that we tend to see Gödel's famous incompleteness theorems as a new Critique of Pure Reason. In its most exuberant form (common particularly among the so-called working mathematicians) this amounts to claiming that

> human reason has encountered its limits by proving that there are truths which are humanly unprovable ("inaccessible") and that it is impossible for our mind to prove its own consistency.[1]

This attitude is not only at variance with the (Kantian) doubts about the possibility of proving the unprovability in an absolute sense, but, more specifically and famously, with the so-called Hilbert program of solving every mathematical problem by axiomatic means. In his *Parisian address*,[2] Hilbert not only phrased the conjecture that all questions which human mind asks must be answerable (the so-called axiom of solvability)[3] but supplemented it, as a kind of challenge, with a list of ten and later of twenty-three problems of prime interest, including the *Second Problem* of the consistency (and completeness) of arithmetical axioms.

In Hilbert's later writings, particularly in his *Königsberg address*,[4] the solvability argument takes a more subtle form. Introducing the finite mode

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1 (Gödel, 1995, p. 310) himself phrased it like this: "there exist absolutely unsolvable diophantine problems [...], where the epithet 'absolutely' means that they would be undecidable, not just within some particular axiomatic system, but by any mathematical proof the human mind can conceive."

2 See (Hilbert, 1900).

3 See (Hilbert, 1900, p. 297).

4 See (Hilbert, 1930).

of thought (*finite Einstellung*)[5] as a new kind of Kantian intuition, Hilbert argues that the harmony between *nature* (experience) and *thought* (theory) must lie exactly in the transcendental fact they are both finite.[6] As a consequence, the seeming infinity of human knowledge (particularly in the realm of mathematics) must have finite roots which are to be identified with a finite (or finitely describable) system of rules and axioms, and finite deductions from them.[7] Hence, "we must know, we shall know."[8] Obviously, this is a *transcendental deduction* of its own kind, namely of inferentialism or broader axiomatism from finitism, starting with the words: in the beginning was a sign.[9]

Gödel (1931), so we are usually told, put an end to Hilbert's optimism by proving that the Second Problem is essentially unsolvable. This verdict is sometimes supported by the seemingly analogous case of Hilbert's *First Problem*, the Continuum Hypothesis, which, partially also due to Gödel, was proved to be undecidable on the basis of currently accepted axioms. In this paper I would like to present Gödel's theorems not as a direct refutation of Hilbert's axiom but only as an impulse to phrase it with more caution, in such a way that the Continuum Hypothesis is no longer regarded as a *real* problem. I will draw on two rather different sources, both, however, connected to Hilbert's philosophy, namely

- the late metamathematical views of Zermelo and
- Lorenzen's post-Hilbertian program of operative mathematics.

This will lead me to a closer analysis of the distinction between proof and truth which does not endorse one of them at the expense of the other, as Lorenzen, the *constructivist*, and Zermelo, the *Platonist*, still tend to do.

1

First, let us discuss the possibility of proving the unsolvability of something. There is a general pattern: if someone comes along with a positive solution to a given problem, one can check to see that it does the required work. But if it is to be shown that the problem is unsolvable, one has to give a precise delimitation of the methods that can be employed. This brings us to the difference between *method* in the *broader* (general) and in the *narrower* (limited) sense.

[5] See (Hilbert, 1930, p. 385) and also (Hilbert, 1926, p. 161).
[6] See (Hilbert, 1930, pp. 380–381).
[7] See (Hilbert, 1930, p. 379) and also (Hilbert, 1918).
[8] (Hilbert, 1930, p. 387).
[9] See (Hilbert, 1922, p. 163).

To illustrate the point let us take some famous geometrical problems like the trisection of an angle or the quadrature of the circle. Due to the methods of modern algebra we positively know that these problems are unsolvable by straightedge and compass. However, we also know that the ancient mathematicians (Hippias, Archimedes) already solved them by extended — so-called mechanical — means (quadratix, spiral)[10] where the epithet "mechanical" means mainly that they were devised *ad hoc*. Similarly, if I give you — meaning somebody sufficiently educated in predicate logic — a formula, I am quite sure you will be able to decide, in a finite number of steps, whether it is a tautology or not. What you might not be able to do, however, is to solve the problem with pre-chosen schematic methods such as with *one particular* Turing machine.

Now, as may be expected, a similar observation applies to Gödel's theorems, only this time it is the provability itself the limits of which beg the question. Gödel showed that for any axiomatic system of arithmetic there will always be an individual sentence that is undecidable by it. The gist of his argument lies in the fact that this unprovable sentence of arithmetic (informally *saying* "I am unprovable") is unprovable because it is true (it *is* unprovable), its truth being proven as a part of the argument. So, the whole argument works only because it employs two different concepts of *proof*, the first being that of *Principia Mathematica* (or Peano arithmetic) and the second being the broader one in which the argument is clinched.

Zermelo, in his unjustly infamous correspondence with Gödel, was probably the first person to make this observation. Setting himself the natural question, "What does one understand by a proof?", his answer went like this:

> In general, a proof is understood as a system of propositions that, when accepting the premises, yields the validity of the assertion as being *reasonable*. And there remains only the question of what may be "reasonable". In any case — as you are showing yourself — not only the propositions of some finitary scheme that, also in your case, may always be extended. So, in this respect, we are of the same opinion, however, I *a priori* accept a more general scheme that does not need to be extended. And in this system, really all propositions are decidable.[11]

What needs to be explained now is the nature of the difference between proof in the narrower and broader sense, or between the *proof* and *truth*, and the sense in which the second one is "decidable", or better: complete and unextendable, as Zermelo claims.

[10] See, e.g., (Heath, 1931).
[11] See (Gödel, 2003, p. 431).

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The analogous differences between the general and narrower *construability* or *decidability* is less problematic since the *ad hoc* constructive or decision methods (like quadratix or spiral) are still bound to some humanly feasible means, and so quite naturally counted as constructions and algorithms. The traditional problem of arithmetic is its very relationship to the empirical world, as (already before Kant) expressed in the claim it is a science of analytical nature. Hence, the whole issue of the difference between the truth and proof can be boiled down to a single question:

> what is arithmetical truth outside of a specific axiomatic system?

It is exactly the lack of any explicit answer to this question that leads to the Platonist account of arithmetical truth. The usual model-theoretical exposition operating with an unexplained concept of standard model ("$2+2=4$" is true if and only if $2+2=4$) confirms this image, particularly when it starts to invoke our "intuitions".

However, to understand sentences like "$2+2=4$" and "$23+4<(6\times 3)+2$" you need no more mathematics than that provided by a good secondary education. This is to say that they are not true or false, at least not in the first place, *because* they are deducible in Peano arithmetic, or happen to inexplicably hold in the standard model, but *because* they are transformable into the simpler forms of "$4=4$" and "$27<20$" where only knowledge of the sequence $1,2,3,4,\ldots$ and the ability to compare symbols is needed. This is the basis of the operativist account of arithmetical truth as developed by Lorenzen in his *Einführung in die operative Logik und Arithmetik* (Lorenzen, 1955), in opposition to the usual standards of Frege that consider such justifications prescientific. According to Lorenzen,[12] the ultimate foundation of arithmetic (including higher analysis) lies exactly in these prescientific practices of counting and operating with *symbols*. They can be made explicit in synthetic (recursive) definitions like

$$\Rightarrow |, \qquad\qquad \Rightarrow x+|=x|$$
$$x \Rightarrow x|, \qquad\qquad x+y=z \Rightarrow x+y|=z|$$

$$\Rightarrow |\times x = x \qquad\qquad \Rightarrow | < x|$$
$$x\times y = p,\ p+y=q \Rightarrow x|\times y = q \qquad\qquad x<y \Rightarrow x<y|$$

introducing (in unary form) the number series, the operations $+$, $\times$ and the relation $<$ respectively. The (true) arithmetical sentences are then defined as

[12] See (Lorenzen, 1974, pp. 199–200).

the consequences of these definitions, prospectively within the broader frame of game- or proof-theoretical semantics (Lorenzen's dialogical games).[13]

As for Gödel's results, Lorenzen[14] claims that instead of being about arithmetic, as completely given by its operative definition, they merely tell us something about Peano's formalism in its particular shape of a first-order scheme within the language containing 0, s, $+$ and $\times$. So, coming from the other side, Lorenzen arrived at the same basic difference as Zermelo. It is also in accord both with Lorenzen's later views, as developed in his *Metamathematik* (1962), and with Zermelo's late project of infinitist logic,[15] to rephrase this difference in *inferentialist* terms as the distinction between two different kinds of consequence: strongly effective or *full-formal* $\vdash$ and the more liberal or *semi-formal* $\models$.[16] Now, simplifying heavily:

Full-formal arithmetic, like the arithmetic of Peano, is arithmetic *in the narrower sense*, and deals with schematically or mechanically given and controllable axioms and rules. Semi-formal arithmetic or *the* arithmetic *proper* employs — in accord with the infinite nature of the number sequence $1,2,3,\ldots$ — rules with infinitely many premises, particularly the (ω)-rule

$$A(1), A(2), A(3),\ \text{etc.} \quad\Rightarrow\quad (\forall x)(Ax). \tag{ω}$$

As an arithmetical rule it is transparent and sound enough (or "reasonable", as Zermelo would say), as long as one interprets the "etc." correctly. In fact, Tarski's idea of semantics[17] employs this kind of rules systematically, with the (ω)-rule as a special case of the more general

$$A(N)\ \text{for all substituents}\ N \quad\Rightarrow\quad (\forall x)A(x). \tag{$\forall$}$$

This rule is then nothing else than the well-known part of the so-called *semantic definition of truth*. Hence, the significance of semi-formalism is to make us think of semantic definitions as special (more generously conceived) systems of rules (proof systems) which — starting with some elementary sentences — evaluate the complex ones by *exactly one* of *two* truth values. The most important point to notice is that the semi-formal rules are called semantic not because they are infinite but because they, unlike Peano's formalism, work with a uniquely determined range of quantification.

As a consequence, arithmetical truth need not be guaranteed by God or by intuition, but, as (Zermelo, 1932, p. 87) put it, simply by the fact that the broader concept of "mathematical proof is nothing other than a system of propositions which is well-founded by quantification." Zermelo's

[13] See (Lorenzen & Lorenz, 1978).
[14] See (Lorenzen, 1974, p. 21–22).
[15] See (Zermelo, 1932).
[16] Both distinctions are due to (Schütte, 1960).
[17] See especially (Tarski, 1936).

claim that all the sentences are decided by his "more general scheme", i.e., completely and correctly evaluated by arithmetical semi-formalism, can be "proved" by an easy meta-induction like this:

1. Elementary arithmetical sentences ($M = P$, $M < N$) are evaluated unambiguously as true or false only on the basis of calculations with numerals.

2. Tarski's definition provides for the evaluation of more complex sentences, particularly because: either for every term N from $1, 2, 3, \ldots$, the sentence $A(N)$ is true and hence $(\forall x)A(x)$ is true, or there is N from $1, 2, 3, \ldots$ such that $A(N)$ is false, and $(\forall x)A(x)$ is false, *tertium non datur*.

It is a known fact that the intuitionists and some constructivists (including Lorenzen,[18] but not, e.g., Weyl[19]) question the completeness of this evaluation, arguing that the existence of concrete strategies for proving or refuting every $A(N)$ doesn't entail the existence of a general strategy for $A(x)$. To give a familiar example: there is no problem in demonstrating whether, for any given even number M, it is the sum of two primes. However, the truth value of the general judgment that every even number is the sum of two primes (Goldbach Conjecture) is still unknown, 250 years after the problem was first posed. Hence, it is possible that we have proofs for all the sentences $A(N)$ without knowing it, i.e., without having the general strategy of how to prove a proposition concerning them all.

Consequently, a decision must be made whether the infinite vehicles of truth and judgment such as $(\forall)$ or (ω) should be referred to as rules

1. only in the case when we positively know that all their premises are true, i.e., when we have at our disposal some general strategy for proving all of them at once, or

2. more liberally, if we know somehow that all their premises are positively true or false. The general distinction between the constructive and classical methods in arithmetic is based on this.

3

Now, if one leaves, like, e.g., Lorenzen and Bishop, the concept of effective procedure or proof to a large extent open and does not tie it, like, e.g., Goodstein and Markov, to the concept of the Turing machine,[20] there is still

[18] See, e.g., (Lorenzen, 1968, p. 83).
[19] See (Weyl, 1921, p. 156).
[20] For further discussion of these differences see, e.g., (Bridges & Richman, 1987).

room for an effective, yet liberal enough *semantics* (semi-formal system) and a strongly effective or 'mechanical' syntax or *axiomatics* (full-formal system). Hence, the constructivist reading does not necessarily wipe out the differences between the proof and truth, as, e.g., Brouwer's mentalism or Wittgensteins's verificationism seem to. As a result, one can officially differentiate not only between full-formal $\vdash$ and semi-formal $\models$ consequence, but also between semi-formal consequence in a stricter (constructive) sense and in the more liberal (classical) sense. All these differences stem from (Gödel, 1931) for the following reason:

Gödel's theorem affects only the full-formal systems, because their schematic nature makes it possible to devise a general meta-strategy for constructing true arithmetical sentences not provable in them. The unprovable sentence of Gödel is of the so-called Goldbach type, i.e., it is of the form $(\forall x)A(x)$ where $A(x)$ is a decidable property of numbers. Now, Gödel's argument shows that this decision is done already by Peano axioms in the sense that all the instances $A(N)$ are deducible and, hence, set as true. So, with Gödel's proof we have a general strategy for proving all the premises $A(N)$ at once, which makes the critical unprovable sentence $(\forall x)A(x)$ constructively true, i.e. provable by means of the (ω)-rule interpreted constructively.

> Lorenzen (1974, p. 222) put it like this:[21]
> ω-incompleteness [...] demonstrates that not all constructively true propositions are logically deducible from the axioms. This should come as no surprise. A universal proposition $(\forall x)A(x)$ is constructively true when $A(N)$ for all N is true. But in order *logically* to deduce the universal proposition $(\forall x)A(x)$, we must first deduce $A(x)$ with a free variable x. So we should have expected ω-incompleteness. But Peano arithmetic is ω-complete if we restrict ourselves to addition. The point of Gödel's proof was to demonstrate that Peano arithmetic with only addition and multiplication (without the higher forms of inductive definition) already shows the ω-incompleteness that was to be expected in general.

It is of real significance here that it was none other than (Hilbert, 1931) who — probably still unaware of Gödel's result[22] — employed the (ω)-rule as a means of improving his old project of founding arithmetic on axiomatic grounds. So, our claim that Gödel's theorems did not destroy but refine Hilbert's optimism in the suggested semi-formal way is sound also from a historical perspective. And using the concept of semi-formalism again, we can extend this optimism yet further by claiming that full-formal systems

[21] Translation by K. R. Pavlovic in (Lorenzen, 1987, p. 240–241).
[22] See Bernays' remarks in (Hilbert, 1935, p. 215) but also Feferman's commentary in (Gödel, 1986, pp. 209–210).

such as Peano and Robinson arithmetic are consistent simply because their axioms are provable in the arithmetical semi-formalism and, moreover, even in its constructive variant. This, in fact, is the usual model-theoretic argument:

> if a theory is inconsistent, then it does not have a model,

in a relative setting:

> if Peano arithmetic is inconsistent, then so is the arithmetical semi-formalism.

In the first case the consequent is precluded "by fiat". In the second case one does not need to use such tricks, because it was actually proved that the rules of semi-formalism do not evaluate arithmetical sentences incorrectly.

4

Now, should we perhaps follow Zermelo further and discard the narrower concept of proof totally by saying that everything true is provable? While the danger of the first extreme lies in the fact that the narrower, limited methods can and eventually will fail because of their limitedness, the shortcoming of Zermelo's alternative is that it is safe to the point of becoming totally idle. The problems of set theory are a particularly good example of such a situation. Let me illustrate it very briefly with the help of the concept of continuum.[23]

Continuum has had an intricate historical development, from the Pythagorean definition of proportion by means of a reciprocal subtraction, through the Euclidian theory of points constructible by means of a ruler and compass, to the Cartesian idea of numbers as roots of polynomials. By grasping real numbers as *arbitrary* (Cauchy) sequences, rather than as sequences that are in some sense law-like, Cantor believed himself to have won the whole game by simple "fiat". But this was no more substantiated than it would have been for the Greeks to define real numbers as points constructible by whatever means, or for us now to say that everything true is provable. Obviously, this would dispose of problems like the quadrature of the circle, the axiomatizability of arithmetic, or the "Entscheidungsproblem", but it would also dispose of the whole of mathematics — insofar as it is understood as an enterprise of solving problems somehow related to human lives rather than as a pure science indulged in for its own sake. Hence, the reason for retaining and developing the difference between the broader (and vaguer) and the narrower (more limited) sphere of methods lies in the fact that it

[23] For a detailed account see (Kolman, n.d.).

mirrors the general process of *explaining* something complicated through something less complicated.

Set theory runs into problems because of its failure to keep these differences apart. Set theorists believe, on the one hand, that the Continuum Hypothesis is either true or false whether we know it or not, but, on the other hand, the only specific idea they can give us about its standard model is one loosely connected to Zermelo's full-formalism, by which it is, however, undecidable, i.e. neither true nor false. So, because the only criterion of truth is the incomplete and possibly inconsistent full-formalism, we must face the possibility that the status of questions like "how big is the continuum?" may be similar to that of questions like "how many hairs does Othello have?", not because we do not yet know the answer, but because no answer is available. This deficit does not make such questions human-independent, but only deeply fictitious, the reason for which, again, is not that they are still undecided (such a decision is not difficult to make, e.g., by endorsing $V = L$) but because nothing really important hinges on them.

My conclusion may resemble the position of (Feferman, 1998, p. 7), according to whom the Continuum Hypothesis, unlike Hilbert's Second Problem, "does not constitute a *genuine* definite mathematical problem," because it is an "inherently vague or indefinite one, as are propositions of higher set theory more generally." I have attempted, however, to be more specific about where the difference between set theory and arithmetic comes from. The so-called iterative hierarchy, described in a pseudo-constructive manner by Zermelo's axioms, is not a model in the same sense in which the standard model of arithmetic is, because the concept of subset is left unexplained, along with the range of quantification and the respective $(\forall)$-rule.[24]

To sum up: Hilbert's solvability thesis is not refuted by Gödel's incompleteness theorems, nor by the Continuum Hypothesis; however, they oblige us to rephrase it as follows: every problem is (potentially) solvable if it is endowed with well-defined truth-conditions, or, as Zermelo would put it, with a "reasonable" concept of truth.

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[24] One can possibly say that set theory has failed both of Frege's criteria for reference, as described so influentially by Quine, namely "to be is to be a value of a bound variable" and "no entity without identity" with "$|P(N)| = ?$" taken as evidence.

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