Kurt Gödel and the Consistency of $\mathbf{R}^{\#\#}$*

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Summary. This paper continues my investigations of arithmetics formulated relevantly. (See references [1], [10], [12], and [9].) It is proved again that relevant **Peano** arithmetic $\mathbf{R}^{\#}$ and relevant **true** arithmetic $\mathbf{R}^{\#\#}$ (with the ω-rule) are **demonstrably consistent** by simple finitary arguments. E.g., it requires little more than truth-tables to show that $0 = 1$ is a non-theorem. This **removes** much of the sting from Gödel's **second** theorem. Regard for relevance **bounds** the harm that even potential contradictions can do. But Gödel still collects his **dues**, since proving **negation-consistency** remains annoyingly (and ineluctably) **non-constructive**. To the extent that $\sim$ in *Formalese* is unlike **not** in *English*, as it seems to be, Gödel's theorems are **dirty tricks**.

1.

Being mainly self-educated in beginning logic, I was alarmed to read in [2] that Gödel had shown that elementary number theory is either inconsistent or incomplete. "What," thought I to myself, "could this possibly **mean**?" Could it be in **doubt** that 2+2 = 4? Might one **multiply** 27 and 37 and get 998? What is **going on** here?

More mature reflection convinces one that what is going on is a logical **dirty trick**. Speaking at his most persuasive, Gödel in [3] conned a certain sentence G into saying of itself that it was unprovable.[1] We all know where the story goes from there, at least intuitively. If G is *false*, then it is provable after all, which engenders contradiction. So G had better be true. And, as this reasoning can be carried out in any sufficiently strong, consistent and effective system **S**, the moral **is** (or is **alleged** to be) that

(I) **S** is incomplete, containing an unprovable truth (Gödel's **first** theorem), and
(II) **S** lacks the means to formalize a proof of **its own** consistency (Gödel's **second** theorem).

I shall throw no stones at (I) here. But (II) is another matter. The idea behind it is said to be (in, e. g., [3] and [4]) that we can **formalize** the proof of (I). This leaves us with the following **S**-theorem:[2]

* This paper is in its final form and no similar paper has been or is being submitted elsewhere.

[1] G is 17 gen r, says [3].

[2] We follow [3] in using "Wid" (for "widerspruchsfrei") for "consistent".

(A) $\vdash_{\mathbf{S}} \mathrm{Wid}(\mathbf{S}) \supset G$.

In view of the rule $\supset$E of *modus ponens*, if we could prove in **S** that **S** is consistent then we could also prove in **S** its non-theorem G. As this is impossible (unless things have gone **very badly wrong**), we cannot prove Wid (**S**) either.

There is no doubt that [3] is an amazing and an incredible achievement in logic, and that it has been seen as such since its publication over 60 years ago. Still, there is something about the result that does not seem to ring true.[3] What I wish to offer here are **philosophical corrections** of the logic that induces the false chime. The first crux lies in the little sign '$\sim$'. This sign is supposed to mean 'not'. The real import of Gödel's arguments may be summed up succinctly thus: '$\sim$' **never** means what it is supposed to mean, **within** a particular sufficiently strong system intended seriously to formalize mathematics.

To be sure, we **can** claim to give a semantic interpretation of an effectively presented arithmetic, and tell the world that on this interpretation '$\sim$' means 'not'. The world will then ask how it comes about that on some occasions on which A is false, we cannot prove $\sim A$ in the system. We shall perhaps reply that relative to the interpretation the system is (negation-)incomplete. But how much more accurate it would be to reply instead that, because of certain formal anomalies in the technical engineering, we just **cannot** so fix things that '$\sim$' works within the system the way that 'not' is to be taken as working in English.[4]

We make this point clear, on Gödelian grounds, with respect to (standard classical Peano arithmetic) $\mathbf{P}^{\#}$.[5] We suppose some standard coding (Gödel numbering) that assigns to each formula A a unique natural number I. We assume that this coding is an effective mapping from formulas to natural numbers, and we shall henceforth use A^I for the formula with Gödel number I.[6] (The scheme of [4] will do for the purpose of furnishing such a (Gödel) numbering.) Let **N** be the set of all natural numbers; and, taking our formal system **S** abstractly, we **identify** natural numbers with the corresponding **numerals** of the system.

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[3] So much so, I was informed by van Fraassen when he was editor of the **Journal of Philosophical Logic**, that a chief source of manuscripts submitted to that journal were "fix-ups" for Gödel's theorems.

[4] In view of the Liar Paradox and associated anomalies, we might more candidly have to admit that we don't know how 'not' really works in English, either.

[5] Take $\mathbf{P}^{\#}$ to be the system **S** of [4, p. 102f]; equivalently, let it be the system **PA** of [7].

[6] Even more elegant is the course of simply **identifying** the **formula** A^I with the **number** I. One might read [3] as suggesting this very course. For as it doesn't really matter what the **formal objects** constituting a denumerable formal system are, they **might as well be** the **natural numbers**. This is **Gödel numbering** with a vengeance!

Then, as is well-known, there is an open formula Px,[7] with sole free variable x, which serves as a **provability predicate** for $\mathbf{P}^{\#}$ in the following sense:

(1) For all natural numbers I, PI is a theorem of $\mathbf{P}^{\#}$ iff A^{I} is a theorem of $\mathbf{P}^{\#}$.

Taking truth in the standard model $\mathbf{N}$ in the usual Tarskian sense, we have also

(2) For all natural numbers I, PI is true iff A^{I} is a theorem of $\mathbf{P}^{\#}$.

So on our semantic understanding of $\sim$ we have immediately from (2),

(3) For all natural numbers I, $\sim PI$ is true iff A^{I} is **not** a theorem of $\mathbf{P}^{\#}$.

But on Gödelian grounds we do **not** have

(4) For all natural numbers I, $\sim PI$ is a theorem of $\mathbf{P}^{\#}$ iff A^{I} is **not** a theorem of $\mathbf{P}^{\#}$.

A counterexample to (4) is our old friend "I am unprovable," alias 17 gen r.

Viewed extrinsically, as in (3), we may perhaps think of '$\sim$' as a formal counterpart of 'not'. Viewed systematically, and given (1), what clearer demonstration could we ask than (4) of the proposition that '$\sim$' doesn't work formally in $\mathbf{P}^{\#}$ the way that 'not' works intuitively in English?[8] And we now fix the considerations and notation set out above for the rest of the paper (including the standard Gödel numbering, which we need not specify further). These considerations are central to an examination of the character and import of Gödel's second theorem. It is of little interest that we cannot **prove** the consistency of $\mathbf{P}^{\#}$ within itself, unless $\mathbf{P}^{\#}$ has the **vocabulary** to say that it is consistent (and moreover that what it says in this vocabulary is in fact unprovable).

Let us reflect. **First**, nobody ever expected $\mathbf{P}^{\#}$ to be muttering introspectively about itself at all. $\mathbf{P}^{\#}$ was constructed to say that $5 + 3 = 8$, that every number > 1 has a prime divisor, and the like. It was **not** constructed to say that nobody ever loved it before Hilbert, that it often wishes it were complete, or (for present purposes) that it is consistent.

Of course, after Gödel we are **all** prepared to believe that $\mathbf{P}^{\#}$ does introspect, in code. And this has made its psychoanalysis (or whatever the equivalent process is in formal systems) a regular element in training logicians. As in all psychoanalysis, there is a certain indistinctness in the method. When, e. g., $\mathbf{P}^{\#}$ seems to be saying, "Every natural number is the sum of 4 squares," we may suppose that it is bragging, "Show me a sentential tautology I can't prove!" Or maybe it is complaining, "I can only demonstrate

[7] In [3] we have "Bew" (for "beweisbar") instead of "P" (for "provable").

[8] Recall that (4) **must** fail, on pain of **bankruptcy** otherwise for the standard arithmetical mythology!

Fermat's Last Theorem for regular primes." And this should lead to a little humility on our part. Stripped of the code, $\mathbf{P}^{\#}$ is still **saying** "Every natural number is the sum of 4 squares." The rest we **read into** what it says; and, since $\mathbf{P}^{\#}$ is incomplete, we err even in simple arithmetic if we try to interpret what it says **categorically**.

Second, we must exercise unusual care in finding a technical form for Gödel's second theorem.[9] We must formalize the statement "I am consistent," uttered by $\mathbf{P}^{\#}$ about itself. There are a couple of ways in which our previous worries about negation will come to the fore. For **all** of our classically equivalent ways (1)-(6) in section 3 below of saying that formal arithmetic is consistent will assert that something-or-other is **not** provable. If we are worried about our capacity to **express** 'not' in $\mathbf{P}^{\#}$ then we shall have **most distressing worries** about how to say that arithmetic is consistent, even in code.

Third, one begins to wonder what Gödel's second theorem adds to his first theorem. No one expects us to be able to **prove** what we cannot **say**. And it then seems otiose to claim that any effective, finitary proof of the consistency of formal arithmetic would yield a proof in $\mathbf{P}^{\#}$ of a formula that, so far as $\mathbf{P}^{\#}$ is concerned, is only a dubious candidate for the role of being **the statement** in the vocabulary of $\mathbf{P}^{\#}$ which expresses its consistency. We don't linger over these issues. We even take a rather orthodox stand with respect to them. But we do note that they are exacerbated when we ask "What particular form, even in English, should the statement that $\mathbf{P}^{\#}$ (or any formal arithmetic) is consistent take?" We devote the next two sections to some of these problems.

2.

We ask ourselves first why we (or Hilbert, or Gödel) should **care** whether arithmetic[10] is consistent. We answer immediately that, mainly, we do **not** care. Put optimistically, we are so strongly convinced that arithmetic is consistent that demonstrating its consistency is just a game—the game of seeing how little, or how much, is required for a formal consistency proof. After all, it was the reliability of mathematical **analysis** that truly worried Hilbert and others. And since the ultimate effect of the great 19th and early 20th century programs was to substitute insecurity in reasonings about sets for insecurity in reasonings about infinitesimals and series, the neck-wringing that Gödel administered to these programs in 1931 leaves us having registered no gain on the main point.[11]

[9] Feferman said so in [5], and Gödel appended a footnote to the same effect to the translation of [3].

[10] or at least any part of it of which **serious** use is going to be made

[11] Indeed, they have led to a certain abandonment of the main point, all hands being needed (as Reid aptly puts it in [6]) to defend the homeland of arithmetic.

But while we are playing the game, what we are presumably concerned to show is that our intuitive arithmetic is **reliable**, by establishing that our carefully chosen formal counterparts of that arithmetic are reliable. So long as we accept Gödel's first theorem, part of that task remains beyond us. For according to that theorem **no** effectively presented formal system in the ordinary first-order vocabulary will serve as a fully acceptable formal counterpart of intuitive arithmetic.

So any effective formal arithmetic is at best a partial arithmetic. We can **improve** this situation in a couple of ways. First, it is clear that being **partial** is not to be confused with being **unreliable**. It is sad to have to relate that this is the point at which the usual appeals to Gödel's second theorem tend to descend into perversity. For it is claimed that no bag of mathematical tricks can be demonstrated to be reliable, except on appeal to some trick that isn't in the bag. So we are confronted with a picture (on the usual story) on which the reliability of any mathematical system (save such as are inadequate for whole number arithmetic) can only be demonstrated in some system **less reliable**, *prima facie*, than the system from which we began.

This picture, if accurate, severs mathematical logic from its chief foundational purpose—namely, making possible a rigorous reconstruction of intuitive mathematics. One gets the impression rather that even the reconstruction of simple arithmetic is dubious enough, and that every step on becomes even more dubious. And it is accordingly no wonder that many mathematical logicians have gone off to live in a world of their own—a world, frankly, that has little relevance to mathematics, even less to the philosophy of mathematics, and almost none to general philosophy. For the depressing picture is that **more** than intuitive mathematics must be assumed in order to reconstruct intuitive mathematics. Chauvinist mathematicians (e. g., Poincaré), who always bridled at the suggestion that their discipline was just pure logic, may find cause to rejoice in this picture. But logicians must weep, for it denigrates exact thought for the sake of the old mumbo-jumbo.

We began to talk about consistency, but we have slipped in this section into talk about reliability. Consistency is a formal property of formal systems (though, depending on the author, it may be any one of several properties, not necessarily related). Reliability is an intuitive property, measuring a formal system against the purposes for which it was designed. And let it be clear from the outset that it is **reliability** that is most desired. We wish that our formal systems shall be adequate to their purposes. And whatever formal property we decide to identify with the honorific 'consistent', it is of interest only insofar as possession of this property is a guide to (and hopefully a guarantee of) the system's reliability.

3.

Now let us pick up a few stones. Already in [4] was Feferman's [5] cited, which does present a plausible candidate for Wid (S) which **is** a theorem of suitable **S**. What are we to conclude from that? Only that the proof of (A) of section 1 will then break down, unless (God forbid) **S** is already inconsistent. But, in choosing our stones, let us **forget** (temporarily) about trying to find a way to say **in S** that **S** is consistent. What, **in English**, is a reasonable way of saying this? Here are some.

(1) There exists a formula A such that A is a non-theorem of **S**
(2) $0 = 1$ is a non-theorem of **S**
(3) All **numerically incorrect** equations[12] A are non-theorems of **S**
(4) All **algebraically incorrect** polynomial equations[13] A are non-theorems of **S**
(5) $\sim(0 = 0)$ is a non-theorem of **S**
(6) For no formula A are both A and $\sim A$ theorems of **S**

These are intended as **increasingly stringent** criteria for consistency.[14] But except for the option (7) of the last footnote, these criteria all come classically to the **same thing**. For intuitively (6) implies (5), and so forth until we get to the fact that (2) implies (1). But (1) implies (6), completing the circle, on account of the implicational paradox $A \& {\sim} A \supset B$. So classical logic **blurs** what are intuitively clear distinctions.

Relevant and other substructural logics **L** exist that **excise** some of the evils of classical logic. Perhaps if we choose one of these **L** in which to formulate our formal arithmetic **S**, the distressing equivalence of **all** of (1)-(6) will disappear. And we need go no further than the Church-Anderson-Belnap system **R** of [8] and [9] to reach this goal.[15] Here for example is a list of postulates to be added to the first-order relevant logic $\mathbf{R}^{\forall\exists x}$ of [9][16] to formulate the first-order relevant Peano arithmetic $\mathbf{R}^{\#}$.

R#1 $x = y \to x' = y'$
R#2 $x = y \to (x = z \to y = z)$
R#3 $x + 0 = x$

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[12] A formula $t = u$ without free variables is correct if so by primary school arithmetic, else it is **incorrect**. $27 \times 37 = 999$ is **correct**. $2 + 2 = 5$ is **incorrect**.

[13] A polynomial equation t=u is **correct** if so by high school algebra, else it is **incorrect**. Thus, e. g., $(x + y)(x + y) = xx + xy + yy$ is incorrect. Correct is $(x + y)(x + y) = xx + 2xy + yy$.

[14] Another criterion begs for admission here—namely, (7) no **arithmetic falsehood** is a theorem of **S**. But (7) jumps from the Deep End into the Standard Numerical Mythology. We do **not** jump with it—**yet!**

[15] As Mortensen and I point out in [10], the stronger Dunn-McCall system **RM** will also do.

[16] Before [9], $\mathbf{R}^{\forall\exists x}$ was called **RQ**. See [10]-[12] for the vocabulary of $\mathbf{R}^{\#}$ and its notational conventions.

R#4 $x + y' = (x + y)'$
R#5 $x \times 0 = 0$
R#6 $x \times y' = (x \times y) + x$
R#7 $x' = y' \to x = y$
R#8 $\sim(x' = 0)$
R#9 $A0 \;\&\; \forall x(Ax \to Ax') \to \forall x Ax$[17]

As rules we take

$\to$E From $A \to B$ and A infer B
$\forall$I From A infer $\forall x A$

Unfortunately for $\mathbf{R}^{\#}$, it is even less satisfactory than $\mathbf{P}^{\#}$ as a vehicle for formal arithmetic.[18] But there is a repair $\mathbf{R}^{\#\#}$, which adds the following ω-rule:

$\forall 012\ldots$ Infer $\forall x Ax$ from **all** of $A0$, $A1,\ldots,An,\ldots$, for every numeral n.

We borrow from previous results to show that $\mathbf{R}^{\#\#}$ (and *a fortiori* $\mathbf{R}^{\#}$) is consistent.

4.

Where n is any natural number, we let F_K be the integers $\{0, 1, \ldots, n-1\}$ modulo n. Note that $+$ and $\times$ are naturally defined on F_K. And we let **S3** be the **3-valued** matrix on $\{+1, 0, -1\}$ defined as follows:[19]

$\to$	-1	0	$+1$
-1	$+1$	$+1$	$+1$
$*0$	-1	0	$+1$
$*{+1}$	-1	-1	$+1$

The $*$'ed elements 0 and $+1$ are *designated values*, and we set $m \leq n$ in **S3** if $m \to n$ is designated. (Note that $\leq$ is just the usual order on $-1, 0, +1$.) Letting $\to$ be defined by the table above, define the other connectives on **S3** by

$$\sim m = -m,$$
$$m \,\&\, n = \min(m, n), \text{ and}$$
$$m \vee n = \max(m, n).$$

Where D is a domain of individuals, we can also interpret the quantifiers in **S3** by setting

[17] This **induction** postulate may be stated as a rule **RMI** for $\mathbf{R}^{\#}$. [13] rightly prefers **RMI** in weaker logics.
[18] Ackermann's rule γ ($\supset$E) fails for $\mathbf{R}^{\#}$. See Friedman and Meyer's [11].
[19] The 3-valued Sugihara matrix **S3** appears early and often in [8] and [9].

$$\forall x Ax = \min(Ad : d \in D)$$
$$\exists x Ax = \max(Ad : d \in D),$$

where for each $d \in D$ we have Ad as the "truth-value" in **S3** that results from evaluating Ax *at* d. We can now **complete a standard interpretation** I of each of the $\mathcal{F}_K$ in **S3** by specifying the value of I on atomic sentences. In the simplest case, where $n = 2$ and $\mathcal{F}_K = \{0, 1\}$, we set

(=2t) $I(0 = 0) = I(1 = 1) = 0$, and
(=2f) $I(0 = 1) = I(1 = 0) = -1$.

It is readily observed that all theorems of $\mathbf{R}^{\#\#}$ take non-negative values on I.[20] This immediately shows that $\mathbf{R}^{\#\#}$ (and *a fortiori* $\mathbf{R}^{\#}$) is consistent in senses (1) and (2) of section 3 , by a completely elementary argument. So much for the demise of the Hilbert program purportedly brought on by Gödel!

What about the notions (3)-(6) of **III**? (3) is in principle no more difficult than (2); for any number equation $t = u$ will reduce to one $m = n$, where m and n are numerals; assuming without loss of generality that $m \leq n$, we can further reduce an **incorrect** number equation to $n - m = 0$, where $n - m$ is a **positive** integer. We generalize (=2t) and (=2f) above to define standard I by

(=nt) $I(k = k) = 0$ for all $k \in \mathcal{F}_K$
(=nf) $I(j = k) = -1$, for all distinct $j, k \in \mathcal{F}_K$

And it is now clear that all theorems of $\mathbf{R}^{\#\#}$ take a designated value on each standard I in **S3** while for each incorrect number equation it is trivial to find a $\mathcal{F}_K$ that refutes it. That's consistency in sense (3). And we can extend this proof to get consistency in sense (4), observing that every **incorrect** polynomial equation has a **substitution instance** in natural numbers which is **numerically incorrect**. So $\mathbf{R}^{\#\#}$ is **polynomially consistent** as well.

We have, however, taken these methods about as far as we can. In particular, we **cannot** get from the $\mathcal{F}_K$ a simple proof that $\mathbf{R}^{\#\#}$ is negation-consistent or even that $\sim(0 = 0)$ is unprovable, which are (6) and (5) respectively of **III**. Consider the latter. As our standard interpretation I will assign 0 to each $k = k$, it will similarly assign $-0 = 0$ to $\sim(k = k)$. As 0 is itself a designated value in **S3**, this is no way to show that negated identities are unprovable. Moreover since $A \,\&\, {\sim}A \rightarrow {\sim}(0 = 0)$ is a theorem scheme of $\mathbf{R}^{\#\#}$ (indeed, already of $\mathbf{R}^{\#}$), we cannot use the $\mathcal{F}_K$ to establish negation-consistency either.

Still, if one thing doesn't work, we can try another. Defining material implication $\supset$ as usual by

(D$\supset$) $A \supset B =_{\mathrm{df}} {\sim}A \vee B$,

[20] The nice thing here is that we have actually made the ω-rule $\forall 012\ldots$ finitary. For, mod 2, the quantifiers are just ranging over 0 and 1!

we already have provable in $\mathbf{R}^{\#}$

$$(8)\ \sim(0=0) \supset 0=1,$$

in view of the theoremhood of $0=0$. Of course (8) is also provable in $\mathbf{R}^{\#\#}$. But $\mathbf{R}^{\#\#}$, unlike $\mathbf{R}^{\#}$, admits $\supset$E, as [12] demonstrates. So, as 0=1 has a finitary refutation *mod* 2, we can rest content that $\sim(0=0)$ is also a non-theorem; similarly, since if any contradiction were a theorem then so also would $\sim(0=0)$ be provable, we can assert that $\mathbf{R}^{\#\#}$ is negation-consistent.

So we can. But the pleasant finitary character of our appeal to **S3** through the F_K is gone and lost forever[21] when we go through the $\supset$E proof of [12]. Granted, on Gödelian grounds a charge will be made **somewhere** for a proof of negation-consistency. And the charge in this case is the **non-constructive** character of the argument of [12].

5.

What, I have been asked, is the relation between $\mathbf{R}^{\#}$ and $\mathbf{R}^{\#\#}$ and more familiar systems like classical $\mathbf{P}^{\#}$? When I first published a few of my results about relevant arithmetic in the abstract [1], it was my hope that $\mathbf{R}^{\#}$ would exactly contain $\mathbf{P}^{\#}$, in the sense that every theorem A of $\mathbf{P}^{\#}$ would be a theorem of $\mathbf{R}^{\#}$, on direct truth-functional translation. This was not to be, on account of Friedman's contribution to [11]. But there are nonetheless several exact translations on which $\mathbf{P}^{\#}$ is a truth-functional subsystem of $\mathbf{R}^{\#}$. Thus all theorems of $\mathbf{P}^{\#}$ are on these translations theorems of $\mathbf{R}^{\#}$, whence all classical metatheory goes through.

$\mathbf{R}^{\#\#}$ is quite another kettle of fish. It stands to $\mathbf{R}^{\#}$ as classical **true arithmetic** $\mathbf{P}^{\#\#}$ stands to $\mathbf{P}^{\#}$. It is proved in [12] (though already noted in [1]) that $\mathbf{P}^{\#\#}$ **is contained** in $\mathbf{R}^{\#\#}$ on direct truth-functional translation. This disposes immediately of many questions that one **might have had** about $\mathbf{R}^{\#\#}$. Its theorems are not recursively enumerable; nor is it recursively axiomatizable. (Why? Because the **truths** of $\mathbf{P}^{\#\#}$ aren't r.e.)

A referee has called attention to an apparent **incoherence** between section 1 (in which we said that Gödelian troubles arose because '$\sim$' does **not** mean 'not') and section 3 (in which we complained that what is most wanted in formal systems for arithmetic and other stuff is **reliability**). This is a good point, though I wish here to reiterate both claims. Naturally we want our systems to be reliable; and I say that relevant systems are (demonstrably) **more** reliable than the standard brands. For we can rest reasonably content with what **has** been shown here. We have sidestepped the layers and layers of Gödelian uncertainty by showing that some sorts of mistakes **just can't happen**, if we formulate our theories relevantly. This has been a point **all too easily overlooked** in the debates for and against the adequacy of

[21] Just like "my darling Clementine"

truth-functional insights. To have **faith** is a wonderful thing (perhaps). But in logic, **reason** is supposed to rule. And not merely actual contradiction but also potential contradiction can undermine a system that is formulated truth-functionally. Regard for relevance **bounds** the harm that even **potential** contradictions can do.

But Gödel bites the truth-functionalist even more deeply. What are we to make of a statement

$$\sim(721 \text{ is provable})$$

when this is the very statement #721? As I said at the outset, this is a **dirty trick**. And dirty tricks ought not to be confused with profound metaphysical insights about 'not'. We rest our case!

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