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Saying it with Numerals

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Abstract This article discusses the nature of numerals and the plausibility of their special semantic and epistemological status as proper names of numbers. Evidence is presented that minimizes the difference between numerals and other devices of direct reference. The availability of intensional contexts within formalised metamathematics is exploited to shed light on the relation between formal numerals and numerals.

> "Many educated people have little grasp for these numbers and are even unaware that a million is 1,000,000; a billion is 1,000,000,000; and a trillion, 1,000,000,-000,000." (Paulos [18], p. 10)

1 Introduction Puzzling Peano never travels to strange cities, is resolutely monolingual, and, indeed, restricts his cogitations to matters arithmetic. Puzzling Peano knows both the binary and decimal numeral systems; he believes that 6 is not less than the sum of its divisors, he *certainly* believes that $110_2 = 110_2$, and also believes (falsely) that 110_2 is less than the sum of its divisors. Since 6 is 110_2, shall we say that Puzzling Peano has contradictory beliefs, or that he believes that $6 \neq 110_2$? This puzzle is adapted from Ackerman [1], p. 151.

This variant of Kripke's puzzles concerning London/Londres and Paderewski (see Kripke [15]) differs from them in involving numbers and numerals. Puzzling Peano is only one among many possible puzzles involving numbers and propositional attitudes that strongly parallel well-documented non-numerical puzzles. The case of Puzzling Peano has a variant in which *identical to 6* replaces the predicate *is not less than the sum of its divisors*; this makes it much like the original Frege puzzle involving Hespherus/Phosphorus. Other puzzles can be produced by alteration of existing problem cases.[1]

Charles Parsons, in presenting a subtle reconstruction of a Kantian notion of the intuition of numbers as types, introduces one problem as follows:

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> [A] problem is cognitive relations, including *de re* propositional attitudes; if I see on a blackboard the formula "$(x)(\neg x = 0 \rightarrow \exists y(x = Sy))$", I do not see the number that corresponds to it under some arithmetization of the syntax of first-order arithmetic. (Parsons [17], p. 161)

Here we see another puzzle involving propositional attitude and reference to numbers, this time in the context of arithmetization. It should be no surprise that reference to numbers and reference to formulas and formalisms are connected; in fact, the trick, as Parsons' problem suggests, is to pull them apart a bit.

Before turning explicitly to numerical puzzles of propositional attitude, I sketch the common ground of the arithmetic and non-arithmetic cases.

1.1 Although Kripke's Puzzle and Puzzling Peano are presented as *de dicto* puzzles, they have their *de re* cousins. The *de dicto* puzzles get their plausibility from the same semantic intuitions about proper names that inform certain *de re* inference patterns. Indeed, the logico-linguistic intuitions supporting *de re* reference are no weaker in the case of numbers, and other abstract objects, than in the case of Ortcutt and his ilk. From:

> Edna believes that 6 is the smallest perfect number.

it follows that 6 is such that Edna believes it to be the smallest perfect number. This latter does not follow from:

> Edna believes that the smallest perfect number is the smallest perfect number.

A related phenomenon is the divergence in meaning (and, in appropriate contexts, truth value) of "Edna believes that some number is the smallest perfect number" and "There is some number such that Edna believes it is the smallest perfect number."

Or so go some essential underlying intuitions which an account of *de re* propositional attitudes about numbers should either explain or explain away. Similar linguistic intuitions support the inference that **P** (Peano Arithmetic) is believed by Edna to be consistent from:

> Edna believes that **P** is consistent,

but not from:

> Edna believes that the largest consistent subsystem of **P** is consistent.

(Although **P** is the largest consistent subsystem of **P**.)

David Kaplan, in his classic work on *de re* attitudes, (Kaplan [14]), is not entirely neglectful of numbers; yet his remarks are not entirely satisfactory. Kaplan's account of *de re* reference to numbers rests on the notion of a *standard name*:

> A standard name is one whose denotation is fixed on logical, or perhaps I should say linguistic, grounds alone. ([14], p. 222)

Kaplan adds to this:

> Numerals and quotation names are prominent among the standard names. . . . [W]hat is at stake is not pure reference in the absence of any descriptive structure, but rather reference freed of *empirical* vicissitudes. ([14], p. 222)

Two pages later, he states: “Numerals are reliable; they always pick out the same number.”

Kaplan’s discussion makes it seem that these are intended as sufficient, as well as necessary, conditions for exportation. Furthermore, as Ackerman notes,[2] Kaplan seems to think that the standard name account extends readily, though restrictively, to belief: “The same trick would work for **Bel**, if Ralph would confine his cogitations to numbers and expressions,”([14], p. 225). Kaplan’s *examples*, numerals and quotation names, are unobjectionable; his characterization of the notion of a standard name, however, admits descriptions like ’the smallest perfect number’—a description which surely refers free of “empirical vicissitudes”. His first characterization, “denotation fixed on . . . linguistic grounds alone,” is narrower and more plausible. Ackerman amends Kaplan’s account by replacing his characterization with his examples. In this way she accommodates some basic intuitions that Kaplan’s account violates.

1.2 Kaplan’s asides aside, one reason that detailed consideration of *de re* reference to abstract objects has been largely neglected in the literature is that, despite the seamlessness of our logico-semantic intuitions, reference to abstract objects appears cut from a different cloth than reference to concrete objects. This owes to a vague epistemic intuition—that having a *de re* propositional attitude entails a certain epistemic *rapport* with the object of the attitude. As a principle of direct reference, the rapport requirement is most often applied negatively: to defeat a putative case of a *de re* propositional attitude construct a context where the relevant *rapport* is missing.[3] Moreover, it is a requirement that the singular term whose exportation is in question plays an essential role in “. . . [getting] one more *en rapport* epistemically.” (Ackerman [1], p. 147). While it remains unclear what precisely the relationship between our logico/semantic intuitions and our epistemic intuitions is, epistemic considerations are sometimes deployed to defeat a claim of *de re* belief.

One can infer from “Edna believes that 6 is even” that Edna believes of 6 that it is even. Our confidence, such as it is, in this inference seems to stem from purely semantical/grammatical knowledge superficially innocent of epistemological considerations. The surest method for establishing absence of the necessary *rapport* is to establish the non-existence of the *res* in question. In the case of numbers a robust tradition utilizes the (more dubious) converse principle. That is, one argues for the absence of the necessary *rapport* and concludes the non-existence of the *res*, or at least the non-existence of a *de re* attitude. Given the state of theories of appropriate *rapports*, it is not clear that this form of argument should be very worrisome. Indeed, I will suggest the availability of a more unitary account of *rapport vis à vis* numbers (and certain other abstract objects) than is evidently available for cities, people, etc.

Ackerman’s proposal, after rejection of some views that do not respect the intuitive data, is that *contra* Kaplan, only some standard names will support exportation and quantifying in: namely, numerals. As it stands though, this amounts to sacrificing an account with some theoretical content in favor of a class of clear examples. As Ackerman herself notes, an adequate account would need to answer two central questions:

1. What are numerals?
2. What makes them special, semantically and epistemically?

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> "But what is special about what is expressed by numerals as compared with other standard names?. . . . [T]here seems to be a sense in which a numeral directly specifies the position of its referent in the progression of numbers. ." (Ackerman [1] p. 151)

The view that numerals are rigid designators is widespread; it is also well-known that belief contexts are not as well-behaved as the alethic modalities regarding rigid designation. Thus, even if Kaplan's Ralph were to restrict his cogitations to numbers, the belief context would cause trouble for the standard name account. The restriction of terms to the numerals still leaves puzzling cases about belief and knowledge. Indeed, Puzzling Peano is just another case of a phenomenon briefly canvassed by Kaplan—"the possibility of a single person bearing distinct exportable names not believed to name the same thing." Analogous cases abound, even with those paradigms of exportability, indexicals.[4]

In the years since Kaplan's paper puzzles and theories of direct reference have been widely published. The line I will take is, loosely stated, as follows: co-referential directly referential terms contribute identically to the propositional content of sentences in which they occur, but "guises" mediate certain relations that people can bear to the designated object, and hence to the propositional content.[5] The relationship, left unspecified here, between a guise and its object can be thought of as; *gives an analysis of*; *is the mode of presentation of*; *is the sense of*; or possibly *fixes the reference of*, depending on the particular theory embraced. Theories can differ widely as to the nature of guises—guises could be grammatical structure or clusters of descriptions. My goal is to point out the most plausible candidate guise for the special case of numerals.

2.1 In English the candidates for standard, exportable names for numbers seem to be the arabic numerals in various bases, and the interesting assortment of expressions in English that "canonically" name numbers. Thus '6', '110_2', 'six' may all be numerals; it may even be that 'six' is just an alternate spelling of '6'.[6] What makes these numerals?—is there an account more illuminating than mere appeal to our intuitions that these will support exportation? The contrast of '6' with '3+3' and with 'the smallest perfect number' suggest that syntactic simplicity may play a role. It may, but the sense of 'syntactic simplicity' is going to have to be complicated; for in what sense of "syntactically simple" is '3+3' not simple, but '13248384873' and even '110_2' simple?

A few words about the simplicity of base 10 notation may be in order. Base 10 notation is so useful because it is a place notation; which is to say that its expressions are (disguised) polynomials, not simple, and subject to the handy laws of polynomial manipulation. One might say that decimal notation has a dual aspect—and it is only its "successor" aspect, not its full polynomial structure, that is playing a role in *de re* ascriptions. (See the discussion of **0**, **S0**, **SS0**, . . . below.) A more linguistically sophisticated discussion would differentiate between compositionality of lexical items and true syntactic compositionality. In this respect objecting to numerals as proper names because of their place notational aspect is like objecting to quotation names as proper names because of their blatant structure, (see Mark Richard [20]). The case of Gödel numbering renders this analogy even more cogent.

My strategy is to review how intensional facts are handled within standard formalized theories of arithmetic and then to see how much can be transferred to natural language. It is thus fortunate that we can locate intensional contexts within mathematics—in particular metamathematics. Going back, at least, to Feferman's rigorous treatment of the Gödel Second Incompleteness Theorem and continuing with the various modal treatments, we do find a rich source of intensionality within classical metamathematics. The formalization *in arithmetic* of "It is provable that. . . ," occasioned by the Gödel Second Theorem, will provide our source of examples.[7] But first some preliminaries on formal numerals.

2.2 In formalizations of arithmetic it is common to refer to the "standard numerals". There are two notions employed in our formalization practice. On one notion, we single out an arbitrary infinite set of primitive constants (0-place function symbols), referring to them by not so arbitrary meta-linguistic names: $\mathbf{c_1}$, $\mathbf{c_2}$, $\mathbf{c_3}$,. . . . The relevant assignments, infinitely many, are given by I($\mathbf{c_n}$)=n.

Using the other notion gives the standard numerals syntactic structure. The numerals are **0**, **S0**, **SS0**, . . . , or some such, where I(**0**) = 0 and I(**S**) = the successor function.[8] Our more usual formalizations of arithmetic are in this vein. Choosing the first method, in the presence of the successor function, results in competitors for the role of standard numeral for 2.

On the second account standard numerals are not syntactically simple, and on the first account the assignment is parasitic on the notion of an inductive sequence in the meta-language. Moreover, every number will have many, infinitely many, terms that name it according to the standard interpretation. Nothing said so far about our formalization practice awards the standard numerals any special status, other than, perhaps, the meta-linguistic stipulation: "These are the standard numerals".

Of course we have left out something. Our interest is partly in the logical behavior of our numerals, and we have omitted the deductive apparatus from our sketch of a formalism. The standard numerals will play a special role in any adequate formalization. Thus, consider Peano arithmetic. In formalizing that every number except 0 is a successor, the standard numeral for 0 is used. More centrally, in the statement of induction we see both **0** and **Sx** making canonical appearances. Here we see a proof-theoretic distinction marking the standard numerals. But it is one that requires us to take the particulars of the formalization seriously—to regard the formal objects in question as distinguished by their axiomatization and not by the set of theorems. This observation will be reinforced below; for the present it should be noted that in a formalization with a rich(-enough) term-forming capability not every true identity between terms will be derivable.[9] This last implies that in axiomatizations that deviate from the usual only in that non-standard "numerals" replace the standard numerals, the usual *axioms* may not be derivable. Such a system would surely represent knowledge distinct from Peano arithmetic. In section 5, an example in this spirit is developed.

2.3 Returning to method one, which formalized standard numerals as syntactic simples, we see that it has numerous defects, for our present purposes. (1) We still have to axiomatize the relation of such simples to functionally constructed ones.[10] (2)

There is an element of "cheating" involved since the productive nature of the naming rule is really just shifted to the meta-language. (3) Formalizing the relevant facts about numerals results in the *de facto* importation of method two into the formalism. (This, in effect, elaborates defect 2).

Let us, then, fix our attention on a standard formalization of Peano arithmetic, **P**, with **0** and **S** appearing in the axioms and induction scheme, with **+** and **·** as two-place function constants. I will refer to **0**, **S0**, **SS0**, etc. as the standard numerals. As a conceptually important terminological convention I use 'derivable', 'derivability', etc., to express a property of formulas of a formalism and 'provable', 'provability', etc., to express a property of propositions. Thus: Gödel *proved* the First Incompleteness Theorem for **P** and claimed that its formalization could be *derived* in **P**.

3 To this point I have raised the possibility that the behavior of formal numerals may aid in explicating two entwined semantic phenomena: exportable terms and the formal representation of arithmetic dicta. I have noted a proof-theoretic distinction concerning our standard numerals; **0** and **S**, by their mere appearance in the axioms, play a special role in the axiomatization of **P**. As noted above, there can easily be terms whose co-designation with some standard numeral will be underivable in the formalism. This is but one ramification of the Gödel Theorems; the price of repairing all such "weaknesses" will be inconsistency. For present purposes, this "weakness" is not just a necessary fact, but a semantically desirable one. As an initial step toward establishing the latter, we look at some propositional attitudes and arithmetic dicta.

A rich stock of examples is extractable from the proof-theoretic skeleton supporting the Gödel Theorems. We will add to our sample *dicta*, in order to bring these examples to bear. Consider formal objects of various syntactic categories as associated with numbers by some usual Gödel numbering. This correspondence makes no mention of the axiomatization of **P**; the same old interpretation of **P** now induces a way of reading the formulas of **P** as syntactic remarks. It is, of course, a different collection of predicates of (conservative extensions of) **P** that will be of interest; typically, those whose extensions are the usual syntactic categories. One useful way of looking at this Gödel-numbering procedure, one which keeps the ontology of numbers, leaves the interpretation (*extensionally* construed) the same but alters the standard specification of the interpretation. Crucial to this Gödelization is the fact that **P** is unchanged, its model-theoretic interpretation is unchanged, and only the names have been changed to permit *dicta* about formalisms, **P** in particular.[11]

Edna has taken enough logic courses to acquire, coincidentally, exactly that set of beliefs about formalisms that are provable in **P**. She retains her arithmetic beliefs, which are represented by exactly the same formulas, albeit with respect to a different specification of the interpretation of **P**.[12] Now, let Edna acquire yet another belief; the belief that **P** is consistent. Now this belief is representable, with respect to our Gödelization, as a formula of **P**. It is well-known (although not to Edna until just now) that such a formula is not derivable in **P**; although **P** can't prove this either. Let **P**$+$ be **P** with **P is consistent** added as an axiom; that is, **P**$+$ represents Edna's current beliefs about **P**.[13]

3.1 Edna's situation differs from Ralph's. Ralph is a competent speaker of English and has had a short and dismal introduction to logic. He has seen a formalization of Peano arithmetic, and has been introduced to the idea of a subsystem of **P**.[14] Ralph believes that the largest consistent subsystem of **P** is consistent, this being a somewhat easy belief to come by. Edna's belief that **P** is consistent and Ralph's belief that the largest consistent subsystem of **P** is consistent are radically different beliefs, although **P** is the largest consistent subsystem of **P**. Note that '**P** is consistent' and 'The largest consistent subsystem of **P** is consistent' differ only by a co-extensive term.

Returning, at last, to the *de re* arena, it seems that in Edna's case, but not in Ralph's, we would accede to exportation. That is, Edna believes *of* **P** that it is consistent while Ralph fails to hold such a *de re* belief.

That this is the case is signalled, in English, by the use of a proper name like '**P**' in Edna's case, versus a Bertrand-Russell's-yacht-like definite description in Ralph's. The question now is, notwithstanding our blithe use of the bold-facing convention for picking out apposite formal representatives, whether these distinctions of *dicta*, and of suitability for exportation, can be adequately reflected in the formal machinery. The brief answer is: Yes, and they already are. Indeed, all I have been offering is a popularization of the central complication of the Gödel Second Incompleteness Theorem.[15] **P** *already* proves Ralph's belief, whereas **P+** is (extensionally) distinct from **P**. To see this requires a brief sketch of a technical treatment of the Gödel theorems. These formal treatments yield the beginnings of an account of direct reference to numbers and to formalisms.

4 First some negative results. The most central requirement on a bold-facing mapping, a mapping from sentences, predicates, and terms of English to sentences, predicates and terms of **P** is the following:

(R1) **p** is derivable in **P** only if it is provable in **P** that p,

and perhaps

(R2) **p** is true only if p

as well.

Those who have seen a proof of the Gödel First Incompleteness Theorem might think that an appropriate and respectable notion is at hand—numeralwise expressibility.[16] In proving the First Incompleteness Theorem a formal predicate is constructed that numeralwise expresses *is a derivation of*, and from this predicate is constructed the famous Gödel sentence. Letting **Pf(x,y)** be an arbitrary predicate that numeralwise expresses *is a derivation of*, then the Gödel sentence is $\neg\exists$**xPf(x,k)**, where *k* is the Gödel number of a formula provably (in **P**) equivalent to $\neg\exists$**xPf(x,k)**. Under the syntactic "reading" of the interpretation it is natural to regard this formula as saying that that very formula is not derivable. Many formulas numeralwise express *is a derivation of*, and any of them will suffice for the First Theorem; but not all of them will suffice to support such a reading. Here's why.

In establishing the Second Theorem, half of the First Theorem is shown to be formalizable in **P**; namely the implication from the consistency to **P** to the underivability of the Gödel sentence. It is then remarked that the consequent here is provably equivalent to the Gödel sentence; hence, given that *modus ponens* is a rule of **P**, the formula formalizing the consistency of **P** must be underivable if **P** is consistent. This talk of "formalizing" is talk of the bold-facing relation. What might such a $\mathbf{CON_P}$ look like?

$\neg\exists$**xPf(x,** $\ulcorner$**0=1**$\urcorner$**)** is a likely candidate, at least in general form.[17] Under the syntactic reading, again, it is natural to read this formula as saying that 0=1 is not provable (by (R1) and **0=1** is not derivable). Of course, if any formula is not derivable, the formalism is consistent; so "the" Gödel formula itself is a consistency sentence, provided it says what it seems to. Unfortunately for the fate of numeralwise expressibility as an explication of bold-facing, this candidate fails to achieve the desideratum (R1). Of the many co-extensive formulas that numeralwise express *is a derivation of* there are some deviant proof-predicates that, playing the role of **Pf** in $\neg\exists x\mathbf{Pf}(x,\ulcorner 0{=}1\urcorner)$, yield derivable formulas. To maintain (R1) in the face of this would be to deny the Gödel Second Incompleteness Theorem.[18]

Furthermore, considering formalisms generally, and not just **P**, (R2) is also violated. There are formalisms such that some consistency sentences constructed as above are true, yet the formalism is not consistent.

4.1 Conveniently enough, it is reasonable transcriptions of Ralph's paltry belief that supply examples of "proof predicates" sufficient for the First Theorem, but inimical to an extensional account of *says that*.[19] Let $\mathbf{Pf}^\star$ be

$$\mathbf{Pf(x,y)\ \&\ \neg\exists x(x<y\ \&\ Pf(x,neg(y))}\ ,$$

which reads "*x* is a derivation of *y* and there is no smaller derivation of the negation of *y*." $\mathbf{Pf}^\star$ is co-extensive with **Pf** and, for consistent formalisms, numeralwise expresses what **Pf** does. A more stripped down Rosser-style predicate is $\mathbf{Pf}^{\star\star}$:

$$\mathbf{Pf(x,y)\ \&\ \neg Pf(x,\ulcorner 0{=}1\urcorner)}$$

The result of replacing **Pf** with either $\mathbf{Pf}^\star$ or $\mathbf{Pf}^{\star\star}$ in the "consistency" formula is a trivial theorem of logic and hence of **P**.

This dooms numeralwise expressibility as a sufficient condition for capturing *dicta*. Given how this construction mimics Ralph's simple state of mind, this is hardly unfortunate. What Ralph believes and what the deviant "consistency" sentences say might be broadly and loosely stated as: **P** is consistent, given that **P** is consistent; otherwise some subsystem is. In the context of meta-mathematics (R1) is no *ad hoc* principle; glossing the Second Incompleteness theorem as being about the *unprovability* of consistency requires that this unprovability be entailed by the *underivability* of a certain formula. The formal facts recreate, in some generality, the semantic mechanisms of "It is provable in **P** that **P** is consistent" vs. "It is provable in **P** that the largest consistent subsystem of **P** is consistent."

4.2 Accounts that neither violate our intuitions about what formulas say nor permit the violation of the Gödel Second Theorem are to be found in rigorous proofs of the Second Theorem. By placing stricter constraints on the proof predicate than numeralwise expressibility, such proofs construct consistency formulas that are underivable (and hence can't be any of the deviant ones).

In Feferman's generalization of the Gödel Second Incompleteness Theorem the bold-facing mapping of complex syntactic notions is achieved by straightforward transcription of their (often inductive) definitions. In particular, the proof predicate is a complex formula that encodes a usual textbook definition of *is a derivation of*. The basis of such a definition is the set of axioms. What varies is the mode of presentation of the axioms.

How is this reference to the axioms handled? Many distinct open sentences will numeralwise express the same set of axioms. Only certain open sentences that numeralwise express the axioms of **P** *really* express the axioms of **P**. Feferman is able to characterize a property, being an “RE-formula”, that guarantees correctness. This approach individuates formalisms by their “presentation”—and co-extensive presentations are not intersubstitutable in the context of the Second Theorem.

It is worth emphasizing that the restriction to RE-formulas is not *ad hoc* on either the positive or negative sides. The RE-formulas capture in general what Gödel went to some pains to achieve in his original paper–definitions of recursive relations whose very form guarantees their recursion-theoretic nature.[20]

On the negative side, we can see that the non RE-formulas pick out the axioms “accidentally”, or rather, like the deviant proof predicates, *via* descriptions bizarre enough to carry a trivial assurance of consistency.

If we let these formulas represent the analyses associated with terms referring to formalisms we can describe matters as follows: If Edna believes some formalism consistent, it is worth asking: “Under what meaning?” If the meaning is non-RE, as in Ralph’s case, we won’t care much. If the meaning is RE then we get to ask to which coextensive formalisms her proof (or basis of belief, whatever it is) extends. If Edna’s grounds are sound, all co-extensive formalisms are certainly consistent—but which are they? For RE meanings certain theorems are available concerning provable coextensiveness; in the normal case, Edna’s proof will transfer *if* we can prove that the second formalism is co-extensive to Edna’s. This is true even in those cases, like Ralph’s, where the first description is non-RE but ours is RE. In such a case, though, we know that a proof of co-extensiveness will need, as the crucial lemma, the consistency of the formalism under an RE-description!

In sum, Feferman gives us a rigorous account of the difference between Edna’s and Ralph’s beliefs in terms of a property of what can aptly be called the mode of presentation of the formalism. RE-formulas permit *de re* attributions; they permit exportation of their associated terms (e.g., ‘**P**’ in our technical English). Puns aside, this gives rise to two distinct questions:

(1) What is the connection between the canonicity of RE-formulas and the behavior of the standard numerals?

(2) Why are only certain fixings of the referent suitable for exportable terms? (Cf.: Let Alma be the smallest spy born in the 21st century).

We turn to a more transparent account of the Gödel Theorems that is designed to specifically highlight the role of numerals in proof theoretic contexts, and that will help us to link this technical material to our initial concerns.

5 The Feferman treatment of non-deviance touches our concern with standard numerals at two places: the involvement of induction and the direct involvement of standard numerals. The definition of a derivation is, of course, an inductive one; the direct involvement of the standard numerals occurs in the definition of numeralwise expressibility *via* the occurrence of the $\ulcorner\urcorner$ operator. For the moment, I merely point at the explicit centrality of induction and take up the non-fortuitous ubiquity of the standard numerals. I return later to the problem of induction.

Here’s the strategy: Enrich the language of Peano arithmetic with extra constants $\mathbf{c_1}$, $\mathbf{c_2}$, $\mathbf{c_3}$,..., which we take to name certain numbers. These will be introduced purely semantically and various useful theorems can be proved with their aid. These constants, though syntactically primitive, will turn out *not* to be standard numerals.

This lack on their part will have consequence (or rather lack of consequence) only when we get to a theorem involving propositional attitudes—namely the Second Incompleteness Theorem.

5.1 Consider the language of arithmetic enriched by some predicates with a specifically syntactic interpretation. Let $\mathbf{D_1}$ be interpreted to be true of a number *n* just in case *n* is a wff derivable from some given set of wffs that axiomatize **P** and that **neg** is interpreted as the function that takes an expression to its negation. Note that this is all at the level of interpretation and is not proof theoretic. Let PA⋆ be this language together with new constants $\mathbf{c_1}, \mathbf{c_2}, \mathbf{c_3}, \ldots$. Let $\varphi_{\mathbf{1}}(\mathbf{x}), \varphi_{\mathbf{2}}(\mathbf{x}), \ldots$ be all the wffs of PA⋆ with *x* as sole free variable. Interpret $\mathbf{c_i}$ as follows:

$$\mathrm{I}(\mathbf{c_i}) = \varphi_{\mathbf{i}}(\mathbf{c_i})$$

Lemma 5.1 (Fixed Point Lemma) *For every* $\varphi_{\mathbf{i}}$ *there is a wff* **S** *and a constant* **c** *such that;*

$$I(\varphi_{\mathbf{i}}(\mathbf{c}) \equiv \mathbf{S}) = \mathit{TRUE} \text{ and } I(\mathbf{c}) = \mathbf{S}.$$

That is, for every property of expressions (expressible in the language of arithmetic), there is a sentence (of the language of arithmetic) that "says that" it (that sentence) has the property. At the moment, this is based only on the specification of the interpretation; the proof-theoretic enforcement of such an interpretation is to come.

Proof: The proof of the Fixed Point Lemma is made trivial by the choice of $\mathrm{I}(\mathbf{c_i})$. For let $\mathbf{c} = \mathbf{c_i}$, $\mathbf{S} = \varphi_{\mathbf{i}}(\mathbf{c_i})$. Then $\mathrm{I}(\mathbf{c_i}) = \varphi_{\mathbf{i}}(\mathbf{c_i})$[21]

Let $\mathbf{D_2 t_1 t_2}$ hold just in case $\mathrm{I}(\mathbf{t_1})$ is a derivation of $\mathrm{I}(\mathbf{t_2})$. Further:

(⋆) Let $\mathbf{D_2}$ numeralwise express *is a derivation of*.

Let **Thm x** be $\exists \mathbf{y D_2 y x}$. For some *g*, ¬**Thm x** is $\varphi_{\mathbf{g}}$. Let **G** be $\varphi_{\mathbf{g}}\mathbf{c_g}$, i.e. $\neg\exists\mathbf{y D_2 y c_g}$. Suppose $\mathbf{P} \vdash \mathbf{G}$. Let *n* be the derivation of **G**. Then

$$(\star\star)\ \mathbf{P} \vdash \mathbf{D_2 n c_g}.$$

So $\mathbf{P} \vdash \exists\mathbf{y D_2 y c_g}$. Thus $\mathbf{P} \vdash \neg\, \mathbf{G}$, so **P** is inconsistent. Hence if **P** is consistent, not $\mathbf{P} \vdash \mathbf{G}$.

The formalization of this proof is the heart of the Second Theorem. It establishes that $\mathbf{CON_P} \rightarrow \neg\exists\mathbf{y D_2 y c_g}$ (i.e., $\mathbf{CON_P} \rightarrow \neg \vdash \mathbf{c_g}$) is a theorem of **P**; that is, that $\mathbf{CON_P} \rightarrow \mathbf{G}$ is a theorem of **P**.

(⋆ ⋆ ⋆) Let $\mathbf{f_2 xy}$ be interpreted as $\mathbf{D_2}\ulcorner\mathbf{x}\urcorner\ulcorner\mathbf{y}\urcorner$.

This enables us to write:

$$\mathbf{(x)(y)((D_2 xy \rightarrow \exists z D_2 z\, f_2 xy)\ \&\ (\neg D_2 xy \rightarrow \neg\exists z D_2 z\, neg f_2 xy))}.$$

This is the formalization of the numeralwise expressibility assumption and is needed as part of the formalized argument.[22] Since this, and other more minor syntactic facts, can be derived in **P**, the argument for the First Theorem can be reproduced in **P**, yielding $\mathbf{P} \vdash \mathbf{CON_P} \rightarrow \mathbf{G}$ and hence not $\vdash \mathbf{CON_P}$. There are several technical lacunae in this rough sketch, but there is one of present interest.

The interpretation of $\mathbf{f_2xy}$, bruited in ($\star\star\star$) is a two-place function, mapping a pair of numbers (i.e. syntactic objects) to the sentence formed by the predicate $\mathbf{D}_2$ filled in with the standard numerals for x and y. This means that the step to the formalization of ($\star\star$) is *not* warranted merely by our the formalization of ($\star$); for ($\star$) is about numeralwise expressibility which utilizes the standard numerals, whereas ($\star\star$) contains $\mathbf{c_g}$. Here, and in other steps, we need that the $\mathbf{c_i}$ are standard names; i.e., $\mathbf{P} \vdash \mathbf{c_i} = \mathbf{n}$ just in case $\mathrm{I}(\mathbf{c_i}) = \mathrm{n}$. Indeed, if the $\mathbf{c_i}$ had different, proof-theoretically tricky, properties the formalization of ($\star$) will not be applicable. Recalling the reading of the Fixed Point Lemma, the underlying mechanism of the Gödel Theorems, we see that the reading given of it is supportable only if $\mathbf{P} \vdash \mathbf{c_i} = \mathbf{n}$. This is a technical analog of restrictions on arbitrary dubbings; the reference fixing description cannot be just any definite description. And what is at the heart of the involvement of the standard numerals is their intimate connection with induction.

5.2 It is instructive in this context to consider a related form of the deviant expression in arithmetic of syntactic propositions. It is clear from the proof of the First Theorem that each instance of the Gödel sentence or the consistency sentence is derivable. That is, for every x; $\mathbf{P} \vdash \neg\mathbf{Pf}(\mathbf{x}, \ulcorner \mathbf{0{=}1} \urcorner)$; it is a primary moral of the First Theorem that the quantifier in the preceding cannot pass through the turnstile. For, we only have two ways of understanding "for every x; $\mathbf{P} \vdash \neg\mathbf{Pf}(\mathbf{x}, \ulcorner \mathbf{0{=}1} \urcorner)$". Most plausibly it means: $\mathbf{P} \vdash \neg\mathbf{Pf}(\mathbf{0}, \ulcorner \mathbf{0{=}1} \urcorner)$ & $\mathbf{P} \vdash \neg\mathbf{Pf}(\mathbf{1}, \ulcorner \mathbf{0{=}1} \urcorner)$ & ..., which is true, and, in effect, lops off the non-standard numbers. Indeed, construed this way, it is formalizable and derivable. Its formalization would contain the numeral for $\neg\mathbf{Pf}(\mathbf{x}, \ulcorner \mathbf{0{=}1} \urcorner)$, the substitution function and the provability predicate, all within the scope of the universal quantifier. The quantifier, however, passes through the filter of standard naming. If it didn't, if $\ulcorner\urcorner$ took formulas to terms like the $\mathbf{c_i}$, in the absence of $\vdash \mathbf{c_i} {=} \mathbf{i}$, then the substitution version might not be derivable. The alternative reading, which is simply the Gödel sentence, is not equivalent and, of course, not derivable.

What sense other than substitutional could be given to "for every x; $\mathbf{P} \vdash \neg\mathbf{Pf}(\mathbf{x}, \ulcorner \mathbf{0{=}1} \urcorner)$"? The substitutional enforces the default intent that the range of the universal quantifier be just the standard integers; that is what we mean in English by "for all n". Now the construction of $Prf(x, \ulcorner 0{=}1 \urcorner)$, as Feferman does it, for example, gives a sense to quantifying in, such that $\forall\mathbf{x}\neg\mathbf{Pf}(\mathbf{x}, \ulcorner \mathbf{0{=}1} \urcorner)$ is a perfectly ordinary (though lengthy) formula of $\mathbf{P}$ and one which adequately captures not $\vdash \mathbf{0{=}1}$. "Adequately captures" here has a straightforward unpacking, as in (R1)—any *proof* we had of not $\vdash \mathbf{0{=}1}$ would fail to transcribe as a derivation of $\mathbf{P}$.

6

> [A] problem is cognitive relations, including *de re* propositional attitudes; if I see on a blackboard the formula "$(x)(\neg x = 0 \rightarrow \exists y(x = Sy))$", I do not see the number that corresponds to it under some arithmetization of the syntax of first-order arithmetic .... What is basic to the concept of type gives identity and difference relations only to other types in the same system of symbols.... [T]his kind of consideration does show a significant disanalogy be-

> tween this kind of mathematical intuition and ordinary perception. *What is intuited* depends on the concept brought to the situation by the subject. (Parsons [17], p. 161)

The semantic mechanisms formalized above do, as Parsons would insist they have to, go beyond intuition. Indeed, the concept "brought to the situation" *includes* just the one that Parsons seems to mark as beyond the limit of (Parsons' version of Kant's notion of) intuition—induction. We have seen that what marks the standard numerals as such is their place in the induction scheme. Similarly, what enables **P** (and us) to encode the notion of a formal system is its (and our) ability to "follow" an inductive definition, namely the definition of *is a derivation of*. This view of the mode of presentation of numbers and formal objects has some consequences and some problems.

For example, it leads to the following claim about certain versions of the Second Theorem. Although the First Incompleteness Theorem can be proved *about* weak systems, such as finitely axiomatizable **Q**, it can only be proved *in* systems with induction. The correct synopsis of this situation is that no formula of a weak system expresses the consistency of that system. Sufficiently weak-minded Ralphs couldn't even entertain the proposition that $\mathrm{CON}_{\mathrm{R}}$, much less believe it.[23]

Other consequences concern the application of the technical facts to problems of direct reference. In one sense we have arrived at the not at all startling claim that the mode of presentation of the integers involves the inductive presentation of the integers. This is hardly news—it occupies a niche in a long tradition that ties our cognitive relations to the integers to our grasp of the notion of a progression, or the notion of adding one and going on in the same way.

The novelties lie in: (1) the location of this mode of cognitive relation to the integers in a specific place in a semantic scheme—as the warrant for direct reference to integers; and similarly for (Gödelized) formal objects; (2) the discovery of prior proof-theoretic enforcement of the distinction between terms of direct reference and others; (3) an approach to the solution of some problems of reference to abstract objects.

There are, however, problems that arise whenever the particular natures of numbers and formalisms begins to play a role. We need to say a little more about the special nature of the modes of presentation of integers and of formalisms.[24]

Fixing the referents for numerals is a wholesale affair, tied to an inductive scheme. There is no such thing as piecemeal, one-by-one, referent fixing in the realm of numbers—unless, of course, it is parasitic on a prior wholesale fixing. We also observe that certain modes of presentation of numbers are privileged, in that they support direct reference.

Parsons' conundrum, presented in the epigraph, now goes like this. We would like '6' and '110_2' to directly refer to 6 and to assimilate solutions to the Puzzling Peano worry to whatever solution seems to work for analogous puzzles of direct reference (whether this takes the form of a triadic belief relation approach, a dyadic belief approach that de-privileges our grasp of propositions, or some other). The deep problem is to avoid having direct reference to (or belief about) formalisms and other syntactic objects be direct reference to (or belief about) numbers. To do this we need to see if Parsons' suggestion, concerning the concept of type, can be recreated in the current context. I think it can. We need to take the inductive reference fixing of names of syntactic entities as being only synthetically related to the reference fixing of numbers.

Fixing the referent of syntactic entities purely extensionally, through the Gödelization, is deficient. This negative thesis is supported by the bad behavior of the $\mathbf{c_i}$ in section 5. However, the treatment in that section is not detailed enough to support a more positive thesis concerning how reference to syntactic entities can be independently handled, without parasitism on the standard numerals. The Feferman treatment, sketched in section 4, is a useful beginning. The mode of presentation of a formalism is reduced to a formula that picks out the axioms in a non-deviant way. It is not misleading to conceive of this as representing a formalism within **P** by transcribing its presentation as it would appear in a rigorous logic text. (Part of Feferman's contribution is to show how this can be done in a uniform way across a wide class of formalisms.) This would include the inductive definitions of the various syntactic categories of a formalism up to and including *is a derivation of*. Fully breaking the spell of Gödelization requires an even more principled approach that constructs a canonical theory of formalisms in the language of syntactic theory (with concatenation as a primitive, for example). In either of these last two approaches the key feature is that the inductive definitions and proofs proceed within the natural syntactic categories and are only related to numbers *via* the semantically accidental process of arithmetization. It is only at this last step that we are tempted to say that a formula of an arithmetically construed formalism says syntactic things. And this step we can do without.[25] Thus the realm of numerals and the realm of terms referring to formalisms are distinct, and we can regard our use of arithmetization as a convenient heuristic for representing some of the important features shared by reference to numbers and reference to formalisms. In the terminology of the epigraph the concepts brought to the two situations (reference to numbers, reference to formalisms) are represented by the different inductive schemes in number theory and formal systems theory. Arithmetization is semantic accident.

6.1 My development of Ackerman's proposal has, therefore, two primary virtues. It reveals a deeper semantic kinship between reference to abstract and concrete objects than Kaplan, for one, was inclined to credit. It also marks an epistemic boundary at an independently defensible place. Much more could be said concerning the relationship between the standard names of proof theory and the proper names of ordinary language. Much more could be said concerning the concepts brought to our conceiving of numbers and our conceiving of formalisms. Nothing ontologically astonishing about numbers has been revealed, any more than the nature of cities is unveiled in treatments of Kripke's puzzle or the nature of persons by accounts of direct reference to people. I hope to have indicted how it is possible that numerals give us reference freed, not just of empirical, but of synthetic vicissitudes.[26]

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NOTES

1. As an exercise one could Putnam a Frege-Russell and run a Twin Earth conundrum reminiscent of Benacerraf's worries over set-theoretic "identifications" of numbers in his [4].

2. See her [1], p. 147. *Some* remarks in Kaplan point away from this otherwise straightforward reading of his stance. The "always" of "they always pick out the same number", in the characterization of reliability, leaves room for interpretation(!)—Kaplan's reference to his dissertation would suggest reading "always", not as "in every possible circumstance", but as "in all models." Room for interpretation is similarly left in the space between freedom from empirical vicissitudes (necessity) and his "linguistic grounds". The considerations in subsequent sections will supply boundaries for this space.

3. Cf. Tyler Burge's [9]. Burge asserts that "beliefs attributed with 'Pegasus' are sometimes not *de re*."

4. This problem crosses the abstract/physical object boundary. One attractive class of solutions, due to Kaplan, Perry and others is to treat belief as a triadic relation between a person, a sentential meaning, and a proposition. For an interesting discussion and treatment of a difficult case, see Mark Richard's [19].

5. I borrow Nathan Salmon's terminology of "guises", because of its non-technical "sound" matches the looseness with which I use it.

6. Although I doubt that 'quatre-vingt-seize' is a variant spelling of '96'.

7. The historical impetus was Hilbert's insistence that meta-mathematics become mathematics; Gödel obliged. Formal arithmetic, *construed as arithmetic*, is extensional; its reinterpretation as its own (partial) metatheory isn't. The nice fact, uncovered by treatments of the Second Theorem, is that the intensionality of meta-mathematics can be straightforwardly represented in formal arithmetic, (see also note 12).

8. Designators of formal objects will be in bold face; this convention will extend to using designators of English to pick out corresponding (according to the interpretation in effect) formal objects. '2' refers to a number, but '**2**' refers to a term whose interpretation is given by I(**2**)=2. '$\mathbf{c_1}$', '$\mathbf{c_2}$', '$\mathbf{c_3}$' might just as well have been '**1**', '**2**', '**3**'.

9. A simple way to see the essence of this observation is as follows: Let **G** be the Gödel sentence; then $\boldsymbol{\iota x((G \ \& \ x=5) \vee (\neg G \ \& \ x=0))=5}$ is true but not derivable. Lifting our eyes for a moment from Peano arithmetic, what should we say about systems without induction? After all, if the standardness of **0** and **S** are linked to their appearance in the induction scheme, will standardness vanish in systems without induction? Yes. The formulas of such systems are best thought of as not "saying" anything much, (cf. note 23).

10. Mark Steiner, in his [24], p. 29, discounts an anti-logicist worry concerning the predicate *NNx*, given by $(F)(F0 \ \& \ (y)\ [Fy \rightarrow FSy] \rightarrow Fx)$, and the numerals **0**, **S0**, **SS0**,. . . . The discounted worry is, roughly, that the proof that *NNx* is true of all and only **0**, **S0**, **SS0**,. . . , requires induction. Cf. the "lacuna" discussed in section 5.

11. A fuller account of this notion of "reading" a formula is presented in Auerbach [3].

12. For what must be meant here by '**P** proves that φ' is that one of **P**'s theorems is a formula whose English translate, relative to the standard specification of the interpretation, is φ. Using the notational convention in force, we can say that **P** proves that 2 + 2 = 4 just in case **2 + 2 = 4** is derivable in **P**. The constraints on our "bold-face mapping" are more severe than those provided by a Frege-Tarski extensional semantics.

13. *We* are representing Edna's two disjoint sets of beliefs by the same axiomatization; this example is mute concerning the psycholinguistic matter of Edna's internal representations. However, they certainly aren't represented one by one; and, plausibly, Edna's finitely presented representation of the theorems of **P** *qua* syntactic beliefs will not be a presentation of the Peano axioms. As to the underlying logical form of **P is consistent**, the reader is referred to the works mentioned in note 15. The disjointness of Edna's arithmetic beliefs from her syntactic beliefs is discussed in the last section.

14. Many different notions of subsystem will do here. For Ralph's upcoming belief state he need understand very little about the notion of subsystem; no more, in fact, than the bare terminology suggests. For concreteness the following will do: the *n*th subsystem is the formalism characterized by the axioms $< n$. **P** is not finitely axiomatizable, and so will have infinitely many subsystems in this sense, no matter what representation of **P** is settled on.

15. Many technical details are suppressed below; no complete treatment is to be found here. The reader unacquainted with adequate proofs of the Gödel Second Incompleteness Theorem will have to take much on faith. The principle article of faith is that the representation of the consistency sentence for a formalism as **P is consistent** is not wildly misleading as to the consistency sentence's logical form. The incredulous will wish to consult: Boolos [6]; Boolos and Jeffrey [8]; Feferman [11]; Jeroslow [13]; and Monk [16]. The following are not as resolutely technical but do stress the relevant complications: Auerbach [2]; Boolos [7]; and Detlefsen [10]. The Second Theorem is not as well understood as the First and is consequently scanted in popularizations of the Gödel results.

16. Numeralwise expressibility is a three-place relation among formal systems, relations or properties of numbers, and predicates of formal systems. A formal predicate that numeralwise expresses a relation in an arithmetically correct formal system is thereby guaranteed to be extensionally correct with respect to that relation. More precisely: φ numeralwise expresses *R*, *R* an *m*-place relation, iff

    i) $\mathrm{R}(n_1,...n_m) \rightarrow \vdash \varphi(\mathbf{n_1},...\mathbf{n_m})$

    ii) $\neg\,\mathrm{R}(n_1,...n_m) \rightarrow \vdash \neg\varphi(\mathbf{n_1},...\mathbf{n_m})$,

    where **n** is the standard numeral for *n*.

17. N.B. '**0=1**' won't do here. '**0=1**' is a notation for a formal sentence—what is required is a notation for a term that stands for the (number of that) sentence. So $\ulcorner\urcorner$ maps numbers (i.e., Gödelized syntactic objects) to closed terms, namely the standard numerals. That is, $\ulcorner\mathbf{0{=}1}\urcorner$ is the numeral for **0=1**. What makes this very like quotation is that it results in standard names. In proof theory this context even supports quantifying in. This is one of those rare instances where the use/mention distinction bears much philosophical weight.

18. Note that the standard gloss of the Second Theorem ("No sufficiently strong formal system can prove its own consistency.") demands a semantics for the formalism; the deviant proof predicates show that this cannot be an extensional semantics. For an elaboration of this and related points, see Auerbach [2]. Note also that I have yielded to custom in using "provably equivalent" and "proof-predicate" where my advertised convention would demand "derivably equivalent" and "derivability-predicate".

19. Better than sufficient. These deviant proof predicates arise from Rosser's enhancement of the Gödel result and are often called Rosser-type predicates.

20. So a prover of the First Theorem shows that the definitions pick out numeralwise expressible sets by adverting to the form of the definitions. When the prover in question is

P itself, as in the context of the Second Theorem, we need a formalization of appropriate form. This, in effect, is what Feferman gives us with RE-formula. An RE-formula is one that canonically, as a matter of form, picks out a recursively enumerable set.

21. Tarski's Theorem follows immediately. This low-overhead version of the Fixed Point Lemma comes to me from Harold Levin, who credits a version of it to Saul Kripke. Raymond Smullyan has published an interesting variant on this approach (see his [22]) in which one "constant" takes the place of all of ours; its interpretation changes with the formula in which it occurs.

22. The derivability of this formula plays the role in the present paper that F-LPC, Formalized local provability completeness, does in [3].

23. Proofs of a Second Theorem for weak systems are radically different in method; they are not proved by showing how to formalize a proof of the First Theorem. I would maintain that what they establish is at best a weak analog to the Second Theorem. See Bezboruah's and Shepherdson's [5].

24. I have said as little as possible about the full content of the mode of presentation, other than to point to the involvement of induction, and thus to the wholesale nature of the reference fixing. There are two reasons for this vagueness. First, I wanted to utilize the relatively simple facts about our pre-existing first-order codifications of arithmetic knowledge – codifications that indicate necessary conditions on appropriate modes of presentation. Secondly, strengthening these codifications to isolate sufficient conditions would not only complicate the technical machinery, but involve substantial foundational issues. For example, I think that a second-order characterization actually underlies our concept of number. And *part* of the reason for this involves the neglected issue of the cardinal, as well as the ordinal, aspect of number. Thus, while certain necessary conditions on the content of the mode of presentation for numbers are forthcoming, sufficient aspects are controversial and lie outside the present framework.

25. The best way to isolate the essential character of formalisms is to represent them as Post Canonical Systems. The classic here is R. Smullyan's [23], but see also Fitting's [12].

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