PROOF THEORY AND MEANING:

ON SECOND ORDER LOGIC

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Abstract: Second order quantification is puzzling. The second order quantifiers have natural and compelling inference rules, and they also have natural models. These do not match: the inference rules are sound for the models, but not complete, so either the proof rules are too weak or the models are too strong. Some, such as Quine, take this to be no real problem, since they take "second order logic" to be a misnomer. It is not *logic* but *set theory* in sheep's clothing [7, page 66–68], so one would not expect to have a sound and complete axiomatisation of the theory.

I think that this judgement is incorrect, and in this paper I attempt to explain why. I show how on Nuel Belnap's criterion for logicality, second order quantification can count as properly logic so-called, since the quantifiers are properly *defined* by their inference rules, and the addition of second order quantification to a basic language is *conservative*. With this notion of logicality in hand I then diagnose the incompleteness of the proof theory of second order logic in what seems to be a novel way.

The idea that inference rules can truly *define* connectives is compelling. Once we commit to inference rules such as these:

$$\frac{p \quad q}{p \wedge q} \, [\wedge I] \qquad \frac{p \wedge q}{p} \, [\wedge E_1] \quad \frac{p \wedge q}{q} \, [\wedge E_2]$$

the sense of '$\wedge$' is specified. We now understand logicians' *conjunction*. Once we learn the inference rules, we learn how to *use* the connectives in deductive reasoning. One does not need to be a disciple of Wittgenstein to think that there is a connection between meaning and use, and in inference rules like these the connection with use is present to hand. The $[\wedge I]$ rule tells us how to *get* a conjunction, and the $[\wedge E]$ rules tell us what we can *do* with a conjunction when we have it. Inference rules speak to the matter of the *use* of the concept of conjunction without positing some 'semantic value' to correlate with the concept.

1   THE TROUBLE WITH TONK

This picture — compelling to many, in what is now called an "inferentialist" tradition in semantics [2, 4, 5] — is not without its difficulties. Arthur Prior, in his influential paper "The Runabout Inference Ticket" [6] presents us one problem for this kind of inferentialism. He asks us to consider the binary connective "tonk" given by these two inference rules.

$$\frac{p}{p \text{ tonk } q} \text{ [tonk}I] \qquad \frac{p \text{ tonk } q}{q} \text{ [tonk}E]$$

Prior concedes that if we can freely define connectives with inference rules, we have defined *something*. However, using the concept tonk which we have apparently defined, we may infer any conclusion from any premise:

$$\frac{\dfrac{p}{p \text{ tonk } q} \text{ [tonk}I]}{q} \text{ [tonk}E]$$

This is too much: the trouble with tonk is the problem with all inference rules. Something has gone *wrong* in our definition. But what?

Nuel Belnap in his "Tonk, Plonk and Plink" [1], gives an inferentialist diagnosis of the puzzle: For Belnap, criteria governing the acceptability of a definition can be given in inferential terms. What makes tonk unacceptable is not some *categorical* feature of tonk, but in the interaction of tonk with the inference rules we already endorse.

> It seems to me that the key to a solution lies in observing that even on the synthetic view, we are not defining our connectives *ab initio*, but rather in terms of an *antecedently given context of deducibility*, concerning which we have some definite notions. By that I mean that before arriving at the problem of characterizing connectives, we have already made some assumptions about the nature of deducibility. [1, page 131]

The point is that rules for tonk are given relative to a background set of commitments concerning inference. (Which may involve, for example, the commitment that the argument from p to q is invalid.) If the addition of the rules for tonk change our commitments concerning inference (we now have a proof from p to q), then we are forced with a choice. Either reject those rules, or modify our original assumptions. Both are options. If we *endorse* the antecedent theory — or what Belnap calls our *antecedently given context of deducibility*, then this gives us a standard by which we can judge putative definitions.

> We may now state the demand for the consistency of the definition of the new connective, *plonk*, as follows: the extension must be *conservative*; i.e., although the extension may well have new deducibility-statements, these new statements will all involve *plonk*. The extension will not have any new deducibility-statements which do not involve *plonk* itself. It will not lead to any deducibility-statement not containing *plonk*, unless that statement is already provable in the absence of the *plonk*-axioms and *plonk*-rules. The justification for unpacking the demand for consistency in terms of conservativeness is precisely our antecedent assumption that we already had all the universally

> valid deducibility-statements not involving any special connectives. [1, page 132]

On these grounds, if the background account of deducibility included the commitment that some p did not entail some q, then, relative to that background, tonk fails the demand of consistency. This is *one* of the tests Belnap considers in the paper. In the case of a natural deduction proof theory or a sequent calculus, we can demonstrate that this criterion is met by means of a *normalisation proof* or a *cut elimination argument*, which usually has as a consequence a *subformula property*. These proof-theoretical results show that if one has a proof for some argument (or a derivable sequent) then we can find a special *normal* proof for that argument (or a *cut-free* derivation of that sequent) in which all formulas used in that proof are *subformulas* of the formulas in the premises or conclusion (or are subformulas of the formulas in the target sequent). This means that if the proof (or sequent) does not use our newly defined connective, then it has a normal proof (or cut-free derivation) not using the *rules* for our newly defined connective, for the rules for this connective must involve it somewhere, and the normal proof (cut-free derivation) does not feature this connective at all. It follows that if the argument is proved (sequent is derived) in the system *using* the new concept, then it can still be proved (derived) in the system *before* its introduction.

Belnap considered another criterion to add to the criterion of conservative extension.

> The mathematical analogy leads us to ask if we ought not also to add *uniqueness* as a requirement for connectives introduced by definitions in terms of deducibility (although clearly this requirement is not as essential as the first, or at least not in the same way). Suppose, for example, that I propose to define a connective *plonk* by specifying that B ⊢ A-*plonk*-B. The extension is easily shown to be conservative, and we may, therefore, say 'There is a connective having these properties'. But is there only one? It seems rather odd to say we have defined *plonk* unless we can show that A-*plonk*-B is a function of A and B, i.e., given A and B there is only one proposition A-*plonk*-B. But what do we mean by uniqueness when operating from a synthetic, contextualist point of view? Clearly that at most one inferential role is permitted by the characterization of *plonk*; i.e., that there cannot be two connectives which share the characterization given to plonk but which otherwise sometimes play different roles. Formally put, uniqueness means that if exactly the same properties are ascribed to some other connective, say *plink*, then A-*plink*-B will play exactly the same role in inference as A-*plonk*-B both as premiss and as conclusion. [1, page 133]

So, inference rules only truly *define* a connective if they go beyond *describing* some of its inferential properties, and go on to determine its behaviour in valid argument. This is the criterion of UNIQUENESS. Keeping on using Belnap's mathematical analogy, we will call his first criterion the EXISTENCE criterion. The antecedently given context of deducibility provides the space for the definition of some concept (it allows for its *existence*), if it may be conservatively extended to include it. The concept is defined *uniquely* if whenever we attempt to define it *twice*, the two concepts defined agree.

In the rest of this paper, I will use Belnap's criteria of existence and uniqueness to examine second order quantification, and to defend the idea that the inference rules for second order quantification truly define a logical notion.

2   PROPOSITIONAL LOGIC, AND OUR CONTEXT OF DEDUCIBILITY

Following Belnap's discussion, we can use the criteria of existence and uniqueness only relative to an 'antecedently given context of deducibility.' The context I will use for this discussion is Gentzen's sequent calculus for classical first-order logic. I have argued elsewhere that this calculus may be understood in terms amenable to an inferentialist semantics [8]. The core notion, a sequent of the form

$$\Sigma \vdash \Delta$$

where Σ and Δ are collections of statements — is to be understood as constraining assertion and denial: if a sequent is derivable, then a dialectical context in which all members of Σ are asserted and all members of Δ are denied is out of bounds: in the terminology I have used before, such a context is *incoherent*. The axiom of the classical sequent calculus

$$\Sigma, A \vdash A, \Delta$$

is straightforward: a position in which Σ is simultaneously asserted and denied is out of bounds.[1] Rules for connectives, then, give us an account of the propriety (or otherwise) of assertions and denials of statements involving those connectives. For example, the classical rules for negation:

$$\frac{\Sigma \vdash A, \Delta}{\Sigma, \neg A \vdash \Delta}\ [\neg L] \qquad \frac{\Sigma, A \vdash \Delta}{\Sigma \vdash \neg A, \Delta}\ [\neg R]$$

tell us that if denying A is out of bounds (along with asserting all of Σ and denying all of Δ) then so is asserting $\neg A$. This is the rule $[\neg L]$. Similarly, if asserting A is out of bounds (along with asserting all of Σ and denying all of Δ) then so is denying $\neg A$. This is the rule $[\neg R]$. General metatheoretical arguments (either cut-elimination, or soundness and completeness results) can show us that these rules for $\neg$ are conservative over various subsystems: it is not hard to show that, given an appropriate starting point, Belnap's *existence* criterion is met by rules for negation and the other propositional connectives. For *uniqueness*, it suffices to show that if we were to introduce the connective *twice*, they would prove to be equivalent. For negtion, we may reason as follows. First, we have these two derivations:

$$\frac{\dfrac{A \vdash A}{\vdash A, \sim A}\ [\sim R]}{\neg A \vdash \sim A}\ [\neg L] \qquad \frac{\dfrac{A \vdash A}{\vdash A, \neg A}\ [\neg R]}{\sim A \vdash \neg A}\ [\sim L]$$

So, any sequent involving $\sim A$ has the same status as that involving $\neg A$, using the following four derivations:

$$\frac{\sim A \vdash \neg A \quad \Sigma, \neg A \vdash \Delta}{\Sigma, \sim A \vdash \Delta}\ [Cut] \qquad \frac{\neg A \vdash \sim A \quad \Sigma, \sim A \vdash \Delta}{\Sigma, \neg A \vdash \Delta}\ [Cut]$$

$$\frac{\Sigma \vdash \neg A, \Delta \quad \neg A \vdash \sim A}{\Sigma \vdash \sim A, \Delta}\ [Cut] \qquad \frac{\Sigma \vdash \sim A, \Delta \quad \sim A \vdash \neg A}{\Sigma \vdash \neg A, \Delta}\ [Cut]$$

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[1] As I have argued elsewhere [8], this does not rule out paraconsistency or dialetheism: you need just to resist the step between asserting $\neg A$ and denying A. A dialetheist, on my account, is one who takes it to be coherent to assert $\neg A$ in some circumstances in which it is incoherent to deny A.

It follows that the two negations ($\sim$ and $\neg$) are interchangeable as far as coherence is concerned. Similar reasoning applies in the case of the other propositional connectives. In this 'context of deducibility' the standard propositional connectives satisfy Belnap's criteria of existence and uniqueness. For short, we will say that the standard propositional connectives are BELNAP LOGICAL.

3 RULES FOR QUANTIFICATION

What of quantifiers? Instead of the usual progression of first order quantification and then second order quantification, we will consider first order and second order quantification together. The starting point for adding first and second order quantifiers is not the quantifiers themselves, but the move to 'split the atom' of the atomic judgement. Instead of taking the atomic unit in our propositional language to be a structureless 'p' — we take it to have some structure of its own. In our new language, the fundamental judgement takes the form

$$Ft_1 t_2 \cdots t_n$$

where F is an n-place *predicate* and t_1 to t_n are n singular terms. In the simplest cases, predicates and terms have no internal structure of their own, but this is not an assumption we need make provided that our language *at least* contains atomic predicates and atomic terms (or, if you like, *names*).[2] Given some such language, we can close it under the propositional connectives as before. Nothing much is added to the *logical* complexity of the language until we add logical resources to exploit the new structure. And the great insight of logic since at least the late 19th Century is hat once we have these positions in a judgement, an enlightening thing to do is to *quantify* into them.

To do this, we will add to our language *variables*. Not only do we have names and possibly other more complex singular terms. We will also include *variables* x, $y,\ldots$ in the same syntactic category. So, not only is Ft a formula — so is Fx. But we have another syntactic category: the predicate. So, we add variables in *this* category too. Not only is Ft a formula, so is Xt. And so is Xx. (Of course, there is little sense to the expression Xx until we know how to interpret the variables X and x, but we will come to that soon.)[3]

Variables make sense only if we can *substitute* for them. The point of the expression Fx is to display and exploit what is common between Ft_1 and Ft_2. The point, of course, is that Ft_1 and Ft_2 are both *instances* of Fx. In general, given a formula A, we may define $A[x := t]$, the formula got by substituting the term t for the variable x. So, Ft_1 is $Fx[x := t_1]$ and Ft_2 is $Fx[x := t_2]$.[4] Instead of labouring over he official definition of substitution (which is covered in good textbooks) we will continue.

We can not only substitute for first order *term* variables such as x, we can also substitute for second order *predicate* variables like X. If A is a formula, then $A[X :=$

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[2]So, we could have complex predicates such as conjunctive or negative predicates, predicate modifiers such as 'very' or 'necessarily,' complex terms involving functions, or descriptions, etc.

[3]In fact, since predicates can have arity 2 and higher, we have classes of predicate-variables of each type. In this paper I will concentrate only on monadic predicate variables, though everything I say applies to dyadic and higher arities too.

[4]The definition of $A[x := t]$ is more than a simple "search and replace" for every x in A, since it is officially the replacement of every *free* instance of x in A by the term t, and furthermore, we do not allow the substitution of a term t for x in A if a variable in the term t would be *bound* in the process. So, $(\exists y)(y \neq y)$ is *not* an instance of $(\exists y)(x \neq y)$.

F] is the formula found by replacing (unbound) instances of the variable X by the one-place predicate F. So, Xt[X := F] is the formula Ft, and (¬Xt)[X := F] is ¬Ft. This is fair enough, but in the case of predicates, we might want to say more. What is common between Ft and ¬Gt? Do they *both* predicate something of the object we named with the term 't'? If we think so, they should both be instances of Xt. Unless we have some substantial account of properties and the confidence that, in our language, atomic predicates pick out these properties, it seems that this is the sensible decision. There is nothing that ¬Gt does that couldn't be done by an atomic predicate too. Similarly, we could take Ft ∨ Gt as an instance of Xt too. How can we give an account of this? A simple notation for this extended notion of substitution uses the notation of λ abstraction. If A and B are formulas, then A[X := λy.B] is the formula found by replacing subformulas of the form Xt by B[y := t].[5] So Xt[X := λy.(Fy ∨ Gy)] is Ft ∨ Gt.

Given these notions of substitution, we may, at last, consider the notion of quantification. If A is a formula, x is a first order variable and X is a second order variable, then (∀x)A and (∀X)A are also formulas, quantifying into the term and predicate position respectively.[6] Now we may proceed to the rules for quantification. Thankfully, they are straightforward. In both cases, the rules for quantification on the *left* of a sequent are what you would expect.

$$\frac{\Sigma, A[x := t] \vdash \Delta}{\Sigma, (\forall x)A \vdash \Delta} \, [\forall^1 L] \qquad \frac{\Sigma, A[X := \lambda y.B] \vdash \Delta}{\Sigma, (\forall X)A \vdash \Delta} \, [\forall^2 L]$$

If we may use an *instance* (either A[x := t] or A[X := λy.B]) as a premise in some derivation, we may use the stronger *generalisation* ((∀x)A or (∀X)A) as a premise in its stead, whatever the term substituted (t or λy.B) might be. In both cases, the universally quantified statement entails each of its instances. More interesting are rules for he *right* of a sequent. Again, the rules are parallel:

$$\frac{\Sigma \vdash A[x := c], \Delta}{\Sigma \vdash (\forall x)A, \Delta} \, [\forall R] \qquad \frac{\Sigma \vdash A[X := F], \Delta}{\Sigma \vdash (\forall X)A, \Delta} \, [\forall R] \quad \text{(c or F fresh)}$$

The side condition of 'freshness' is that the atomic term c is absent from Σ ⊢ (∀x)A, Δ, and that the atomic predicate F is absent from Σ ⊢ (∀X)A, Δ. If we can derive A[x := c] in a context in which we have made no other claims on c, then that derivation could equally well apply to anything, so we can derive (∀x)A in that same context. Similarly, if we can derive A[x := F] in a context in which we have made no other claims on F, then that derivation could equally well apply to anything, so we can derive (∀X)A from the same raw materials.

How do we fare with regard to Belnap's criteria? For uniqueness, we want a conservative extension result: this could be proved proof theoretically (by way of a cut-elimination argument), or model theoretically. Once we have models for the quantifier-free fragment of the logic, we can show that each model may be extended with quantifiers satisfying our rules. This suffices to show conservativity, since any argument invalid in the quantifier-free logic will be invalid in the full logic too. Since the standard model theory for second order logic (discussed in the next section)

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[5] Again, we take this to be defined only in the cases where no new variables are captured: so no variable in B[y := t] is bound in the formula A[X := λy.B].

[6] And here, a thorough presentation would explain the notion of free and bound variables in formulas, but as I am giving a sketch, I take these notions as read.

suffices for this, I declare *existence* proved. For uniqueness, we may point to a pair of straightforward proofs. Suppose we have *two* quantifiers of he same order. Without loss of generality, suppose they are second order quantifiers. So, $(\forall X)$ and (X) are both universal quantifiers, using rules of the same form. We can reason as follows:

$$
\dfrac{\dfrac{A[X := F] \vdash A[X := F]}{(X)A \vdash A[X := F]}\ [(\ )^2 L]}{(X)A \vdash (\forall X)A}\ [\forall^2 R]
\qquad
\dfrac{\dfrac{A[X := F] \vdash A[X := F]}{(\forall X)A \vdash A[X := F]}\ [\forall^2 L]}{(\forall X)A \vdash (X)A}\ [(\ )^2 R]
$$

This suffices to show that the two are interderivable. A language cannot have *two* differing second order quantifiers using these rules. They collapse into the one notion. Uniqueness is proved, and the quantifiers — both first and second order — count as Belnap-logical.

So, why is second order quantification taken to be problematic in a way that first order quantification is not? For this, we need to look at models for second order logic.

4   MODELS FOR SECOND ORDER LOGIC

Due to constraints of space, I must skim over the technicalities, since these are covered well elsewhere [9]. Take the fragment of the language with merely the predicates and terms, without the variables. To interpret the language, it suffices to take a non-empty domain $\mathfrak{D}$ of objects, and to interpret the predicates and terms appropriately in terms of $\mathfrak{D}$. We take an interpretation function I to select an element $I(t)$ of $\mathfrak{D}$ for each term t. For each n-place predicate F, we need to determine a truth value for each choice of n objects from $\mathfrak{D}$. A simple way to do this is to interpret F by $I(F)$, a subset of $\mathfrak{D}^n$, the n-fold Cartesian product of $\mathfrak{D}$. Then, the *model* $\mathfrak{M} = \langle \mathfrak{D}, I \rangle$ satisfies $Ft_1 \cdots t_n$ (we write "$\mathfrak{M} \models Ft_1 \cdots t_n$") iff $\langle I(t_1), \ldots, I(t_n) \rangle \in I(F)$.

So much is straightforward: the subtlety comes in with the interpretation of *variables*. We follow Tarski to define satisfaction relative to an assignment α of *values* to the variables. For each first order variable x, α will assign a value to x from the domain $\mathfrak{D}$. So, for any term t (involving variables or not), we have an assignment $I_\alpha(t)$ from $\mathfrak{D}$, either using I alone, if t doesn't feature any variables, or also consulting α, if t does. A natural choice for an n-ary second order variable X is the same: an assignment α gives to X a value as a subset of $\mathfrak{D}^n$. So, for a predicate P (involving variables or not), we have an interpretation $I_\alpha(P)$, as before. So the satisfaction clause for atomic sentences is now as follows:

$$
\mathfrak{M}, \alpha \models Pt_1 \cdots t_n \text{ iff } \langle I_\alpha(t_1), \ldots, I_\alpha(t_n) \rangle \in I_\alpha(P)
$$

For the quantifiers, we allow the values of variables to — well — *vary*. The first order case is well understood.

$$
\mathfrak{M}, \alpha \models (\forall x)A \text{ iff } \mathfrak{M}, \alpha[x := d] \models A \text{ for each } d \in \mathfrak{D}
$$

What about the second order case?

$$
\mathfrak{M}, \alpha \models (\forall X)A \text{ iff } \mathfrak{M}, \alpha[X := D] \models A \text{ for each } D \in \ ??
$$

I have left unsettled the appropriate range of variation for the second order variable X. What goes in the place marked "??"? All we have said so far is that the variable X

ranges over subsets of $\mathfrak{D}^n$. Just *which* subsets of $\mathfrak{D}^n$ is the crux of the issue before us.

One obvious choice is to let the variables range over the set of *all* subsets of $\mathfrak{D}^n$. This gives us *standard* models of second order logic. Standard models have the advantage of simplicity. They have the *disadvantage* of being stronger than the rules for second order logic discussed in the last section. For example, if — as many think — the axiom of choice is true, then there is a sentence in the language of second-order logic true in *every* model.[7] Further, if this is valid, then the axiom of choice is true. Similarly, there is a sentence valid iff the continuum hypothesis is true, and a different sentence true in every model iff the continuum hypothesis is false.[8] None of these sentences are provable in our deductive system.[9] So, these models — whatever the 'real' universe of sets is like — overgenerate, relative to the proof system.

Other models do not. Henkin models, due to Leon Henkin [3], restrict the domain of second order quantification to *some* subsets of $\mathfrak{D}^n$. In our case we need the subsets of $\mathfrak{D}^n$ to be closed under the operations definable in the language of second order logic. Stating exactly the closure conditions required is a little subtle, and so, I leave it to other discussions to explain the properties of Henkin models of second order logic. Suffice it to say that these models are ones in which the second order variables range over a possibly restricted collection of subsets of the domain $\mathfrak{D}$, and there is much freedom in choosing which collection to use. An example which will sustain us into the next section is provided by models for second order Peano arithmetic: PA2. In this theory, the induction axiom takes the form

$$(\forall X)((\forall x)(Xx \supset Xs(x)) \supset (X0 \supset (\forall x)(Nx \supset Xx)))$$

For any X at all, if it is closed under successor, then if 0 is X, *every* number is X. It is well known that this theory has models in which there are *more* numbers than just the standard numbers $0, 1, 2, \ldots$ There are models with *non*-standard numbers. They are not *standard* models of second order logic, but they are Henkin models, in which the second order quantifier ranges over a reduced collection of subsets of the domain. In particular, it does not take in the set of all and only the *standard* numbers. Given the induction scheme above, if the collection of standard numbers was an instance of X, then we could show that all numbers are standard. So, the only models of PA2 in which there are non-standard numbers are non-standard models.

5   THE ARGUMENT

Now we can state our problem. We have two choices for understanding models of second order logic.

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[7]What sentence? $(\forall X)((\forall x)(\exists y)Xxy \supset (\exists f)Xxf(x))$ will do. It uses quantification over functions. It is a nice exercise to reduce quantification over functions to quantification over binary relations. A function is a binary relation such that for every source there is one and only one target. We can say this in the language of second order logic by saying . . .

[8]Which sentences? I won't tell you here as they are more complicated.

[9]Why not? One easy way to show it is to use standard models (for which the proof system is sound), *inside* models of a set theory like ZF. Take one ZF model in which the continuum hypothesis is true, and another in in which it is false (thankyou, Gödel and Cohen!). Inside these models, construct counterexamples to our chosen sentences. The model in which CH is true gives us a counterexample to the sentence ¬CH, and he model in which CH fails gives us a counterexample to the sentence CH. Both models show that *neither* sentence is deducible, as the soundness proof can be carried out *inside* ZF.

Option 1 Standard models are not so special. Bite the bullet and agree that *Henkin* models characterise validity. To choose standard models as the only models of second order logic is to privilege set theory over logic. Standard models build in too much set theory. Logic is the logic of Henkin models.

Option 2 Standard models characterise second order consequence. The proof theory is incomplete.

Both positions are unwelcome for the friend of proof theory for second order logic. To take *Option* 2 is to reject the proof theoretical analysis of validity. It is out for an inferentialist who takes anything like the sequent calculus (or any deductive system) to characterise validity, as there is *no* deductive system strong enough to characterise the logic of standard models. To take *Option* 1, however, is also inadequate, and to conclude this paper I wish to explain why and to point the way forward.

Consider the case of the non-standard Henkin model of PA2. We can use this as a *device* to tell us something of what can and cannot be proved in the deductive system for PA2. If we have a counterexample to something in this model, it will not be derivable in our system. However, even for the friend of a proof theory, there is something *defective* in non-standard models, even if they help characterise consequence in the logic of our deductive system. The defect is straightforward: once we *describe* the non-standard model, we have the resources to show how it is not a *stable* model of PA2. The notion of stability at play here is simple: take the non-standard model of PA2 with non-standard numbers. Now *extend* the language of our formal logic with a new predicate, and and make this new predicate true of all standard numbers and false of all other objects in the domain. (This model will have to have an expanded domain of second order quantification.) In this model, the theory PA2 no longer holds, since the second order induction axiom is now false. The extension of our *new* predicate provides a counterexample.

Here is a slogan: non-standard Henkin models are good as models of a second order deductive theory. However, they do not necessarily *stay* as models as we extend the language. Standard models don't have this problem. In a standard model, adding a new predicate (or term) to the language does not mean that we have to adjust the domain of quantification of second order variables, as it is as big as can be already. This opens up space for an option intermediate between 1 and 2.

Option $1\frac{1}{2}$ Henkin models characterise validity in a given language. However, non-standard Henkin models need not be stable under extensions of that language. A standard model for a theory remains a model for that theory even after new vocabulary is added, but this need not hold for Henkin models.

The non-standard model of PA2 is a case in point. It is a model for a theory in which the induction axiom holds for every property *expressible* in that theory. But the second order quantifier is not a quantifier ranging merely over arithmetically definable properties. If we extend the language to include a *new* property, then induction holds for that property too. (If mathematicians consider a new property— F-ness—then they do not ask if it is still true that if whenever n has F-ness, so does its successor, and if 0 has F-ness, then every number is F. That is a part of the concept of being a natural number. This holds even if F-ness is not definable in PA2. It follows that the non-standard model for PA2 need not be a model for PA2 together with a theory of F-ness.)

There is much more to say. One important issue is the converse of the claim made in *Option* $1\frac{1}{2}$. Is it true that *only* standard models are stable under extensions

of the language? I conjecture that this is the case, but I do not have time or space to consider this conjecture any further.

What I hope to have shown is that an inferentialist can rightly claim that first and second order quantification both bear important marks of logicality (relative to this context of deducibility at least, they are Belnap-logical), and to sketch how the inferentialist, who takes deduction to be the starting point for the characterisation of second order logic can find something useful to say about the gap between Henkin models and standard models.

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