Critical Studies / Book Reviews

JAAKKO HINTIKKA. ***The Principles of Mathematics Revisited***. Cambridge: Cambridge University Press, 1996. Pp. xii + 288. ISBN 0-521-49692-6.

Games Some People Would Have All of Us Play

Review by NEIL TENNANT*

Introduction

In this ambitious work, an 'application and further development of the ideas of game-theoretical semantics' (p. 27), the author is 'hoping... to prepare the ground for the next revolution (in Jefferson's sense rather than Lenin's) in the foundations of mathematics' (p. vii). He is 'trying to wake [his] fellow philosophers of mathematics from their skeptical slumbers' (p. ix), and to 'disprove, once and for all, the myth that the notion of truth in a sufficiently strong language is inexpressible in that language itself' (pp. 126–7). He is candid about the work's comparative stature: 'I only regret that I have developed my own ideas far too late to discuss them with the likes of Tarski, Carnap or Gödel' (p. xi). He believes that his results 'have realized the constructivist's dream' (p. 32), and will be an 'essential part of any half-way satisfactory theory of negation when it comes to natural languages' (p. 155). The author declares that 'a serious crisis is about to break out' (p. vii). He regards all of the following dogmas as 'ripe for rejection' (I quote or paraphrase freely): (1) the true elementary logic is ordinary first-order logic; (2) a truth definition for a given language can be formulated only in a stronger metalanguage; (3) non-trivial first-order mathematical theories are inevitably (descriptively) incomplete; (4) first-order logic is incapable of dealing with the most characteristic concepts and modes of inference in mathematics—mathematical thinking involves essentially higher-order entities; (5) axiomatic set theory is a natural framework for mathematical theorizing; (6) negation is a simple concept involving merely reversal of truth values; and (7) the principle of compositionality: the semantical value of a complex expression is always a function of the semantical values of its component expressions.

The author promises at the outset (p. x) a 'philosophical essay, not a research paper or treatise in logic or mathematics'. But this does not make amends for what appear to be logical errors, oversights, misjudgments, and/or failures to anticipate objections. The book fails to provide a clear understanding of the expressive innovation on which all of Hintikka's claims

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in the philosophy of mathematics are based; and it turns a blind eye to the twin defects of expressive incompleteness and deductive incapacitation even while claiming definitive advantages on other foundational issues. Hintikka would have mathematicians unable to deny claims by refuting them, and unable to pursue truth by means of proof.

Chapter summaries

In this section I give as thorough and accurate an account as I can, in the space available, of what the author tries to accomplish in each chapter.

1. The functions of logic and the problem of truth definition

What is the role of logic in mathematics? The author discusses Hilbert's axiomatization of Euclidean geometry. '[A]ll substantive assumptions are codified in the axioms, whereas all the theorems are derived from the axioms by purely logical means' (p. 2). The 'task of intellectual mastery is a much more important motivation of the axiomatic method than a quest for certainty' (p. 2). The author distinguishes the *deductive* function of logic from its *descriptive* [or expressive] function (p. 9). His emphasis is on the latter function, and on model theory, hence truth definitions, as philosophically important (p. 12–13). He identifies what he calls 'Tarski's curse', that is, belief in the ineffability of semantics. There are problems, he alleges, in taking set theory as the universal framework of mathematics.

2. The game of logic

The author sets out game-theoretical semantics (GTS) for ordinary first-order logic, and makes a case for its naturalness, and for the prospect of extending it to various constructions in natural language. An interesting line of generalization of GTS is to 'infinitely deep' sentences, for which normal recursive definitions of satisfaction and truth cannot be applied. The 'language games' of seeking and finding exemplars for quantifications are nicely formalized by GTS. The locutions 'whatever you choose for x, I can find some y such that ...', which are familiar from informal renderings of the Cauchy-Weierstrass definitions of continuity, *etc.*, do indeed seem custom-made for GTS treatment. Truth is explained as the existence of a winning strategy for the player starting out as the 'verifier'; and falsity as the existence of a winning strategy for the player starting out as the 'falsifier'. Negation signs are dealt with by having the two players switch their roles as verifier or falsifier before resuming play on the unnegated formula.

3. Frege's fallacy foiled: Independence-friendly logic

Anachronistically put, Frege thought logic was a game with perfect information (p. 50). This is the fallacy of the chapter title. The author stresses the notion of a dependent quantifier, and the associated game-theoretical notion of informational dependence. Independence-friendly (IF) first-order logic allows for 'branching' or partially ordered quantifier prefixes, in which choices for existentials are made only with 'knowledge of' what choices have been made for earlier quantifiers *on the same branch.* These choices would therefore be *independent* of choices made for quantifiers on *different* branches. The author introduces 'slash notations' which allow him to make explicit the particular dependencies and independencies involved, while yet linearizing the notation (so that one supposedly does not have to use literally branching, two-dimensional formulae). The extension of ordinary first-order language being proposed is *permissive.* All ordinary first-order sentences are still in the language. An IF first-order language simply allows more sorts of sentences to be formed. IF first-order logic will also be a conservative extension of ordinary first-order logic.

The notion of *negation normal form* is introduced. In such a form, a formula has negations occurring only immediately before atomic subformulae. The author distinguishes what he calls *priority scope* from *binding scope.* In ordinary first-order logic these two notions of scope are conflated.

He then reviews metalogical results for IF logic. It is compact; it has a (restricted form of) the downwards Löwenheim-Skolem property; the separation (*i.e.*, interpolation) theorem holds; Beth's theorem on definability holds. Every IF first-order sentence has a Σ^1_1 second-order translation, and every Σ^1_1 sentence has a logical equivalent in IF first-order language. The unfamiliar aspects of logical behavior of IF languages are that IF first-order logical truth does not admit a complete axiomatization; and that IF first-order languages do not admit a *Tarski-type* truth definition. IF languages, however, express their own truth definitions explicitly, without any need for recursive definition. The law of excluded middle fails. Although IF logical truths are not axiomatizable, IF logical falsehoods *are*, which means that negation behaves in an unusual way. 'In general, alas, there is no contradictory negation of C in the relevant IF first-order language (*i.e.*, no formula which would be true just in case C is not)' (p. 68).

4. The joys of independence: Some uses of IF logic

This chapter is about what IF logic can do, rather than what it is like. Hintikka stresses the ubiquity of the phenomenon of informational independence in natural languages. He makes a case [of dubious force—see below] for our *need* for IF first-order locutions in order to express exactly the notion of a function's uniform differentiability in an interval, and to capture the logical form of certain combinatorial theorems in Ramsey Theory. Two provocative suggestions are that (i) IF logic can help 'to demystify the indeterminacy phenomena of quantum theory' (p. 77), and (ii) IF first-order logic is the logic of parallel processing. Hintikka urges that, in the light of IF first-order logic, the axiom of choice is an acceptable and purely logical principle. He also examines briefly the application of IF logic to the analysis of wh-constructions in epistemic logic.

5. The complexities of completeness

'IF first-order logic is much stronger than its more restricted traditional version.' (p. 88) The author addresses what he regards as a misconceived modern delineation between logic (complete) and mathematics (incomplete). He then distinguishes the following senses of completeness: descriptive, semantical, deductive, and Hilbertian. Descriptive completeness is also known as categoricity. (A theory T is descriptively complete if and only if there is only one intended model, up to isomorphism, of T.) Semantical completeness is a matter of the logical truths being recursively enumerable. Deductive completeness (also known as theory completeness) of a theory T is a matter of T's containing, for every sentence C, either C or $\sim C$. Hilbertian completeness (not much further discussed) is a matter of certain models being 'maximal' for the axiom systems concerned. Some arguments are advanced to downplay the importance of deductive completeness as a regulative ideal in mathematics.

6. Who's afraid of Alfred Tarski? Truth definitions for IF first-order languages

The author sets out to establish the claim that IF first-order languages 'open up a way of actually freeing the expressibility of the concept of truth from the serious problems that have been seen to beset it' (p. 105). He summarizes important aspects of Tarski's work: explicit definitions of truth for formal languages; the appeal to the notion of satisfaction; the need for a stronger metalanguage in order to formulate the definition; the impossibility of thus defining truth for colloquial language; and the allegedly 'less deep but eminently popular suggestion' that the theory of truth be required to meet Convention T (*i.e.*, that the theory should contain all instances of the well known T-schema

$$\delta \text{ is true} \leftrightarrow S$$

'where δ is a quote or a description of S'). The author believes that we ought to abandon *all* of these features.

The author goes on to discuss the role that the principle of compositionality plays in Tarski's account. IF first-order logic, he says (p. 109), shows the 'utter futility of trying to abide by' this principle. IF first-order languages are not compositional. Therefore Tarski-type truth definitions do not apply to them. He proceeds to give a clause-by-clause (explicit, not recursive) definition of a predicate $TR[X]$ of Gödel numbers of sentences, and claims that the truth predicate has the form $(\exists X)(TR[X] \,\&\, X(y))$. That is, the latter expresses 'y is true in the standard model of arithmetic', or, more accurately, 'y is [the Gödel number of a sentence of arithmetic that is] true in the standard model of arithmetic'. The latter predicate (if its definition is well framed), being Σ^1_1, has an equivalent in the IF first-order language of arithmetic itself. The rest of the chapter is devoted to arguing the philosophical merits of the new truth definition, chief among which is claimed to be that model theory is now 'conceptually independent' of set theory.

7. The liar belied: Negation in IF logic

The author argues that the law of excluded middle fails for IF logic; that, despite containing its own truth definition, an IF language will not fall prey to the Liar paradox. He investigates the notion of contradictory negation by means of which an IF language might be extended. He argues, by means of examples from natural language, that 'in any sufficiently rich language, there will be two different notions of negation present': the weaker, 'contradictory' negation, and the stronger, 'dual' negation characterized by his semantical games.

8. Axiomatic set theory: Fraenkelstein's monster?

The iterative conception does not do justice to Cantor's intended universe of sets (p. 169). For set-theoretical Liar sentences (using the author's truth definition) the inference "ϕ, therefore $T(`\phi')$" fails. The set-theoretical claim corresponding to $T(`\phi')$ is false, but 'ought' to be true. The sets that 'should be' the Skolem functions whose existence is asserted in the statement $T(`\phi')$ of truth conditions fail to exist in the model M in question, even though ϕ is true in M (p. 174). Difficulties face any 'merger' of IF logic with set theory. '... [T]he entire strategy of trying to capture sets as extensions of formulas fails to do its job when the logic it is based on includes independent quantifiers' (p. 178). We cannot use IF formulae in the axiom scheme of comprehension (p. 174). No principle remotely like reducibility holds (p. 180). Explicit definitions cannot be used outside ordinary first-order logic without strict precautions (pp. 180–81).

9. IF logic as a framework for mathematical theorizing

Various mathematical notions such as mathematical induction, finiteness, infinity, uncountable infinity, well-ordering, cardinality and power set cannot be captured in ordinary first-order logic. But they can be captured in IF first-order logic (provided only that, in some cases, we 'extend' the logic by adding a sign for the otherwise inexpressible contradictory negation). IF logic yields a 'tremendous increase in conceptualizing power' and we are forced to 'radically revise our ideas of the borderline between logic and mathematics' (p. 190). One ought to have qualms about the higher-order entities involved in capturing these mathematical notions in second-order logic. One does not need to ascend above first-order, since '... virtually all of classical mathematics can in principle be done in extended IF first-order logic' (p. 196). For 'categorical theories, mathematical truth can in a sense be equated with logical truth in IF first-order logic... The truth of Goldbach's conjecture is... equivalent to the validity of an IF first-order formula' (p. 197). '... [M]athematics is at bottom combinatorial rather than set-theoretical in character' (p. 199).

10. Constructivism reconstructed
Restriction to recursive functions as strategies in games yields constructivistic GTS. This is argued to throw more light on the true nature of constructive mathematics. Against the Dummettian tradition, the author urges that 'the role of constructivistic notions has nothing to do with the meaning of mathematical statements. ... It is a matter of sentence-model relationships, ultimately a matter of truth definitions. It is not a matter of deductive relationships between propositions' (p. 232).

11. The epistemology of mathematical objects
The author discusses epistemic logic and what it is to know a function as opposed to knowing that a mathematical proposition is true. Various 'perspectives' and 'vantage points' are revealed from which aspersions on extant forms of contructivism and intuitionism would appear to be in order.

Appendix (by Gabriel Sandu)
The appendix seeks to provide the technical details needed both to clarify the new concepts (such as well-formedness of formulae in IF logic) and to back up the interpretive claims in the body of the text. Most important is the formal definition of truth and the proposed proof of its adequacy (Proposition 1, p. 261).

Criticisms

I turn now to my criticisms, organized by topic.

• *On well-formedness and the Henkin quantifier.* The author gives a formal definition of well-formed formulae of IF ('independence friendly') first-order languages (p. 52), but it generates only linear formulae with 'internal independencies' registered by means of a slash notation. He starts with an ordinary formula in negation normal form, and then tampers with quantifier prefixes by introducing slashes. For example, one could start with the formula $\forall x \exists y Fxy$ and change it to $\forall x(\exists y/\forall x)Fxy$. The latter would mean that the choice of a value for y would be independent of whatever had been chosen earlier for x. This particular example would be equivalent, then, to the ordinary formula $\exists y \forall x Fxy$. But in general the IF formula created by introducing slashes into an ordinary formula will not be re-translatable into an ordinary formula (which is why IF first-order logic properly extends ordinary first-order logic in expressive power). A case in point would be the IF formula $\forall x \forall z(\exists y/\forall z)(\exists w/\forall x)Fxyzw$. This is equivalent to the 'branching' structure

$$\left.\begin{matrix}\forall x \exists y \\ \forall z \exists w\end{matrix}\right\} F(x, y, z, w) \tag{1}$$

whose whole prefix is known as the Henkin quantifier.

No attempt is made to convert the definition of well-formedness of 'slash formulae' into a definition of well-formedness of actually branching sentential structures. It would nevertheless appear to be the case, in Hintikka's earlier presentation, that the result of prefixing the negation operator to any formula would not always be a formula. One has to ponder the definition on p. 52, which begins with an innocent-looking appeal to a formula of ordinary first-order logic *in negation normal form*, in order to realize that negation signs are permissible in IF-formulae only when prefixed to atomic formulae. In Gabriel Sandu's technical Appendix, however, IF formulae are suddenly *permitted* to have negations in front of any subformula. Nevertheless, if we were to take Hintikka's first definition of well-formed formulae of IF logic on p. 52 strictly, there would be no such thing as an IF-formula of the form $\sim P$ when P is complex.

At best, the two accounts of well-formedness provided by Hintikka and Sandu are in tension on the question of where negations are permitted to appear. In the Appendix, it is also confusing to encounter Sandu's use, as the negation sign for non-contradictory negation, the very sign $\neg$ that Hintikka had used, throughout the other chapters, as the sign for contradictory negation—which is inexpressible in IF first-order languages. They should both have used the tilde when meaning the kind of negation that is interpretable by the game rules, that is involved in negation normal forms, and that does not extend IF first-order logic.

One might have hoped that Sandu's Appendix would have yielded a precise understanding of well-formed formulae. But it only raises new difficulties. Sandu's definition (pp. 254–5) has the untoward consequence that no provision is made for

$$\left.\begin{matrix}\exists x \forall y \\ \exists z \forall w\end{matrix}\right\} F(x,y,z,w) \tag{2}$$

This is serious, since we need to understand the latter kind of existential-universal branching quantifier prefixes in order to deal with negation normal forms of, for example, the negation of (1). Sandu's quantifier prefix G^*xyzw, it should be noted, is *not* equivalent to the existential-universal prefix in the formula just displayed. For the latter formula does *not* possess the truth conditions implicit in Sandu's clause (A.2.9) for formulae beginning with G^*xyzw.

• *On negation normal forms.* On p. 30 the author claims that it is not overly restrictive to consider only sentences in negation normal form, since every first-order formula can be brought into this form by an effective procedure. He fails to remark, however, that the underlying logical equivalences required for such transformations do not all hold intuitionistically. Thus classical logic is presupposed by such a treatment. This means that there is an internal tension later on, when he accepts the failure of excluded middle for his 'independence friendly' logic. As early as p. 33 he is claiming that 'the law of excluded middle cannot in general be expected to hold if truth is defined game-theoretically'. So: he appears to be willing to help himself to the law of excluded middle in order to massage sentences into the negation normal form in which they have to be in order for his treatment to apply; *but* to be willing also to say that excluded middle fails, given the resulting treatment! The reader is owed at least some discussion to resolve the apparent tension here.

• *On the semantical game rules.* The author provides a statement of the semantical game rules only for sentences of *ordinary* first-order logic. But such a statement of game rules is missing for the sentences of IF first-order logic that are introduced later. It should not be consigned, without reference, to the Appendix. Moreover, the rules of GTS in Sandu's technically more precise account fail to capture the meanings of the 'information independent' quantifiers provided for with the slash notation. Applying clause (vii) on p. 256 for Sandu's linearization of (1), for example, we are told that 'the falsifier chooses [an object] a, then the verifier chooses an object b, after which the verifier chooses [an object c], and then finally the verifier chooses [d]... The game goes on [with the sentence $F(a, b, c, d)$].' There is no condition stated here to ensure the requisite information-independence of the verifier's choices of c and d!

In summary: Sandu does not provide for the existential-universal Henkin sentence as well-formed; and he does not provide correct interpretations even for the formulae that he *does* count as well-formed. Thus even the most basic and crucial features of the new approach are stated unsatisfactorily in what is supposed to be a definitive, technically rigorous presentation.

• *On compositionality.* In the long discussion of compositionality, the author offers no precise and general characterization of what it would mean to say that a semantics was compositional. He does not anticipate the objection that the 'failure of compositionality' for IF first-order languages might be a bad mark only against particular choices of *notation* for the branching logical structures in question, rather than a bad mark against the principle of compositionality for those structures themselves. The author even concedes (p. 112) that he has 'not given a strict impossibility proof' to the effect that a compositional semantics for IF first-order languages would be impossible. Indeed, to do so would require a strict definition of compositionality in advance, and an argument to the effect that it covered all possible forms of compositionality.

• *On deductive axioms and rules.* On p. 4, when discussing axiomatization of logic, the author writes as though Hilbert-style axiomatizations were the only ones available. No mention is made of the much more natural and entirely rule-theoretic presentations given by Gentzen. This is a puzzling omission, since the author repeatedly stresses the 'naturalness' of game semantics as its chief theoretical virtue.

The author does not ever state a set of axioms and/or inferential rules by means of which the single turnstile of deducibility ($\vdash$) would be defined for IF first-order logic. Yet the single turnstile is used in the statement of important metatheorems for IF first-order languages, such as the separation theorem on p. 133, which, it must be noted, involves the single turnstile in contexts other than proofs of inconsistency. It is also involved in the alleged 'axiomatizability of the inconsistent (unsatisfiable) formulas of IF first-order logic' (p. 68). Hintikka, to be sure, gives some indication (p. 68) of how such inconsistency proofs would be thwarted attempted model-set constructions by means of certain (unspecified) expansion rules. But when so many unusual things are happening in this new logic, such matters should not be left to the reader's unschooled intuitions.

• *On logical consequence.* No attempt is made to connect the game-theoretical account of truth (and of falsity) with the usual requirement, concerning patterns of correct inference, that they should always transmit truth from the premisses to the conclusion of any inference, or re-transmit falsity from its conclusion to at least one premiss. This makes matters particularly difficult when one deals with negation. It is not clear (despite certain remarks on p. 67) whether, for example, the 'logical truths' of an IF language are those sentences on which player F never has a winning strategy in any model, or those sentences on which player T always has a winning strategy (and dually for logical falsehoods). Thus when the author asserts the 'axiomatizability of the inconsistent (unsatisfiable) formulas of IF first-order logic' (p. 68) the reader has no precise idea of how, semantically, these formulae are characterized. The word 'unsatisfiable' is ambiguous here between 'always allowing player F to win' and 'never allowing player T to win'. (The word 'inconsistent', of course, is no help, since that term usually means 'provably implies a contradiction'; and by that usual meaning it would be obvious and analytic that the inconsistent formulae would be recursively enumerable.)

• *On the Löwenheim-Skolem Theorem.* On p. 6, the 'upwards Löwenheim-Skolem theorem' is stated as the claim that 'a consistent finite set of sentences has a countable model'. This of course is the *downwards* theorem, and in it 'finite' can be strengthened to 'countable'. The proof, on p. 59, that IF first-order logic has the downwards Löwenheim-Skolem property leaves something to be desired, since it goes through only for (finite sets of) sentences, not arbitrary theories.

• *On the conditional.* On p. 25, in the statement of the rules of GTS for ordinary first-order logic, the connective for implication is conspicuously absent. This does not matter in the classical case, where $A \rightarrow B$ can be defined as $(\sim A) \vee B$. But it is an important lacuna for the intuitionist, who would be interested in what light *effective* strategies in these games might shed on the notion of intuitionistic truth. Despite the official absence of the conditional from formulae of IF logic, the author makes free with conditionals in the definition of truth that is supposedly given within the IF language itself.

• *On Hilbert's program.* The author claims that

> Hilbert could not have conceived of his famous project of proving semantical (model-theoretical) consistency of mathematical theories by establishing their proof-theoretical consistency, if he had realized that the true basic logic he needed in those theories would not have a complete proof-theoretical axiomatization.

Ironically, however, since IF first-order logic is *inconsistency complete*, Hilbert could still have conceived of his project in exactly this way! For the proven consistency of a theory would guarantee the existence of a model for it.

• *Varieties of completeness.* On pp. 91–2, where the author canvasses four kinds of completeness (and incompleteness)—descriptive, semantical, deductive, Hilbertian—he overlooks one very important kind, namely *expressive* completeness (and incompleteness). We have a pre-theoretical intuition that there is a range of *things we want to be able to say*, given what else we can already say; and a good language should be closed under arbitrary (finite) iterations of this requirement. And among the most important things we want to be able to say, given that fellow speakers can already say that P, is 'No, it's *not* the case that P'. I shall return to this point below when I discuss Hintikka's treatment of negation.

• *On descriptive completeness and second-order logic.* On p. 94, the author suggests we should take seriously 'the possibility of formulating descriptively complete nonlogical theories based on a semantically [in]complete logic'. The paradigm of such a nonlogical theory, which he does not note, would be the second-order theory of arithmetic. Like IF logic, second-order logic has no complete proof procedure. Yet, unlike IF logic, second-order logic affords a descriptively complete (*i.e.*, categorical) account of the natural numbers. Thus it is not really to the credit of IF logic, and IF logic alone, that the possibility in question is indeed a serious one. The author asks

> . . . must there for some reason be just one indivisible logic serving both the descriptive and the deductive purpose?. . . [This] fundamental [question has]. . . been scarcely raised in the literature. (p. 10)

Only much later in the text (pp. 192–3) does the author concede that in fact Stewart Shapiro[1] has investigated extensively the deductive and expressive contrasts between first-order and second-order logic, with precisely

[1] *Foundations without Foundationalism.* Oxford: Clarendon Press, 1991.

the same concerns about capturing important mathematical concepts and characterizing important mathematical structures up to isomorphism.

Hintikka's overriding concern with descriptive completeness has to be weighed against the sacrifice that he appears willing to make over and above the sacrifice of deductive completeness. The extra sacrifice, which is not candidly enough admitted, and not incurred by the second-order logician, is a terrible lapse into expressive incompleteness with regard to negation, or at least a peculiar interrelationship between truth and falsity (as Hintikka defines them).

• *On the treatment of falsity and negation.* In Hintikka's logic one cannot even show that P is false by establishing that P's truth would entail P's falsity. If one could reason thus about truth and falsity:

(1) if "P" is true then "P" is false; so, "P" is false;
(2) P; therefore "P" is true;
(3) "P" is true; therefore P,

then we could re-instate the Liar reasoning (involving the truth predicate defined in IF first-order language) that Hintikka is at such pains to avoid. By infirming *any one* of the three inferences above, however, the IF advocate does such violence to the semantic notions of truth and falsity that one can legitimately refuse to be impressed by the claimed emancipation of 'IF truth' from the Tarski-Gödel phenomena.

Even the deflationist would subscribe to the three rules just stated! And so does the intuitionist. That is because both of them define ' "P" is false' as 'It is not the case that "P" is true', and use negation in the standard way—according to which one may infer the negation of any proposition Q by simply refuting Q. The problem for Hintikka is that his strong 'negation' operator maps any sentence Q to the negation normal form of what we would usually regard as Q's negation; and one can mathematically refute Q without being able to infer the Hintikka 'negation' of Q. Freedom from the Tarski-Gödel phenomena looks to be terribly expensive at this price.

• *On the importance of Gödel's and Tarski's results.* The author says (p. 94) that

> Philosophers have been impressed by Gödel's result because they have overestimated the importance of deductive and computational techniques in mathematics. They have been seduced by the oversimplified picture of mathematical activity as mere theorem-proving. In reality, it is clear that deductive incompleteness is not the most important kind of incompleteness.

Now the picture would be oversimplified only if it were one of mathematical activity as mere theorem-proving *within some single fixed system.* But with this qualification the overestimation claim is most implausible.

On p. 98, the author writes

> the descriptive incompleteness of Peano arithmetic is an almost direct consequence of the compactness of first-order logic. It does not need Gödel's

> elaborate argument for its proof. All this shows how many important issues are left untouched by Gödel's results.

This appears a little harsh, given that it was Gödel himself who proved the compactness theorem for first-order logic, in his doctoral dissertation of 1929.

The author also appears to be of two minds about his chosen peers. On the one hand,

> I do not think it is any exaggeration to say that the community of philosophers and mathematicians has still not managed to cope with the true import of Gödel's discoveries and to draw the right consequences from them for the future development of the foundations of mathematics. (p. 90)

Yet on the other hand:

> In my considered judgment, [the] importance [of the negative results of Tarski, Gödel and Lindström] has been vastly exaggerated. (p. 18)

> ...From [the] perspective [of IF first-order logic], it must be said that the philosophical community has not acknowledged how limited the significance of Gödel's incompleteness result really is. (p. 94)

• *On proof theory versus model theory. Pace* Hintikka, mathematics is *all about proofs*, albeit proofs within ever-expanding systems of axioms and rules. The incompletability of mathematics does not entail the existence of absolutely undecidable mathematical conjectures. For any incomplete system can be properly extended (even if, perforce, only to another incomplete system). The so-called 'semantic argument' for the truth of the independent Gödel-sentence for Peano arithmetic, for example, is still a *proof*—but in a (proof) system for arithmetic augmented with new instances of the induction axiom scheme, instances which are made available by the explicit adoption of a truth predicate for the earlier system.

Moreover, the representation theorems of group theorists (cited by Hintikka as a paradigm case of how mathematics is not a matter of deriving theorems from axioms) are still *proved* within a suitably extensive system of universal algebra or set theory. Just because the group theorist happens to have interests other than those of pursuing the strictly first-order consequences of the group axioms does not mean that he has ceased to construct *proofs* when arriving at the results that really do interest him. (Lagrange's theorem for finite groups is *proved*, is it not?)

Pace Hintikka, almost all the most important developments in foundations this past century have come from a scrutiny of systems of *proof* and systems of inductive definitions. Beginning with Frege, the question was raised as to what axioms, rules of inference and definitional methods one needed in order to carry out mathematical *proofs*, once the linguistic devices for the expression of mathematical claims had been identified and analyzed. Both the *Grundgesetze* and Russell and Whitehead's *Principia Mathemat-*

ica were devoted to *re-proving* whole edifices of mathematics within their new logical systems.

Formalism was a view about the nature of *proof*, as was intuitionism: the former concerned more with trying to understand what we were up to when discovering and communicating proofs; the latter, with what rules were really licit for their construction.

The earliest investigations in set theory were directed to the question of what was needed in order to *prove* certain desired results, once one had weakened methods of proof (or principles of set formation) enough to avoid the paradoxes. Likewise, the more recent foundational area of reverse mathematics is essentially concerned with calibrating the proof-theoretic strength of various comprehension axioms in higher-order arithmetic. Important collections of mathematical theorems (*i.e.*, *proven* results in mathematics) are *proved* equivalent modulo a given form of comprehension. The whole stress is on what *provably implies* what, not on what describes what.

The reason why logical languages lacking a complete proof procedure do not find favor is that we would not in general be able to come to know, by means of *proof*, what the logical consequences of axiom systems in such a language were. Hintikka seems to underestimate the pervasive importance of *finitary proof* as the vehicle of epistemic advance in mathematics. Who cares if one can, by means of a set of axioms in some language, exactly describe some mathematical structure M, if one cannot thereby work out, *by means of proof*, what *other* claims, expressible in the *same* language, are true of M? The characterization of mathematical activity (p. 96) as involving our ability to 'deal with' elementary arithmetic 'descriptively' is really empty, *unless* it can be shown how such 'dealings' give us a satisfactory alternative to the *epistemic access* that proof is designed to give.

- *On descriptive completeness (categoricity).* On page 98, the author says

> there are deductively complete and hence decidable theories of reals and of sufficiently elementary geometry... These theories are nevertheless—or, for that very reason—descriptively incomplete [*i.e.*, non-categorical]. They admit different nonisomorphic models, and hence will not satisfy a mathematician.

There are two things wrong with this. First, *any* theory that has a countable model will, by the upwards Löwenheim-Skolem Theorem, have models of every infinite cardinality, hence 'admit different nonisomorphic models'; so their deductive completeness and ensuing decidability would be beside the point. That is, deductive completeness will *not* be the 'very reason' why the theory in question is descriptively incomplete. So perhaps Hintikka meant only to be understood as talking about categoricity in some particular (infinite) power, such as $\aleph_1$ (since he is talking about the reals). Could this construal save his point? His claim would then have to be qualified so as to read:

> there are deductively complete and hence decidable theories of reals and of suf-

> ficiently elementary geometry... These theories are nevertheless—or, for that very reason—not $\aleph_1$-categorical. They admit different nonisomorphic models of cardinality $\aleph_1$, and hence will not satisfy a mathematician.

I fail to see how the alleged 'reason' operates. Surely the intended structure for the theory cannot be at all important. Hintikka should be able to make his point just as forcefully with $\aleph_0$ in place of $\aleph_1$. So consider:

> there are deductively complete and hence decidable theories of orderings... These theories are nevertheless—or, for that very reason—not $\aleph_0$-categorical. They admit different nonisomorphic models of cardinality $\aleph_0$, and hence will not satisfy a mathematician.

This is false, as the well known example of the theory of the ordering of the rationals makes clear. A famous result of Cantor's is that any two non-trivial, countable, dense, linear, unbounded orderings are isomorphic. Thus the (finitely axiomatized) theory of the ordering of the rationals is $\aleph_0$-categorical (hence complete, hence decidable); and Hintikka's overly swift explanatory inference is infirmed.

• *On set theory.* The author has very critical things to say about set theory, motivated by his understanding of what happens when one uses IF first-order formulae to talk about the universe of sets. But he provides no clues at all as to what sort of axiomatization he is entertaining for set theory *within* an IF first-order language. Is it just the usual axioms (ZFC, say) of the ordinary first-order language based on the membership predicate? Or will the schemata of separation and replacement now have substitution instances involving IF first-order formulae as substituends?

Hintikka claims that 'set theory is inappropriate as a basis of mathematics' and cites in support of this view

> its descriptive incompleteness. This is the reason why set theory cannot provide aims, much less guidelines, for a search [for] increasingly stronger new assumptions which can be thought of as providing better deductive methods for dealing with it. (p. 100)

But there is a very special reason for the descriptive incompleteness of any particular theory of sets. It has to do with the inexhaustibility, and the indefinite extensibility, of the universe of sets meeting any given characterization. Reflection principles and large-cardinal axioms are ways of trying to make this ontological richness explicit. Indeed, precisely this 'ontological essence' of an ever-receding horizon is what motivates a major new breakthrough, due to Harvey Friedman, in the re-axiomatization of existing set theory by means of simple, elegant principles. This affords the prospect of generating well-motivated new axioms of set theory in future. Friedman has re-derived what I shall call ZF* (ZF without Foundation, and with Reflection in place of Replacement) from a couple of axiom schemes (besides extensionality) of attractive simplicity. One of the schemes expresses separation for a distinguished subworld W; the other is a form of Russellian reducibility 'in W', designed to give expression to the indefinite extensibility of the ontology of sets:

Subworld separation:

$$\forall x(x \in W \to \exists y \in W \forall z(z \in y \leftrightarrow (z \in x \ \& \ \phi))),$$

where ϕ does not have y free;

Reducibility:

$$\forall x_1 \ldots \forall x_n \forall z[(x_1, \ldots, x_n \in W \ \& \ \phi(x_1, \ldots, x_n, z)) \\ \to \exists y \in W \phi(x_1, \ldots, x_n, y)].$$

The effect of Reducibility is to make the subworld W elementarily indistinguishable from the 'wider universe beyond' W. Friedman's main metatheorem is that his theory has exactly the same W-free theorems as ZF*. By slightly altering the form of Reducibility, Friedman is also able to derive the consistency of ZF augmented by various large-cardinal axioms.

The very existence of this innovative approach undermines Hintikka's dismissive claim that 'set theory cannot provide aims, much less guidelines, for a search [for] increasingly stronger new assumptions'. Set theory is condemned to a permanent state of descriptive incompleteness (that is, there can be no categorical theory of the universe of sets) because of the very nature of sets; while yet set theory can be employed to reduce forever the extent of deductive incompleteness of any branch of mathematics, by generating new axioms in a principled way that will settle hitherto undecided statements of the branch in question. Given the essential incompletability of mathematics (which is not the same as the existence of absolutely undecidable mathematical statements) it is no surprise that any theory aspiring to be all-encompassing for mathematics will be saddled with an ontology that cannot be harnessed in any one system. It is an open-ended epistemic paradox: set theory, though condemned to a perpetual lack of categoricity, is nevertheless the only theory in sight that now offers the remotest prospect of perpetual deductive completion of the main areas of mathematics.

Another major recent metatheorem of Friedman's in this connection is a finite independence result. He shows that a particularly 'concrete' combinatorial statement proves the consistency of ZF augmented by some very-large-cardinal axioms, and is itself provable by assuming an even larger cardinal. Thus astronomical upper reaches of the universe of sets are required in order to secure the truth of the combinatorial statement. This is the first time that this constellation of logical possibilities has been instantiated.[2]

[2] See H. Friedman, 'Finite independence results in set theory', forthcoming in *Annals of Mathematics*. Note that the Paris-Harrington independent sentence, by contrast, can be proved in a very weak fragment of ZF. In order to establish the truth of Friedman's

It also completely undermines Hintikka's rejection of axiomatic set theory and his taking refuge instead in the allegedly 'razor-sharp [combinatorial] characterization' of structures such as jigsaws or tiling patterns (p. 206). Friedman's work shows that such combinatorial statements themselves, in simple, accessible, and fundamental formulations, can be just as imponderable as some very-large-cardinal existence claims. The combinatorial statements are not at all 'clear cut' in contradistinction to set-theoretic claims, as Hintikka would have us believe. Moreover, given his rejection of excluded middle, Hintikka should be the last person to be urging (*loc. cit.*) that '[e]ither there exists a structure of a certain kind or else there does not exist one'.

• *On the exorcism of Tarski's curse, and New Age Negation*

> [T]he apparent dependence of Tarski-type truth definitions on set theory is in my view one of the most disconcerting features of the current scene in logic and in the foundations of mathematics. I am sorely tempted to call it 'Tarski's curse'. (p. 16)

It is worth looking closely at what Hintikka later proposes as an 'exorcism of Tarski's curse' (p. 117).

The reason why the Liar does not arise for IF first-order languages is not at all the one offered on pp. 142ff. The reason is not that the Liar sentence would be neither true nor false. Rather, it is that Hintikka's notions of truth and falsity, and his operation of 'negation', do not behave as required by the three rules given above (in the subsection on falsity and negation). We can use the Hintikka 'negation' to get a 'negation' of the 'truth' predicate and even obtain a fixed point for this 'negated' truth predicate by diagonalizing. But without the three rules just mentioned, we cannot arrive at the usual contradiction as we would for a *materially adequate and formally correct* definition of truth within a language with a *well behaved* negation. The situation has nothing to do with excluded middle!—for we are dealing here with Tarskian reasoning that holds intuitionistically. Hintikka's exorcism has let other devils in through the back door.

Along with New Agers' belief in exorcism goes their tendency to lend credence to just about any proposition, even in the face of a definitive refutation of the proposition concerned. New Agers do not like to be negative. But a rational agent is concerned to know: What is going on if one cannot form the 'explicit, initial' negation of any given sentence? How can a discipline advance if thinkers cannot in general (even if mistakenly) explic-

independent sentence, one *is required* to postulate a very large cardinal. The independent sentence in question is of immediate interest to graph theorists and computer scientists. In other work in progress, Friedman shows that certain easily graspable descriptions of *corporate management structure and procedures* prove the consistency of various large cardinals! (The business-management metaphor is simply a way of giving a concrete interpretation to the combinatorial statement involved.) Thus in some sense the structure of the whole universe of sets is all around us, in the everyday world!

itly deny any given claim? And why *should* one have to do more, in order to be entitled to deny a claim, than simply refute it? Hintikka provides no answers to these questions; and one can with justification say that his 'negation' operator delivers at best New Age Negation.

• *On Hintikka's definition of 'true in L' in L.* Quite apart from my worries over the inferential price Hintikka is willing to pay in order to draw the Liar's sting, I have reservations about the legitimacy of his truth definition via the predicate TR[X] defined on p. 115.

First, various conjuncts in the definition have quantificational structures $(\forall z)(\ldots \mathbf{z} \ldots)$ where, as explained on p. 113, the 'numeral which represents a natural number n will be referred to as **n**.' Thus the boldface **z** stands for the *numeral* for whatever number might be assigned to the variable z. This means that in a really explicit formalism, **z** would have to be replaced by some defined functional expression *numeral-for(z).* This function in turn can be defined only for standard natural numbers. It would therefore appear that the quantifiers $(\forall z)$ appearing in various conjuncts in the definition of TR[X] have to be interpreted as 'for every *standard* natural number z'; and this, arguably, imports the higher-order notion 'is a standard natural number' into the definition. Even if this misgiving is ultimately mistaken, it strikes me as a serious omission on the author's part not to have dispelled in advance whatever misunderstanding the misgiving may rest upon.

Secondly, Hintikka's truth definition, as given in the text, fails to capture the right extension for '... is true (in arithmetic)'. Inspection reveals that TR[X] would hold (according to the definition given) when X is the notion *true atomic sentence*. At the very least it would appear that the terminal conditionals of conjuncts (a)–(d) ought to be firmed up to biconditionals in order to avoid this untoward result. (The mistake is perpetuated in Sandu's Appendix on p. 260.)

The reader would be justified in refusing to accept Sandu's proof of Proposition 1 on p. 261. (This is the alleged result to the effect that the truth definition succeeds.) It is not enough to conclude in the inductive step, as Sandu does, in the case where ϕ is of the form $\forall x\psi$, that

$$(N, A) \models \forall x X(\mathrm{Sub}\,(`\psi\text{'},\ `x\text{'},\ x)).$$[3]

For we need the stronger conclusion

$$(N, A) \models X(`\forall x\psi\text{'});$$

but in taking the needed step to this stronger conclusion, one would be affirming the consequent in one's implicit appeal to clause (A.2.6) in the definition of $L(X)$. For that clause embeds only the detachable conditional

$$X(`\forall x\psi\text{'}) \rightarrow \forall x X(\mathrm{Sub}\,(`\psi\text{'}, `x\text{'}, x));$$

[3] Sandhu mistakenly omits one of the arguments of Sub here.

whereas it is the converse of this conditional that is needed.

Worse, Sandu omits the reasoning for the most difficult case in the inductive step, namely the one which would have dealt with the innovative, information-independent quantifiers! Given the grave defects in his treatment of these quantifiers already pointed out above, this is an inexcusable omission.

It is worth noting that even if these defects in the formal proof could be repaired, all that will have been shown is only that an IF predicate has been defined whose extension in the standard model N is the set of Gödel numbers of those IF sentences that are true in N. We have not been given an explicit definition of 'ϕ is true in M' for *arbitrary* models M; nor for 'ϕ is a true sentence of arithmetic', defined *without any set-theoretic reference* to the standard model N. Nor can the defined notion T( ) be shown to be at least materially adequate, in the sense of validating the Tarski inferences

P; therefore T('P').

T('P'); therefore P.

• *On negation, and the alleged failure of the law of excluded middle in the IF extension of classical logic.* Chapter 7 begins by promising an account of why the Liar-inspired diagonalization method will not count against the IF-definability of truth for IF sentences. But, as pointed out above, the failure of the law of excluded middle would be irrelevant anyway to the proofs involved in this method; for they are all intuitionistic. So it is all the more unsettling that Hintikka mislocates the 'deviation' of IF-logic from ordinary logic. That deviation, to repeat, is not that excluded middle fails, but rather (at best) that IF-languages might in general fail to furnish sentences whose assertions would be the logically well behaved denials of claims made by other sentences of the language. (It is worth noting that this complaint can be made both by the intuitionist and by the classicist.)

How is it that an IF language can fail to express the negation of one of its own sentences? Take, for example, the oft-occuring exemplar of IF first-order formulae in the text, namely

$$\left.\begin{matrix}\forall x \exists\, y \\ \forall z \exists\, w\end{matrix}\right\} F(x, y, z, w) \tag{3}$$

Informally, this is to be read as follows:

Whatever is chosen for x one can choose some y (using only information concerning the earlier choice for x) and whatever is chosen for z one can choose some w (using only information concerning the earlier choice for z) such that $F(x, y, z, w)$.

Imagine that we wish to deny this by asserting its negation. That is, we want to say something like $\sim$(3), *i.e.*,

$$\sim: \left.\begin{matrix}\forall x \exists\, y \\ \forall z \exists\, w\end{matrix}\right\} F(x, y, z, w)$$

If this were well-formed (which appears not to be the case for Hintikka), then according to the game rules, anyone wishing to assert this claim would find himself playing as the falsifier on (3). But that would be exactly like playing as the verifier on

$$\left.\begin{matrix}\exists x \,\forall y \\ \exists z \,\forall w\end{matrix}\right\} \sim F(x, y, z, w) \tag{4}$$

which is the negation normal form of $\sim(3)$. But is (4) really the contradictory of (3)? The answer is negative. For the second-order Skolemization of (3) is

$$\exists f \exists g \forall x \forall z F(x, f(x), z, g(x)).$$

The contradictory of (3) would therefore have to be equivalent to the negation of this second-order formula, namely to

$$\sim\exists f \exists g \forall x \forall z F(x, f(x), z, g(x)),$$

which in turn is classically equivalent in second-order logic to

$$\forall f \forall g \exists x \exists z \sim F(x, f(x), z, g(z)).$$

But the latter is *not* equivalent to the IF first-order sentence (4). (4) cannot therefore serve as the contradictory of (3) within the IF first-order language. He who would deny (3), therefore, cannot do so by asserting its Hintikka negation within IF first-order logic. Informally, (4) can be read as

> One can choose some some x such that whatever y is chosen (using only information concerning the earlier choice for x) and one can choose some z such that whatever w is chosen (using only information concerning the earlier choice for z), it is not the case that $F(x, y, z, w)$.

But this is much stronger than saying that it is not the case that (3).

Hintikka does not despair, as he should, over this expressive incompleteness of IF logic. He simply draws the triumphant conclusion that excluded middle fails for IF logic. He claims that what happens with examples like (3) above

> is not only that *tertium non datur* fails, but that the contradictory negation of a sentence is not expressible in the IF first-order language. (p. 138)

But no such conclusion is justified for any language that fails to furnish genuine negations. The word ‘only’ in the last quotation should be omitted! That is, Hintikka should be saying that what happens with examples like (3) above is *not* that *tertium non datur* fails, but *only* that the contradictory negation of a sentence is not expressible in the IF first-order language. To say that the law of excluded middle fails for IF first-order languages is like accusing someone standing outside a car of breaking the law by not having his seat-belt fastened.

Hintikka is simply defining two game-theoretic notions concerning a sentence P and a model M—(i) player T has a winning strategy on P in M; and (ii) player F has a winning strategy on P in M—and then *stipulating* that these respectively explicate the semantical claims (1) P is true in M; and (2) P is false in M. He is able to display cases (of a sentence P and a model M) where it is neither the case that (i) nor the case that (ii). If (and only if) his identification of (1) with (i), and of (2) with (ii) were correct, would he then be entitled to say that P is neither true nor false in M—that is, that bivalence fails to hold for P. But *this* would not yet be failure of the law of excluded middle, *unless* the language in question contained an operation $n(\ )$ such that for all P, $n(P)$ is true if and only if P is not true. But IF languages precisely lack any such operation.

Note that I am not merely requiring an operation such that for all P, and for all M, player T has a winning strategy on $n(P)$ in M if and only player F has a winning strategy on P in M. For there is an operation of the latter kind, namely taking (as $n(P)$) what would be called the negation normal form of $\sim P$, as we did for (3) above. Given the game rule of role-reversal on occurrences of $\sim$ we have the immediate result that each of the players would have a winning strategy on $n(n(P))$ if and only if he had one on P (against the background of any model M whatsoever). Thus, *if* (1) could be identified with (i), and (2) with (ii), we would have the remarkable result that $n(n(P))$ was logically equivalent to P while yet excluded middle $P \vee n(P)$ failed.

And *this* is precisely what shows that in this case (1) cannot be identified with (i), nor (2) with (ii). The reason is that *any genuine negation operator obeying the usual introduction and elimination rules will obey excluded middle if it obeys double negation elimination*. What IF logic lacks is an operation $n(\ )$, interpretable by a semantical game rule, for which the usual introduction and elimination rules would *both* hold:

$$\begin{array}{c} \overline{\quad P \quad}\ (i) \\ \vdots \\ \bot \\ \hline n(P) \end{array}\ (i) \qquad\qquad \frac{P \quad n(P)}{\bot}$$

For Hintikka's 'negation', the elimination rule holds; but the introduction rule fails. This is because (modulo other assumptions about a given model) there are ways of refuting the assumed 'truth' of P—that is, the assumed existence of a winning strategy for player T—without its being the case that player F would have a winning strategy. (Indeed, Hintikka himself provides just such a case on p. 135.) Thus the game-theoretic notion of 'falsity' (*i.e.*, existence of a winning strategy for player F) does not have the right relationship to the notion of mathematical refutation for any associated 'negation' operator to function the way *ordinary mathematical reasoning—be it classical or constructive—requires it to.* Not even the essentials of intuitionistic reasoning are conserved in the IF account!

Indeed, let us call *polar* (within a logical system) any sentence that counts as logically true or logically false in that system. (If the system is set up only syntactically, then the polar sentences are those that either are theorems or are provably inconsistent.) Consider the sentences $\phi \vee \sim\phi$, $\sim(\phi \vee \sim\phi)$, $\sim\sim(\phi \vee \sim\phi)$. All three are polar in ordinary classical logic. The second and third are polar in intuitionistic logic. But *none* is polar in IF first-order logic.

Hintikka owes the classical logician a detailed argument as to why one should equate correct denial with possession of a winning strategy as player F when the game is one of *imperfect* information. But no such argument is forthcoming, apart from dogmatic assertions of 'naturalness'. The equation of correct denial with possession of a winning strategy as player F is acceptable in the game of *perfect* information, precisely because we know, game-theoretically, that in such games exactly one of the players has a winning strategy—so that T *fails* to possess a winning strategy if *and only if* F *does* possess one. But in games of imperfect information the left-to-right direction fails; so the identification of the game-theoretic conditions of correct denial would be moot.

This being so, it strikes this critic as reasonable to insist, on behalf of the classical (resp., intuitionistic) logician, that the game-theoretic condition of correct denial within IF languages be so identified as to save as many of the classical (resp., intuitionistic) phenomena as possible. But, alas, hardly any of the classical phenomena can be saved if one equates correct denial of P with T's possessing a winning strategy on $\sim P$, or (equivalently) with F's possessing a winning strategy on P. We still need a principled argument for the conclusion that, in games of imperfect information, one should equate correct denial with possession of a winning strategy as player F—at least, given the current game rules. There is no point in choosing what appears to be the only peg around one, if the peg is a round one and the hole is square.

• *On the alleged failure of Tarski's adequacy condition on any theory of truth for IF first-order languages.* The argument for the failure of Tarski's adequacy condition is the most puzzling part of the book. I shall quote it at length (pp. 138–9):

> ...consider Tarski's T-sentences. They are substitution-instances of the schema
>
> $$\Pi \text{ is true} \leftrightarrow p \qquad \text{(T)}$$
>
> where "Π" is a placeholder for a quote or a structural description of the sen-

tence which is to replace "p". (T) is equivalent to

$$(\Pi \text{ is true} \,\&\, p) \vee (\Pi \text{ is not true} \,\&\, \sim p) \qquad (\mathrm{T})^*$$

$(\mathrm{T})^*$ clearly entails

$$p \vee \sim p.$$

> In other words, any sentence whose truth can be characterized by means of an explicitly formulated instance of the T-schema satisfies the law of excluded middle; it has a contradictory negation in the same language. But if so, Tarski's T-schema is useless in its usual form in IF logic.

The first peculiar feature here is the circuitous passage negotiated 'from'(T) via $(\mathrm{T})^*$ to $p \vee \sim p$. The crucial half of the equivalence of (T) and $(\mathrm{T})^*$ is from left to right; and this requires the use of excluded middle *in the metalanguage*. Indeed, in this passage the truth predicate plays no role whatsoever. One might as well replace 'Π is true' by the propositional variable q. The claim is that the biconditional $q \leftrightarrow p$ (classically) implies $(q \,\&\, p) \vee (\sim q \,\&\, \sim p)$ which in turn (intuitionistically) implies $p \vee \sim p$. The first of these implications is most naturally proved by using excluded middle (on p)—and can be proved *only* by means of a classical rule equivalent to excluded middle. Thus the combination of the two implications boils down to proving the law of excluded middle by assuming it!

The telling point is that both p and q, in the context of the discussion of truth theory, are *sentences of the metalanguage*. And excluded middle (or one of its equivalents) is being assumed to hold *in the metalanguage*, in the course of 'proving' metalinguistic excluded middle $p \vee \sim p$ from (T). Hintikka might as well have replaced the whole quoted passage with the simple observation that 'the metalanguage is classical; so in it we have the law of excluded middle $p \vee \sim p$'.

But consider now the ensuing strange twist taken in Hintikka's reflections upon arriving, via his unnecessarily circuitous classical route, at the law of excluded middle: '... any sentence whose truth can be characterized by means of an explicitly formulated instance of the T-schema... has a contradictory negation *in the same language*.' (My emphasis.) The negation sign $\sim$ appearing in $p \vee \sim p$, however, is *in the metalanguage. Nothing in Hintikka's argument supports the conclusion that Tarski's adequacy condition cannot, or should not, be maintained on any theory of truth for an IF language.* Just how puzzling his attempted demonstration is can be appreciated from the standpoint of an intuitionist, for whom all instances of (T) must hold, while yet of course excluded middle is not a valid law.

• *On the alleged mismatch between a set-theoretical Liar sentence and the statement of its truth conditions.*

> Let [the truth predicate] be $T[x]$ and let $g(S)$ be the Gödel number of S. Then by the diagonal lemma there is a number n [equal to $g(\sim T[\mathbf{n}])$]. *Along the lines*

> *indicated above* one can see that $\sim T[\mathbf{n}]$ is true but that the sentence... $T[n]$ which attributes the truth predicate to its Gödel number is false.... The failure of Tarski's T-schema for the [Liar] means that there inevitably exists in any model of set theory a set-theoretical sentence which is true in the ordinary sense of the word but whose truth condition, when expressed set-theoretically, is false (p. 174)... the earlier paradoxes of set theory are all caused by there being, so to speak, too many sets required to exist by some set-theoretical assumption or the other. The charge I am making against the usual formulations of set theory is that it does not allow the existence of functions (sets) which ought to exist, and in this sense assumes too few sets to exist. (p. 177) (My emphasis.)

Hintikka is speaking here of the Skolem functions whose existence is asserted by his brand of statement of the truth conditions of a sentence—in this case, of the Liar. But this reader failed to find the 'lines indicated above' by means of which one would allegedly be able to see that the set-theoretical Liar is *true* according to Hintikka's definition. Indeed, his claim here *contradicts* his earlier one (p. 142) to the effect that 'The Liar sentence merely turns out to be neither true nor false'.

Even though the latter claim was in the context of arithmetic, the author has failed to show that there is any principled difference between an arithmetical Liar and a set-theoretical Liar, which would be capable of making the former neither true nor false, but the latter true. The arresting ontological point was supposed to be that one can find pervasive examples of true sentences for which the verifying Skolem functions (whose existence is required by the statement of their truth conditions) nevertheless fail to exist. This was supposed to be an indictment of axiomatic set theory, of an even more serious kind than, say, Skolem's paradox about the power set of ω in a countable model. But the point appears to be founded on a confusion, which the author has not cleared up. Moreover, since the downward Löwenheim-Skolem theorem holds for IF first-order logic anyway, the Skolem paradox itself will arise for any set theory expressible in an IF first-order language.

• *On IF logic as a framework for mathematical theorizing.* It is a pity that the claims of IF logic to be able to capture certain notions not definable in ordinary first-order logic are not well served by the mistaken definitions that the author offers. The definition (9.1) of equicardinality on p. 186 needs its last occurrences of x and y interchanged. And the two equivalent definitions (9.4) and (9.5) on p. 187 of the infinity of the universe of discourse only succeed in ensuring that the universe contains more than one individual! (The definitions come out true whenever there is a non-trivial permutation of the domain.)

It should also be noted that some of the concepts—well-ordering, mathematical induction, power set, continuity—require *extended* IF first-order logic, in which a contradictory negation is assumed, for which there is no game-theoretical explication. Moreover, in extended IF logic the Liar would be reinstated, thereby making it impossible for the extended language to contain its own truth definition.

The first substantial case for the need for IF logic's expressive resources in mathematics is in connection with the notion of uniform differentiability on p. 74. The function $f(x)$ is differentiable at each point in an interval (a, b) if and only if

$$\forall x \exists y \forall \epsilon \exists \delta \forall z \left((a < x < b \;\&\; |z| < |\delta|) \rightarrow \left| \tfrac{f(x+z)-f(x)}{z} - y \right| < |\epsilon| \right).$$

For *uniform* differentiability on (a, b), however, one needs to replace $\exists \delta$ with $(\exists \delta / \forall x)$. That is, the slope y of the curve at point x is approximable via a choice of the 'deviation' δ (in response to a specified tolerance ϵ) that is independent of the point x. Thus, since y depends on x, the choice of δ is independent of y as well. The Henkin quantifier can express this nicely:

$$\left.\begin{matrix} \forall x \exists y \\ \forall \epsilon \exists \delta \end{matrix}\right\} \forall z \left(a < x < b \;\&\; |z| < |\delta| \rightarrow \left| \tfrac{f(x+z)-f(x)}{z} - y \right| < |\epsilon| \right)$$

But this claim can also be spelled out in the language of ordinary first-order set theory; there is no pressing need to resort to IF languages for the mere expression of uniform differentiability. We could instead just use the more explicit first-order set-theoretical formula

$$\begin{aligned} &(\exists \text{ function}\, \delta : R \rightarrow R) \forall x \exists y \forall \epsilon \forall z \\ &\qquad \left((a < x < b \;\&\; |z| < |\delta(\epsilon)|) \rightarrow \left| \tfrac{f(x+z)-f(x))}{z} - y \right| < |\epsilon| \right). \end{aligned}$$

Indeed, there is every reason *not* to advance to IF languages, for this or for any other notion supposedly better 'captured' therein than by the time-honored methods of Bourbaki. For the move to IF logic deprives one of comprehensive deductive grasp of the inferential commitments one makes with one's mathematical assertions. If we wish to be able still to *prove* interesting theorems about uniform differentiability, we would be well advised to eschew IF logic and do our mathematics as usual.

• *On the illumination of constructivity provided by game theoretical semantics.* Perhaps the most disappointing and least convincing set of topical theses concerns intuitionism and constructivism. On p. 32, the author remarks that

> paradoxically, none of the consequences which constructivists have been arguing for follow from the game-theoretical definition [of truth]. In fact, as far as first-order logic is concerned, the game-theoretical truth definition... is equivalent to the usual Tarski-type truth definition, assuming the axiom of choice.

This equivalence does not actually need the axiom of choice for its proof; for the notion 'player T (or player F) possesses a winning strategy in such-and-such state of play' can be rigorously defined without any explicit quantification over functions.[4] That none of the usual constructivist consequences follows could, of course, from a constructivist's point of view, be taken simply as showing that something is wrong with Hintikka's game-theoretic modelling of sentence semantics. (One man's *modus ponens* is another man's *modus tollens*.)

And indeed there *is* something wrong. Hintikka had made no provision by the stage at which the last quote appeared for the strategies to be *effective*. Arguably, if one imposes a requirement of effectiveness, and if one is willing to reconsider the form of the game rules, then something in the neighbourhood of intuitionistic logic will result.[5] Moreover, winning strategies will actually be codified as proofs—finitary objects that can guide their possessor in actual play.

The author is too quickly dismissive of the carefully thought-out meaning theories of Dummett and Prawitz, and too quick, later on, to press a rather naïve constructivization of game-theoretical semantics into foundational service. When he finally pays attention to effective strategies, his idea is simply to leave the constitutive game rules unchanged (including, especially, the rule that requires players to exchange their roles on negations) and to require that the strategies be recursive functions.

But this immediately mangles the intuitionistic treatment of denial of a universal $\forall x F(x)$. Note that for an intuitionist to be able to deny $\forall x F(x)$, no constructive counterexample $F(t)$ is needed; all that is needed is a proof that inconsistency would follow were there to be a proof of an arbitrary instance $F(a)$. Imagine now defining an undecidable predicate $P(\ )$ of natural numbers and asking whether $\forall x(P(x)\ \&\ {\sim}P(x))$. The intuitionist can immediately assert ${\sim}\forall x(P(x)\ \&\ {\sim}P(x))$ on the basis of the obvious *reductio* proof using elimination rules. This is perfectly reasonable; the statement can be refuted on the basis of its logical form alone. But Hintikka's constructivist would, by contrast, have to possess a recursive winning strategy as player T on ${\sim}\forall x(P(x)\ \&\ {\sim}P(x))$. Thus he would have to have a *recursive* winning strategy as player F on $\forall x(P(x)\ \&\ {\sim}P(x))$. He would have to be able to choose a value t for x, and then choose which of $P(t)$ or ${\sim}P(t)$ to maintain as false. So he would be required, in effect, to effectively produce a *decided instance* of P! This is surely to ask too much of him.

Hintikka does not consider these logical basics before giving his critique to the effect that, simply because matters turn out differently on his 'constructivization', it must be the other theorists who are in the grip of some deep misapprehension about the true nature of mathematical constructivity. Philosophically, 'Constructivism reconstructed' is by far the least satis-

[4] See N. Tennant. *Natural Logic*. Edinburgh: Edinburgh University Press, 2nd ed., 1990, pp. 35–7.

[5] See N. Tennant, 'Language Games and Intuitionism', *Synthese* **42** (1979), 297–314.

factory chapter, marred both by misrepresentation of opponents' views and failure to make certain fundamental distinctions, such as that between the proof-theoretic rules of existential elimination and existential introduction (p. 216).

• *Standards of book production.* The author has been badly served by the Press. The copy-editor, it seems, seldom intervened in the interests of better English style on the part of a non-native speaker. There are many solecisms, particularly ones involving prepositions. There are sense-destroying lexical substitutions or omissions. This reader has never before found so many spelling mistakes and typos in a scholarly book, particularly ones affecting crucial logical formulae.

The type-setting is awful. Corner quotes feature as a sort of typesetter's confetti. They often languish in the margins, or are to be located somewhere between 10 and 11 o'clock, an inch or so away on the page from where they ought to be, occluding other characters. One has to diagnose corner quotes from the vacant spaces around formulae from which they have been blown away.

Cramped footers and margins make the reader's annotations difficult.

The subject and title index is sub-standard, and the list of references is incomplete. The subject and title index, for example, has no entry under 'C' for combinatorial, nor under 'F' for Frege's alleged fallacy. There are no entries *at all* under 'H' or under 'Q', despite the Henkin quantifier being the mainstay of the author's examples. There is no entry for 'program', despite the Hilbert program featuring prominently in the philosophical discussion. There is no entry for 'negation', a major topic in the book; nor for 'negation normal form', a crucial notion deployed in the author's favored logic. Under 'S', the reader will find no entry for 'strategy', and this in a book devoted in large measure to a game-theoretic semantics for quantified languages. Nor is there any entry for 'Skolem normal form', despite much being made of it. Under 'V', there is no entry for 'verifier' or 'verification', despite much discussion. It is no defence to say that for topics identified with a proper name and a noun (such as 'Frege's fallacy'), one should have recourse to the index of names; for that entails the inconvenience of paging through many irrelevant occurrences of the name in question before encountering the topic associated with that name.

The reference Hilbert 1923 (p. 40) is not in the list of references.

These are not minor quibbles; distinguished authors and serious scholarship devoted to their work should be better served by such a distinguished Press.

Finally, a word of caution to the unwary reader about a more unusual kind of mark that the book might leave: do not discard the dust jacket, or you will have blue-black fingers and thumbs in next to no time. I earnestly hope that the cover's dye is not toxic.