Jaakko Hintikka

GAME-THEORETICAL SEMANTICS AS A CHALLENGE TO PROOF THEORY

Game-theoretical semantics (GTS) is but a systematization of the familiar "epsilon-delta" technique of mathematicians. It relies essentially on the game-theoretical notion of strategy for its truth definition. Since we want to express all possible patterns of dependence among variables by means of our logic, we must replace the received first-order logic by an independence-friendly (IF) logic. This logic is semantically incomplete. Hence we cannot characterize the meanings of logical notions by their inference rules. This changes the role of logic in mathematics from fallacy policing to a search for stronger logical principles. Since a typical mathematical problem is equivalent to the validity of some IF first-order formula, this makes a (reoriented) proof theory the engine of discovery in mathematics.

The theory known as game-theoretical semantics (GTS) is revolutionizing logic and the foundations of mathematics. (For this research approach see Hintikka and Sandu 1997; for its impact on the foundations of mathematics, see Hintikka 1996.) For one thing, it prompts immediately the question whether the semantical games this approach operates with are games with perfect information or not. Since the latter answer is self-consistent, GTS leads inevitably to the development of a logic in which moves connected with nested quantifiers can be informationally independent of each other. This logic is known as independence-friendly (IF) logic. Since it is the unrestricted basic logic of quantifiers, it is nevertheless unnatural to give it a qualified name. Instead, it should be called the logic of quantifiers or first-order logic *simpliciter* and the received first-order logic should be called dependence-handicapped logic. (Or would it perhaps be more correct politically to call it independence-challenged logic?) By means of IF logic, we can among other things obtain a radical *Aufhebung* of Tarski's famous impossibility theorem concerning the definability of truth. This

Nordic Journal of Philosophical Logic, Vol. 4, No. 2, pp. 127–141.

impossibility is generally blamed on the fact that the relevant first-order languages are too strong, so strong that the Liar Paradox arises in them, making a truth-definition impossible. In reality, the Tarskian impossibility is due to the relevant languages being too weak in their expressive capacities: informational independence between quantifiers is not always expressible in them (see Hintikka 1998). When this defect is corrected, truth-predicates for a suitable first-order language can be formulated in the same language (cf. Hintikka, forthcoming).

In IF first-order logic, the law of excluded middle does not hold. In other words, the negation defined by the usual GTS rules is a strong (dual) negation, not the contradictory negation. By adding a sentence-initial contradictory negation, we obtain a stronger extended IF first-order logic.

But what is the history of this powerful new logic? Where did it originate? How old is it? Even though its explicit, self-contained formulation of GTS is of a relatively recent origin, there is a rich history of the use of game-theoretical ideas in logic and in mathematics. This history is in fact so rich and subtle that one cannot do justice to it in one paper. The referee of this paper has kindly reminded me of such names as Borel, Brouwer, Novikov, Lorenzen and Thierry Coquard. One key to their role is the relationship of game strategies to the notion of choice sequence. Another logician might prefer to try to link the subterranean history of GTS to such ideas as Skolem function or Hilbert's epsilon-technique. For the purposes of this paper, I will restrict my attention to what is the most explicit anticipation of the game-theoretical interpretation of quantifiers. It was discovered by Hilpinen (1983). He shows that Charles S. Peirce presented an explicit game-theoretical interpretation of quantifiers, complete with two players ("utterer" or "defender" vs. "interpreter" or "opponent"). Among other things, Peirce realized clearly that the difference between an existential and a universal quantifier lies in the player who makes the choice of the value of the variable bound to it. He is likewise cognizant of the importance of the order in which the different moves in the quantifier games are made, and so on. Most importantly, game-theoretical ideas were not used by Peirce as a gimmick to interpret logical inferences, but as spelling out the very meaning of quantifiers.

Peirce's game-theoretical perspective on quantifiers and on their logic was not merely an illustration or a metaphor. It helped to sensitize him to some of the most interesting aspects of the behavior of quantifiers, for instance to the importance of quantifier ordering and to their role in spelling out some of the most important mathematical concepts, such as the concepts of continuity and derivation. The significance of these insights is perhaps appreciated more keenly if one compares Peirce with Frege, who is usually thought of as the father of quantification theory. Frege never seems to have emphasized or as much as appreciated the role of quantifiers as the way of implementing the Weierstrassian epsilon-delta technique which was the backbone of the entire arithmetization of analysis. One might thus suggest that Peirce was a better quantification theorist than Frege. If so, his superiority stems from the same source as his awareness of the game-theoretical interpretations of quantifiers.

But this suggestion prompts a historical near-paradox. Why did Peirce fail to make any significant use of the game-theoretical interpretation of quantifiers? For fail he did; his game interpretation remained a semiotic curiosity which did not play any overt role in his logical theory. What went wrong? What is it about the use of game-theoretical notions that has later led to spectacular developments but was missed by Peirce? Clearly it is not the use of game-theoretical terminology.

The answer is surprisingly simple. Whatever else we might require of an adequate logical language, it must be able to express all possible patterns of dependence and independence between variables. Now such dependences between variables are in a first-order language expressed by dependences between quantifiers. For instance, y depends on x according to the law $D[x,y]$ if and only if the following are true:

$$(\forall x)(\exists y)D[x,y] \tag{1}$$

$$(\forall x)(\forall y)(\forall u)((D[x,y]\,\&\,D[x,u]) \supset y = u) \tag{2}$$

Here the dependence of the variable x on y is expressed by means of the dependence of the quantifier $(\exists y)$ in (1) on another one, i.e. $(\forall x)$. More complicated patterns of dependence and independence between variables can be expressed by more complicated patterns of dependence and independence between quantifiers.

But what precisely is meant by a dependence relation between quantifiers? Received logical theory does not offer us any built-in explanation beyond some intuitive pretheoretical idea of dependence. It is here that game-theoretical semantics comes to the rescue. In game theory, there is ready and available an explicit concept that captures the relevant idea of dependence. It is the notion of informational dependence of a move on another in a strategic game. If and only if the player making a move $m2$ knows what happened at another move $m1$, is $m2$ dependent on $m1$. This is also expressed by saying that move $m1$ is in the information set associated with move $m2$.

Game-theoretical concepts are not the only ones that can be used to spell out such dependence relations, although I believe that they are by far the most natural ones. However, one thing is made absolutely clear by the game-theoretical interpretation. It is that dependence relations cannot always be specified by recursive (inside-out) rules of any sort. The reason is that such rules presuppose compositionality, that is, semantical context-independence. And that in turn presupposes that dependence relations are transitive. And the fact that this is simply not true in general is beautifully clearly brought out by GTS. For there, dependence of the kind dealt with here is naturally construed as informational dependence. The set of earlier moves that the player making a move is at the time aware of is called in game theory the information set of that move. Then the transitivity of dependence would require that the information sets are transitively ordered by class-inclusion. And there is no viable reason why this should be the case. It is not always true in game theory. The first question a true aficionado of game theory would ask about semantical games is in fact: Are they games with perfect information?

It is important to realize that this need of admitting nontransitive dependence relations is not an architectonic desideratum imposed on our logic for logic's sake. This need is forced on us by having to be prepared to use our logical language to discuss all different possible configurations of dependence and independence between variables. This is not a matter of logical inference relations but of the ability of our logical language to deal adequately with the world. There cannot be any a priori reason to dismiss sight unseen certain patterns of dependence and independence between real-life variables.

And yet this is what the received Frege-Russell logic does. In it, dependence is indicated by scopes, and in the usual notation those scopes are of course partially ordered. (The usual scope notation is equivalent to the labeled tree notation used by linguists.) Hence completely independently of whether we like to use game-theoretical concepts or not, the ordinary first-order logic has to be extended so that we can by its means conceptualize all possible patterns of dependence between variables and *a fortiori* between quantifiers.

This extension is what is accomplished by IF logic. Philosophical logicians seem to think that the typical IF formulas not reducible to received first-order logic are so-called branching quantifier structures, such as the Henkin quantifier structure

(3) $$(\forall x)(\forall z)(\exists y/\forall z)(\exists u/\forall x)S[x, y, z, u]$$

In reality, the most interesting IF quantifier structures are the symmetrical ones, exemplified by the following:

(4) $(\forall t)(\forall x)(\forall y)(\exists z/\forall x)(\exists u/\forall y)((x = z)\,\&\,(y = u)\,\&\,S[t, x, y])$

It is also turning out, even though no results have yet been published, that such quantifier structures not expressible in ordinary first-order languages are unavoidable for the purpose of describing and analyzing actual physical phenomena. In other words, it is my expectation that very soon it will be shown that the need of extending the ordinary (dependence-handicapped) logic to IF logic is not only a matter of pure logic, but a very real need for some of the most important applications of logic.

Most philosophers seem to think of IF first-order logic as if it were little more than the study of branching quantifiers. This is a misperception. It is already amply clear that the most important applications of IF logic are elsewhere on the one hand in the area of mutually dependent quantifier structures and on the other hand in epistemic logic, where informational independence turns out to be the lifeblood of the entire epistemic logic.

Mutually dependent quantifiers are especially instructive because among other reasons they show the futility of trying to formulate a half-way natural compositional semantics for IF logic. This futility is illustrated by the fate of attempts to restore compositionality by allowing other than sentence-initial occurrences of the contradictory negation. An example of such an attempt is found in Janssen 1997. Such attempts falter on the so-called strong liar paradox. In an arithmetical IF first-order language, we can formulate a truth predicate $T[x]$ for the same language. This $T[x]$ says that the IF sentence with the Gödel number x is true. It is *a fortiori* a formula of the corresponding extended IF language, and so is a $T[x]$, if contradictory negation is allowed to be prefixed to an open formula. In this language, the diagonal lemma is clearly valid, producing a Gödel number n (expressed by the numeral n) which is the Gödel number of the sentence,

$$\neg T[\mathsf{n}]$$

But this sentence is true if and only if it is not true, which is a contradiction. Hence compositionality cannot be restored in this way without contradiction. The same problem does not arise with the strong negation $\sim$ because the analogous sentence

$$\sim T(\mathsf{n})$$

need not be either true or false.

One particularly instructive game-theoretical concept is the crucial concept of all game theory, viz. the concept of strategy. In GTS, strategies govern the choice of individuals by the two players. These choices depend on the moves that are in the information set of the player who is making the move. Such strategies can be expressed by the set of functions that guide the verifier's choices. These functions are known as Skolem functions. What the information set of a move by the verifier is, is indicated by the sets of arguments of these different functions. Once this is understood, it is seen that there is no valid reason to restrict the choices of the argument sets of Skolem functions to those allowed by the arbitrarily restricted logic of Frege and Russell. The central role of strategies in GTS is highlighted by the fact that the truth of a sentence S is there defined as the existence of a winning strategy for the initial verifier in the game $G(S)$ correlated with S. And this means the same as the existence of a full set of Skolem functions (in a slightly extended sense where the verifier's choices of disjuncts are also expressed by functions) for S. From what has been said it follows that this concept of truth cannot be captured by any recursive (compositional, inside-out) rules.

This helps to answer also my historical question about the reasons for Peirce's failure to put his game-theoretical interpretation of quantifiers to systematic use. What he missed was the concept of strategy, which was in game theory introduced by von Neumann (and perhaps also by Borel) in the late twenties. There are some indications that the general idea of strategy was never grasped firmly by Peirce in other contexts, either.

Likewise, Skolem functions (strategy functions for semantical games) were introduced by Skolem only in the twenties. The idea was there in earlier logicians such as Schröder and Löwenheim in a clumsy form, viz. in the form of double indexing. The genesis of the idea of Skolem function in logic is a counterpart to the introduction of the concept of strategy which was the starting-point of von Neumann's game theory.

Unfortunately, some contemporary philosophers insist on repeating their predecessors' failures. They do not acknowledge the potentialities of the notion of strategy, as illustrated by the role of Skolem functions in logic, and hence underestimate the promise of GTS. They are in effect adopting the same attitude to the received Frege-Russell first-order logic as Kant adopted to Aristotelian logic, viz. the belief that it has never made any real progress and that it will never do so. Since they realize that this pious belief is threatened by game-theoretical semantics and IF logic, they have resorted to desperate and in the last analysis dishonest efforts to force game-theoretical semantics to the procrustean bed of Frege-Russell logic and compositional semantics. Since they believe that the Frege-Russell logic is the only road to salvation, they make a big deal of the fact that in the special case of Frege-Russell languages the game-theoretical truth-predicate reduces to a Tarski-type one in the sense of being extensionally equivalent. On this basis, it is alleged that GTS does not yield anything new. Of course, an appeal to such truth-definitions as an argument for Frege-Russell logic is circular. On the basis of what has been said earlier, it is seen that the correct conclusion from the possibility of such truth-definitions is on the contrary the radical inadequacy of Frege-Russell logic as a general, unrestricted logic of quantification.

Another variant of the same superstition is the claim sometimes made (cf. e.g. Velleman 1999), that IF logic is "merely second-order logic in sheep's clothing". At its face value, this claim is an oxymoron. Second-order logic is defined by its involving quantification over second-order entities, which IF logic does not. What is in effect meant by the claim that IF logic is second-order logic is that one cannot formulate a compositional semantics for IF logic on the first-order level. But the joker in that deck is the assumption of compositionality. If it is given up, such crucial semantical predicates as truth can be defined for IF languages on the first-order level.

What makes things tricky here is the subtle way in which the assumption of compositionality is smuggled into the semantics of quantifiers. This is done by tacitly assuming that the meaning of quantifiers lies in their "ranging over" a class of values. This way of thinking is applicable to a number of cases, but not always, as the case of mutually dependent quantifiers shows. In reality, what quantifiers do semantically speaking is to deputize certain choice functions, as Skolem and Hilbert seem to have grasped, however half-heartedly. And if so, the assumption of compositionality becomes the assumption that the argument sets of these functions can always be linearly ordered by class-inclusion.

The fallacy of claiming that IF logic is "really" second-order logic is of course a private failure of the worshippers of the *status quo*. But their line of thought becomes suspect when they begin to claim that they can understand game-theoretical semantics within the framework of Frege-Russell logic. A ploy they have used is to introduce game-theoretical terminology and perhaps even game-theoretical interpretation to the metatheory (semantics) of the received Frege-Russell languages. Because of the severely restricted nature of these languages, they can then show that the game-theoretical truth-definition for such languages is equivalent with Tarski-type recursive truth definitions. This is then taken to show that GTS does not offer anything new as compared with Tarski-type semantics. Of course, such a conclusion follows only if you assume that Frege-Russell languages are the only show in town.

But what is worse, some philosophers have been pretending that such a pseudo-game-theoretical treatment of received Frege-Russell languages is all that there is to the real game-theoretical semantics. An example is offered by Neil Tennant's 1978 book. There he presents (pp. 35–37) what he calls game-theoretical semantics. He even mentions the game-theoretical idea of defining truth as the existence of winning strategy. But he never uses this idea or any other genuinely game-theoretical concept. He does not even have any symbols for strategies, let alone quantification over them. His truth conditions for quantified sentences are old-fashioned compositional truth-conditions which have nothing to do with game-theoretical semantics except for the fact that Tennant uses the vocabulary of games in formulating them. But to take such terminological idiosyncrasy as a reason for calling Tennant's construction "game-theoretical semantics" is to indulge in word magic. Tennant's use of the term "game-theoretical semantics" is a prevarication.

This makes Tennant's recent 1998 discussion of GTS and especially IF first-order logic totally irrelevant. In one of his mellower moods, Georg Kreisel once prefaced his comments on a paper by Imre Lakatos by expressing the wish that people who talked about logic knew something about it. It might seem that the most appropriate comment on Tennant's animadversions is to express the wish that people who talk about game-theoretical semantics understood something about it. This would not be accurate, however. The obvious reason for his being upset is that he has been betting on a different approach which GTS has shown to be philosophically irrelevant. He is not imperceptive, he is in denial.

There are plenty of examples to illustrate this point. Tennant's attempts to foist his own misconceptions on game-theoretical semanticists have in fact led him to patently false claims. For instance, in his review article 1998 he claims that in ordinary ("dependence handicapped") first-order languages it is possible to prove the equivalence of game-theoretical and conventional truth definitions with the axiom of choice. Let's see. The game-theoretical truth-condition of a sentence of the form

(5) $$(\forall x)(\exists y)S[x, y]$$

is (as Tennant himself asserts) the existence of a winning strategy in the correlated game. This existence is expressed by

$$(\exists f)(\forall x)S[x, f(x)] \quad (6)$$

But the inference schema that leads us from (5) to (6) is nothing but a form of the axiom of choice. Hence Tennant is in effect claiming that he can prove the axiom of choice out of thin air—a striking claim indeed, flying as it does in the face of Paul Cohen's results.

What Tennant is doing is embarrassingly obvious: he assumes that his truth-definition, which he misleadingly calls game-theoretical, is the same as the one used in GTS. This simply is not the case. Hence all that he accomplishes is to flaunt the fact that his truth definition is not game-theoretical in the sense of GTS. As was pointed out, it is simply a traditional truth definition camouflaged by using words and phrases from game-playing.

It is instructive to consider these misconceptions in a wider systematic and historical perspective. One of the most conspicuous reassessments mandated by IF logic concerns the status of proof theory, including its job description. This matter has quite a general interest vis-à-vis the research policy of logic. Indeed, I suspect that the new perspective of proof theory has shocked some philosophers more than anything else about GTS and hence affected the recognition of GTS and in particular the reception of IF first-order logic.

It is a plain fact that IF logic, not the received Frege-Russell logic, is our basic elementary logic. But there is one striking and perhaps disconcerting thing about it that may lie at the bottom of the reaction of philosophers like Tennant. IF first-order logic is not axiomatizable (semantically complete). The set of valid formulas is not recursively enumerable. As a consequence the set of valid IF formulas cannot be exhausted by proof-theoretical (axiomatic and deductive) means. This might seem to reduce greatly the interest and importance of proof theory. Validity still depends on logical (syntactical) form alone, but we have to identify more and more valid axioms. And this can happen only model-theoretically. A theorist of logic who has staked his fortunes on proof theory might very well be initially afraid that I am taking the bread out of his mouth. An example is again offered by Neil Tennant. Here we can see the reason why he uses in his recent animadversions on IF logic (Tennant 1998) a disproportional amount of space and printer's ink to discuss rules for formal proofs of validity, which in the best of circumstances yield only indirect indications of the truth conditions that are the real life blood of logic. But Tennant's reaction is hopelessly defensive.

Here we can in fact see one of the most important general perspectives opened by IF first-order logic. Far from reducing the interest and importance of deductive logic, the semantical incompleteness of IF first-order logic makes logic much more important in a wider context than it has been thought to be. It has been generally assumed that our basic first-order logic is semantically complete but that all interesting mathematical theories are deductively incomplete. If so, there could not be any genuinely creative discoveries in first-order logic. All the truly creative work in mathematics is done by mathematicians in their search for stronger axioms and stronger methods of proof. The discovery of stronger set-theoretical axioms would be a paradigm case of mathematical progress. Logician's basic job would be only fallacy policing, making sure that mathematicians' inferences are according to Hoyle.

In contrast, the prospects revealed by IF logic assign a much more important role to logic and logicians. I have shown (in Hintikka 1996, ch. 9) that all normal mathematical problems can be reduced to questions of the validity of formulas of IF first-order logic. Hence all the logical assumptions that mathematicians in principle need fall within the province of IF logic. The real progress does not consist in the discovery of stronger set-theoretical axioms, but of more powerful rules for establishing validity in IF logic. This is logically speaking what is happening in mathematical progress. Mathematicians' discoveries are not beyond the province of logicians; they can all be viewed as discoveries in logic. It is a symptom of deep insecurity on the part of logicians to think of their task as merely maintaining argumentative hygiene in mathematics. Such a defensive posture is now seen to be self-defeating, for the valid inference patterns of IF first-order logic cannot be exhausted by any recursive axiomatization.

Even though much work remains to be done systematically and historically, it is not outlandish to suggest that this picture of the relation of logic mathematics is more realistic than the received one. For instance, new set-theoretical axioms have not led to significant new developments in the rest of mathematics. In contrast, combinatorial theory is shot through with independent quantifiers. In logical theory, whenever conventional compositional truth-definitions have proved inadequate, logicians have spontaneously resorted to game-theoretical conceptualizations. This has happened in the theory of so-called game quantifiers, in infinitely deep logics and in the theory of branching quantifiers.

From this it is seen that proof theorists need not (so to speak) fear unemployment if they see their job in the right perspective. What has to be jettisoned are only certain misguided philosophical pretensions of some proof theorists. The most common of them is the widespread misconception that the meaning of logical constants can somehow be characterized in terms of the rules of inference that they obey. Since there is no recursively enumerable list of rules of inference for our basic unhandicapped logic, the IF first-order logic, this view is shown to be mistaken once and for all. It is perhaps not surprising that Tennant is upset by this result, for he has been committed to such a fallacious definition-by-inference-rules view. What is lamentable is that he does not offer rational arguments for his opinions, but instead criticizes game-theoretical semanticists for not sharing his misconceptions.

An example is offered by Tennant's curious demand that in IF logic one should formulate rules for all different new quantifier combinations, including even such combinations as reduce to ordinary first-order logic. His examples include

(7) $$(\forall x)(\exists y/\forall x)S[x, y]$$

What is weird about Tennant's demand is that any such rule would be totally redundant. The meaning of (7) is determined by the semantical rules explicitly formulated in any exposition of GTS, without needing any additional rules. As is clear to anyone who understands (7), it is equivalent to the ordinary first-order sentence

(8) $$(\exists y)(\forall x)S[x, y]$$

(If you have to make the choice of the value of y independently of that of x, you might do it first.) So why on earth is Tennant demanding a special rule to determine the meaning of (8) when its meaning has already been determined? He is clearly thinking of formal rules of inference as defining the meaning of logical symbols, wherefore there should be a rule for all its contexts of occurrence. (Otherwise it would not be defined in all contexts.) But to demand a special rule to specify the meaning of an expression like (7) in GTS is to misconstrue this entire approach. The meaning of (7) is specified fully by the game rule for the existential quantifier plus the requirement of informational independence. For the purpose, no rules of inference need as much as be mentioned.

Here one can see most clearly the bankruptcy of Tennant's position. It is not merely that his argumentation is circular in that he assumes the correctness of his own position in criticizing GTS. Independence-friendly logic in fact proves conclusively that Tennant's assumptions are not tenable. He could have compared without prejudice received first-order logic and IF first-order logic in general philosophical terms and likewise discussed whether or not IF first-order logic is in different philosophical perspectives really the general basic logic that I suggest that it is. But when Tennant assumes, as he clearly does, that the meanings of logical constants must be characterizable in terms of rules of inference or in terms of inside-out semantical rules, he is not putting forward a possible philosophical opinion or arguing for a philosophical point. He is representing a position which has been conclusively proved to be wrong. Even if IF first-order logic were not the basic logic of quantification, it is in any case a viable and important logical system which as such constitutes a conclusive counter-example to Tennant's tacit but unmistakable assumption that logical constants are fixed by the rules of inference that govern them. Hence Tennant's argumentation in criticizing IF logic is not only circular. Its premises are demonstrably false.

It is especially ridiculous to think that the specific character of IF logic as distinguished from ordinary first-order logic should be spelled out by a set of rules for the independence indicator (slash) /. For it is easily shown (cf. Hintikka 1997) that we can notationally dispense with the slash altogether merely by relaxing the use of parentheses. But parentheses are in effect punctuation marks, and as such have a meaning of an altogether different sort from the meaning of such logical constants as quantifiers. I doubt that even the most dedicated Tennants of this world will have had the courage of their prejudices so as to claim that the meaning of parentheses should be spelled out by giving rules of inference for their use, let alone that there should be separate rules of inference for them for the different contexts in which parentheses can occur.

The irrelevance of rule manipulation for the purpose of understanding the nature of IF logic—and the nature of logic in general—is especially blatant on the semantical level. There the move-by-move game rules characterizing the received ("ordinary") first-order logic remain completely unchanged when we move to IF first-order logic. This even includes the natural "classical" rule for negation. (This invariance should not be surprising, for the rules of semantical games are but codifications of our pretheoretical ideas about truth.) The only new thing is the admissibility of informationally independent quantifiers (and connectives). No new rules and no changes in the old ones are needed.

It might at first sight seem strange for instance that this admission of independence should change the behavior of negation to such a degree that the law of excluded middle fails or that no move-by-move game rules can be given for the classical (contradictory) negation. From a game-theoretical perspective, there nevertheless is nothing weird here.

For instance, the law of excluded middle is seen to be merely the assumption that semantical games are determinate, which games often are not. What is truly strange here is that any one should as much as dream that IF logic could be captured by changing the inference rules for different logical constants. Admittedly, these inference rules are grounded in the corresponding rules for the same constants in semantical games. But these very game rules are not changed when we move from the received first-order logic to IF logic.

Even though the point is only indirectly relevant to proof theory, it is also instructive to note that the meaning of the usual contradictory negation cannot be characterized by any semantical game rules, either, let alone by means of rules of inference. The reason is the one just mentioned, viz. that the classical rules for negation together with the equally classical rules for other connectives and quantifiers yield the stronger dual negation. Hence the only thing one can say (in a metalanguage) of the contradictory negation $\neg$ is that $\neg S$ is true if and only if S is *not* true—with the italicized *not* being interpreted as a contradictory negation. In a sense, contradictory negation is thus not definable by any rules. Moreover, this result is not a mere philosopher's paradox but has specific implications for our formation rules ($\neg$ can occur only sentence-initially) and even for the behavior of negation in natural languages.

Rightly understood, IF logic presents proof theorists also with a technical challenge. In treating so-called ordinary first-order logic, one can formulate quantifier rules only for formula-initial quantifiers. The way suitable natural-deduction type rules operate, one can patiently wait until a quantifier (say $(\exists x/\forall y_1, \forall y_2, \dots)$) occurring inside a complex sentence $S_1 =$

$$S_1[(\exists x/\forall y_1, \forall y_2, \dots)S_2[x]] \tag{9}$$

surfaces so as to be prefixed to an entire formula, whereupon it can be treated by the usual rules. (The sentence S_1 is here assumed to be in negation normal form.)

The trouble with this usual rule of existential instantiation is that in this process of lifting the existentially quantified sentence $(\exists x/\forall y_1, \forall y_2, \dots)S_2[x]$ (or, strictly speaking, one of its substitution-instances) to the surface of the formula, indications of the dependence and independence relations between the inside quantifier and the universal quantifiers in whose syntactical scope it occurs in S_1 can be disturbed or can disappear. Hence in the proof theory of IF first-order logic we need a rule for existential instantiation which can be applied also inside larger sentences. It might authorize us to move from (9) to

$$S_1[S_2[f(z_1, z_2, \dots)]] \tag{10}$$

where f is a new function symbol (a "dummy name" for a function) and $(\forall z_1), (\forall z_2), \dots$ are all the universal quantifiers in the scope of which $(\exists x/\forall y_1, \forall y_2, \dots)$ occurs in S_1, except for $(\forall y_1), (\forall y_2), \dots$.

This rule is perfectly first-order. (It might be called the rule of functional instantiation.) No quantification over second-order entities is involved. The only change as compared with the old rule of existential instantiation (i.e. with the special case in which all the $(\forall y)$'s disappear) is that the dependences of the instantiating individual on others are spelled out.

Rules like this provide an interesting new challenge to proof theorists, including proof theorists of the received first-order logic, in that they will now have to be taken into account in proof-theoretical theorems. For their own sake I hope that they do not disregard it or deny its seriousness. This new rule can be used in ordinary first-order logic, even though there it yields a conservative extension in that everything that can be proved by its means can be proved without it.

It turns out that this new rule (possibly together with its dual rule) is enough to yield, when added to the older rules, a complete *disproof* procedure for IF first-order logic. This illustrates vividly what was said earlier about the absence of any need to change game rules or even suitable earlier rules of inference in moving to the IF first-order logic.

Game-theoretical semantics has many important constructive uses. However, it has also highly important critical repercussions. It has for instance already revealed the philosophical irrelevance of Tarski's celebrated impossibility theorem. It is of course true that one cannot formulate a proof predicate for a first-order language in the same language, but only if "first-order" means received first-order logic. Tarski's theorem does not apply to IF first-order languages. The foundational role of axiomatic set theory may also be among its victims (cf. Hintikka 1998). Likewise, the unavoidable conclusion yielded by an examination of Tennant's claims is that the approach he represents in his own work, not only in his comments on the GTS normally construed, is without any theoretical interest. Playing with rules of inference is not going to result in an analysis of the meaning of logical constants, in truth-definitions or anything with a similar philosophical or foundational significance, no matter whether or not one uses the magical word "game" in trying to do so.

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Department of Philosophy
University of Helsinki, Finland
Boston University, U.S.A.