SCHEDULE

University of Granada

XXI SIUCC
Invited speaker: Jaakko Hintikka (Boston University)
Thursday 24th November – Saturday 26th November

Venue: Camen de la Victoria.

Provisional Programme

Thursday 24th November:

9.00 h: Registration

9.30-11.00   Jaakko Hintikka (Boston University)
             “A logic for the future (from the past)”

11.00-11.30  Break

Session 1:

11.30-12.30  Gabriel Sandu (University of Helsinki)
             “Truth and Probability in Independence-Friendly Logic: Some Recent
             Developments”

12.30-13.30  Andrés Bobenrieth (University of Valparaíso/University of Chile)
             “On Hintikka’s view on negation through IF logic, confronted with
             paraconsistency”

Session 2:

15.30-16.30  José Ferreirós (Universidad de Sevilla)
             “On arbitrary sets in logic and set theory”

16.30-17.30  Ahti-Veikko Pietarinen (Helsinki University)
             “Mathematical and linguistic practices from Peirce to Grice to Hintikka”

17.30-18.00  Break
Friday 25th November:

Session 1:

9.00-10.30  Jaakko Hintikka (Boston University)  
"What should a mathematical or logical theory be like?"

10.30-11.30  José Miguel Sagüillo (Santiago de Compostela University)  
"Epistemic proof and information-theoretic logic."

11.30-12.00  Break

12.00-13.00  Xavier Donato and Jesús Zamora (Santiago de Compostela University / UNED)  
"The empirical testing of structuralist theory-nets through game theoretical semantics"

13.00-14.00  Hans van Ditmarsh (Sevilla University)  
"Playing cards with Hintikka. An introduction to dynamic epistemic logic."

Session 2:

16.00-17.00  Ángel Nepomuceno-Fernández (Sevilla University)  
"The fundamental problem of contemporary epistemology"

17.00-18.00  David Pearce (Universidad Politécnica de Madrid)  
"Objective Belief as a Basis for Minimal Belief"

18.00-18.30  Break

18.30-19.30  Daniel Quesada (Universidad Autónoma de Barcelona)  
"A Bridge Over Troubled Water? Hintikka on Husserl’s Phenomenology"

21.00  Workshop dinner

Saturday 26th November:

Session 1:

9.00-10.30  Jaakko Hintikka (Boston University)  
"Logic as a theory of computation"

10.30-11.30  Tomás Calvo (Universidad Complutense de Madrid)  
"The Verb ‘to be’ (εἰμί) and the Aristotelian Ontology"

11.30-12.00  Break
<table>
<thead>
<tr>
<th>Time</th>
<th>Speaker</th>
<th>Title</th>
</tr>
</thead>
<tbody>
<tr>
<td>12.00-13.00</td>
<td>Alfonso García Suárez (Oviedo University)</td>
<td>“The Metaphysical Status of the Objects of Wittgenstein’s TRACTATUS”</td>
</tr>
<tr>
<td>13.00-14.00</td>
<td>Juan José Acero (Granada University)</td>
<td>“Carnap’s MEANING AND NECESSITY and the Universalist Tradition in Semantics”</td>
</tr>
</tbody>
</table>
ABSTRACTS

JAAKKO HINTIKKA (Boston University)

LECTURE 1: A LOGIC FOR THE FUTURE (FROM THE PAST)

Gottlob Frege is supposed to have formulated the basic part of our logic, the quantification theory aka the received first-order logic. If so, he left the job incomplete. Mathematicians had already before Frege used informally a much richer logic of quantifiers. By following Frege, logicians have for hundred-odd years been using a flawed logic. The richer logic was formulated only in the 1990s under the title IF logic. It requires a semantics different from Tarski's. It can be extended so as to capture the force of the entire higher-order logic at the first–order level.

LECTURE 2: WHAT SHOULD A MATHEMATICAL OR LOGICAL THEORY BE LIKE?

Frege’s mistake caused the paradoxes of set theory and thereby a crisis in the foundations of mathematics. Attempts were made to overcome it by means of the axiomatic method which nevertheless was not correctly understood. So-called axiomatizations of logic and first-order axiom systems of set theory are misuses of the method. Liberation from mistakes facilitates strengthening of set theory, e.g. making available an unrestricted “axiom” of choice. Gödel–style limitations of axiomatization are symptoms of the weakness of the particular logic used, not of logic as such or of the axiomatic method.

LECTURE 3: LOGIC AS A THEORY OF COMPUTATION

The leading idea of “symbolic” logic is to consider formal deductions as computations. But can every computation (e.g. arithmetical one) be considered as a deduction? If the received first-order logic is used, the answer is no; if IF logic is used, yes. This makes logical theory employing IF logic a resource for the theory of computability.

GABRIEL SANDU (University of Helsinki)

TRUTH AND PROBABILITY IN INDEPENDENCE-FRIENDLY LOGIC. SOME RECENT DEVELOPMENTS

Independence-friendly logic (IF logic) has been introduced by Hintikka and Sandu (1989) to allow for patterns of dependent and independent quantifiers that exceed in expressive power of ordinary first-order logic. We survey some of the phenomena which arise in IF logic under its game-theoretical interpretation, in particular signaling and indeterminacy.
ON HINTIKKA’S VIEW ON NEGATION THROUGH IF LOGIC, CONFRONTED WITH PARACONSISTENCY

Introduction

When I read Hintikka and Sandu 2007 I learned that they are proposing to distinguish between two negations: strong or dual negation (¬) and weak or contradictory negation (¬). That took my attention but not because of being a proposal of different types of negation, because there have been so many proposals in that sense, but because contradictory negation was presented as the “weak” one. Having myself worked on paraconsistency, I am used to have some negations that are presented as “weaker” negations (like paraconsistent negations, paracomplete negations, which includes intuitionistic negation, etc.) if they are compared with classical negation, which is normally understood as the negation that established the contradictory relation. The main reason to consider them as weaker is the fact that they do not sustain some of the classical principles, like non-contradiction, \textit{ex contradictiones sequitur quodlibet} (ECSQ), \textit{tertium non datur}, introduction or elimination of double negation and so on. Well, Hintikka and Sandu say that his “dual negation” does not obey \textit{tertium non datur}, yet it is stronger than the contradictory one. And they claim:

“In view of the “classical” character of the game rules for negation in independence-friendly logic, it appears quite as natural to maintain that this logic shows that the law of excluded middle is not part and parcel of the classical conception of logic” (Hintikka and Sandu 2007: p. 27, Spanish translation p. 39)

This is quite shocking, so one of the purposes of this presentation is to explore the proposal made by Hintikka and Sandu within IF logic about negation, and then to study what consequences Hintikka himself drives from that proposal about the nature of negation.

Later, researching on this subject, I came to know that:

“Hintikka, in a series of talks in Brazil in 2008, defended that IF (“independence-friendly”) logic and paraconsistent logic are, in a sense, very similar. Having sketched the proposal of a new paraconsistent system, he maintains that several achievements of IF logic could be reproducible in paraconsistent logic.” (Carnielli 2009: p. 283)

This proposal generated several reactions among paraconsistent logicians in Brazil, which led to the publication of Hintikka’s proposal in a book edited by the logicians from UNICAMP (Hintikka 2010) and also a written replay by Carnielli (2009), where he addresses some of the main logical issues, and especially points out several developments on paraconsistent logic that are quite relevant to Hintikka’s proposals. He also claims that some paraconsistent logics are the best way of expressing the internal logic within Socratic \textit{elenchus}, touching on a subject that is very important for Hintikka’s epistemology, as it is well known (see Hintikka 2007). Thus, a second main purpose of this presentation will be to comment on Hintikka’s proposals, but in a more philosophical vein.

Negations and IF logic

There are several general presentations of Independence-friendly logic [IF logic] and in all of them a very important feature is that:
in this logic, we have to distinguish from each other two different negations. The natural semantics rules define a strong (dual) negation ~ which does not obey the law of excluded middle. Of course we need also the contradictory negation ¬ (Hintikka 2007a).

IF logic is based on a game-theoretical semantics (GTS) which, accordingly to Tulenheimo, was inspired by Wittgenstein’s idea of language-game and introduced by Hintikka in 1968 (cfr. Tulenheimo 2009: p. 6). Hintikka allows us to understand it in basic terms:

"Semantics games admit in fact a simple interpretation. When there is perfect information, such a game (for instance, the game G(S) connected with a sentence S) can be thought of as a kind of game verification and falsification, albeit not in the most literal sense of the expression. From the point of view of the verifier, it is an attempt to find some of the "Witness individuals" that would verify S. The falsifier tries to make this task as hard as possible or even impossible. From this idea, the game rules can be gathered without any difficulty. For instance, the first move is G((∃x)F[x]) is the verifier’s choice of an individual a from the domain. The game is the continued as in G(F[a]). The first move in G((S₁&S₂)) is the falsifier’s choice of S₁ or S₂, say S₀. The game is then continued as in G(S₀)." (Hintikka and Sandu 2007: p. 25. Spanish translation p. 35)

Concerning negation, Hintikka defends that a game rule for negation must be postulated, instead of treating it as "merely a matter of some kind of omnipresent symmetry in propositions" (Hintikka 2002: p. 587). His aim is that this rule will allow us to “capture certain regularities of the semantics of English” (Hintikka 2002: p. 587). The rule is presented as follows:

"The natural game ruler for negation stipules that the two players of a semantic game sometimes called the verifier and the falsifier exchange roles. This is an explicit, well-defined rule. [...] Also this rule implements in a natural way philosophers’ idea of negation as merely exchanging the rules of truth and falsehood." (Hintikka and Sandu 2007: p. 25)

"Independence friendly» refers to allowing quantifiers to be independent of each other, so IF logic, in a way, extends first-order logic language introducing the (/) slash or independence indicator, so in the formula ∀x∀y(∃z/∀y) R (x, y, z), ∃z is syntactically subordinate or dependent to ∀x but not to ∀y (as it would be the case in standard first-order logic), so in this IF logic formula ∃z is marked as «independent» from ∀y (cfr. Tulenheimo 2009: p. 3). IF logic make use of the notion of Skolem functions, which spell out “the dependences of witnesses for existential quantifiers on interpretations of universal quantifiers” (Ibid., see also Hintika 2009: sec. 2).

Hintikka explains that the usual laws of first-order logic holds in IF first-order logic with the exception of tertium non datur, which does not hold in general (cfr. Hintikka 2002:p. 589), yet he adds that this feature does not make IF logic a «nonclassical» logic, rather a «hyperclassical» logic, because "in it the applicability of the classical semantical rules (rules for semantical games) is extended more than before. This includes the rule for negation." (Hintikka 2002: p. 588) In general, this seems as a strange result but Hintikka thinks that it is a quite natural one, and explains the situation as follows:

"A failure of tertium non datur perhaps should not be surprising to a game theoretic. In IF logic, the truth of a sentence S is defined as the existence of a winning strategy in the correlated two-person game G(S) for one of the players, sometimes called the verifier. The falsity of S is accordingly defined as the existence of a winning strategy for his/her/its opponent called the falsifier. The law of excluded middle as applied to S then becomes the thesis that G(S) is determinate. And, as any aficionado of game theory knows, determinacy is an exception rather the rule in game theory in general". (Hintikka 2002: p. 589).
Hintikka concludes that the failure of this principle results from «hyperclassical» assumptions (with one restricted exception\(^5\)) and then present the IF logic negation (\(\neg\)) as a dual negation\(^6\) and defends that «in any language whose logic includes first-order logic and allows informational independence there is inevitably present a negation behaving like \(\neg\).» (Hintikka 2002: p. 590), and takes it to a further conclusion:

\[\text{"Hence in the logic of a natural language, too, the logically fundamental negation is a dual one which does not obey the law of excluded middle. Of course, its presence can be tacit or, so to speak, a virtual one."}\] (Ibid. p. 590; cfr. Hintikka 2009: sec. 5)

Hintikka himself states that this conclusion seems “blatantly paradoxical, not to say absurd” (Hintikka 2002: p. 590) and the argument that he gives is as follows: “For the negation that is used in natural languages like English is obviously intended to be the contradictory negation and to be «classical» in the sense of obeying the law of excluded middle.” (Hintikka 2009: p 590) I will return to this argument later, but let me finish presenting the two negations. Hintikka then proposes to add a contradictory negation (\(\neg\)) to an IF first-order logic “stipulation that \(\neg\) [sic.] is true if \(S\) is not true” (ibid.), and then he explicitly states:

\[\text{“this is all that can be said of the semantics of } \neg \text{ [...] there are no games rules for } \neg \text{ and hence no such games as } G(\neg S). \text{ The reason is that the games rules must of course be the «classical» ones that we already have in ordinary first-order logic, but these rules lead to the dual negation – rather than to the contradictory negation } \neg. \text{ This negation } \neg \text{ does not govern any moves in a game. Its meaning does not refer to what happens during a semantical game. The meaning of } \neg S \text{ is parasitic on the games that involve other logical constants, and all it can serve to express is that the verifier does not have a winning strategy in the game } G(S). \text{ In this definition, the designated } \neg \text{ must be understood as being a contradictory one. Here contradictory negation in an object language can only defined in a metalanguage that already possesses contradictory negation. On the formal level, this is reflected by the fact that strictly speaking the contradictory negation can only have a meaning when it occurs prefixed to an entire sentence. It does not make sense inside a quantificational sentence. It cannot be prefixed to an open formula.”}\] (Hintikka 2002: p. 590f.)

For me it is quite difficult to accept that «the negation used in natural languages» could have all these restrictions, and I will comment on this later on. There is another quotation that is very helpful: “For natural-language negation is obviously in the first place the contradictory one («not true») rather than the dual one («definitively false»).” (Hintikka 2002: p. 594). It gives us grounds for further discussions and also it gives us a reading of each of these negations. Actually, some pages before that, Hintikka has asserted the presence, at least implicit, of two different notions of negation in natural languages (cfr. Hintikka 2002: p. 591). This allows him to make the following comment:

\[\text{“For one thing, the distinction between the contradictory negation } \neg \text{ and the strong (dual) negation } \neg \text{ can be thought of as a rational reconstruction on the time-honored (or perhaps time-}
\text{abused) contrast between contradictories and contraries. I do not claim that the match is perfect.}
\text{The numerous dead logicians and rethoricians who have proposed or defended the contrast}
\text{might very well have had a multiple of different ideas in the mind. What is nevertheless interesting}
\text{is that the distinction between } \neg \text{ and } & \text{ is quite general. It is present on the most fundamental level}
\text{of language, viz. in first-order logic without any intensional notions.”}\] (Hintikka 2002: p. 591)

That paper goes on studying different aspect of natural language, like anaphora, and then gets to negative quantifiers (no, nobody, nothing…), in virtue of that Hintikka says that “in natural languages we spontaneously avoid having explicit negation to occur within the scope of a quantifier.” (Hintikka 2002:p. 595) and then introduces a new symbol (\(N\)) for the negative universal quantifier, which operate by a “kind

\(^5\) [...] “the only possible exception of the admission of , as it were, \textit{ad hoc} informational dependence. And this admission is not a «nonclassical» idea, but a necessary correction to «classical» (i.e. received) first-order logic needed to enable it to fulfill its job description.” (Hintikka 2002: p. 589)

\(^6\) Tulehmo (2009: p. 19) shows that it is referred to as «strong negation», «dual negation» or «game-theoretical negation».

\(^7\) I guess it should say: \(\neg S\)
of substitutional interpretation”, and discusses that point further (Hintikka 2002: p. 595). Toward the end of this text, there is a comment that can be quite relevant for my purposes:

“one virtue of the analysis presented in this paper is that it predicts its own exceptions. In many particular examples, perhaps in most examples linguist are likely to contemplate, the difference between ~ and ¬ does not make any difference. Hence competent speakers’ Sprachfühl may be tolerant enough in such cases to allow one of them the same privileges as to the other.” (Hintikka 2002: p. 597).

This is linked but is not the same as what is stated in Hintikka’s other important paper on negation:

“When no slashes are present, the difference between ~ and ¬ should not make any difference. Indeed, the received first-order logic can be apparently be identified with the slash-free fragment of IF first order logic. […] the distinction between ~ and ¬ makes no difference for slash-free formulas only if is assumed that atomic sentences obey the law of excluded middle. If they do not, there is a difference after all. Among other things, the same sentences are no longer logically true.” (Hintikka 2009: sec. 5)

That paper explores the relations of the two negations with several important issues: conditionals, sorites paradox, fuzzy logic, liar paradox (and strong liar paradox) and the possibility of certain extension of IF logic, with increased expressive richness “that might have a claim to be our genuine Sprachlogik.” (Hintikka 2009: sec. 5).

Comments on Hintikka’s two negations proposal

So far we have followed very close some main features of Hintikka’s (and Sandu’s) proposal on negation. The aim has been just select some of the aspect that constitutes the basis of this proposal, allowing Hintikka to speak for himself; that is why I have made such an extended use of quotations. Now I want to sketch some of the main aspect in which I am interested in considering more deeply. All of these, having in mind that in his “Intellectual Autobiography” Hintikka (2006) highlights among the consequences of the novel insights made possible by (extended) IF first-order logic this one: “insights into the nature of negation in logic and in natural languages” (Tulenheimo 2009: p. 41).9

1) Relations with other proposals of several negations, particularly with von Wright’s. In 1959 von Wright presented a system of propositional logic with two negations. Horn’s summary is useful:

“In this system, STRONG negation (¬p) is an affirmation as well as a denial. As with the predicate term negation of Aristotle, LEM [Law of Excluded Middle] does not apply to a proposition and its contrarily opposed strong negation. Like the corresponding term logic predications S is P and S is not-P, p and ¬p may both be false, namely, when the subject doesn’t exist or when it exist but the predicate cannot be naturally applied to it. WEAK negation (–p), on the other hand, is a contradictory operator, corresponding to predicate denial, amounting to the proposition that it is not true that p.” (Horn 1989: p. 132).

Later on, in 1986 von Wright presented a logical system using a modal operator T for «is true that», yet distinguishes between an external negation: ¬Tp «it is not true that», and an internal negation T¬p «it is true that no p», the fist one would correspond to not true and the second to falsehood, being stronger the last one. In that system holds the principle of non-contradiction in its weak formulation ¬(Tp&T¬p), but not the strong one: T¬(p&¬p) (cfr. von Wright 1986: p. 6ff.).

---

8 The others are: “reconstruction of normal mathematical reasoning on the first-order level, a novel perspective on the notion of truth in axiomatic set theory, […] the formulation of a self-applied truth-predicate, and the conceptual problem of mutually dependent variables. A related topic of general interest is the phenomenon of informational independence in natural languages.” (Tulenheimo 2009: p. 41)

Apart from interesting historical relations that might exist between these two proposals, one could ask: if Hintikka states, as von Wright first did, that for «strong» negation tertium non datur does not hold, why he does not follow his teacher path and rejects in some cases the principle of non-contradiction?

2) Comparison with other proposals, where negations are applied to types of actions, like in Sainsbury 2003 paper, where he portrays «option negation». “One difference between option negation and classical negation is that the former can apply to things other than statements (e.g. to types of actions such as choices of what to say). Another difference is that NOT may, […], be somewhat independent of truth.” (Sainsbury 2003: p. 88).

3) Comparison with paraconsistent and paracomplete negations. Many paraconsistent systems, particularly the ones that follow da Costas original ideas, allow that both p and not-p can be true; their dual systems are called paracomplete and they allow that neither p nor not-p are true. And they all have the possibility of expressing classical negation\(^\text{10}\). So together these systems consider the possibility of true-value gluts and truth-value gaps. Carnielli has highlighted this in relation to Hintikka’s two negations (Carnielli 2009). Yet, Hintikka asks “But what on earth can it mean for a proposition to be true and false?” (Hintikka 2010 apud Carnielli 2009: p. 289), and I would say that many people working on paraconsistency would have the same question. Paraconsistency is not to be confused with one special case of it, dialetheism, where it is claimed, mainly by Graham Priest, that an statement (like the liar sentence), can be both true and false.

Hintikka seems to be in the aim of building bridges between IF logic and paraconsistent logic, especially through his «paratyconsistent logic», related to which he points out that:

> “Especially basic are questions concerning negation. What is the natural treatment of negation in paratyconsistent logic? Should the negation that is in fact used in paraconsistent logics be interpreted as the contradictory negation or as some kind of stronger (dual) negation? Some of the problems listed above can only be solved by introducing a second negation into one’s logic. How can this be done in the context of paraconsistent logic?” (Hintikka 2010 apud Carnielli 2009: p. 289)

Carnielli says that this challenge can be met using the «possible-translations semantics» for paraconsistent systems that he and his student Marcos have developed (cfr. Carnielli 2009: p. 286). Yet the general issue would be how far we can go in this aim of bringing together IF logic and paraconsistent logic.

4) Let’s consider the game-sematics that sustains IF logic. Carnielli has conceived an understanding for games that would interpret paraconsistent logics\(^\text{11}\). Yet, let me ask a simple question: if the lack of determinacy in many games is the reason for not accepting tertium non datur, could there be games where some G(S) are, as it were, «over-determinate»?

This could mean, I guess, that there is a winning strategy for the verifier, but also for the falsifier. Going back to the idea that “from the point of view of the verifier, it [the verification and falsification game] is an

\(^\text{10}\) For a general study on different paraconsistent (and paracomplete) systems see Bobenrieth 1996. There are several text where paraconsistente logic is presented in its many aspects, among them Priest 2002 and da Costa / Krause / Bueno 2007 are main reference texts.

\(^\text{11}\) “But how could contradictions be dealt with by means of strategic rules? As much as games that interpret IF logics emerge from the intuition that the truth of a sentence S means the existence of a winning strategy for the “verifier” in a game G(S) related to S, the games that would interpret paraconsistent logics would amount to the existence of a non-losing strategy for the “falsifier”\(^\text{*}\), as in the famous Tic-Tac-Toe game: any of both players cannot lose if playing strategically, but cannot win either if the adversary is also playing strategically. Of course, every game ends in a tie if both players are playing strategically. So, to determine (from the game-theoretical viewpoint) the truth of S in a paraconsistent logic (in particular, the “paratyconsistent”) is to determine whether such a non-losing strategy exists or not.” (Carnielli 2009: p. 290)

\(*\) At this point there is a footnote that says: “There are some evidences that Hintikka would agree to this suggestion, which I proposed during his seminars at UNICAMP in 2008.” (Carnielli 2009: p. 297)
attempt to find some of the “Witness individuals” that would verify S. The falsifier tries to make this task as hard as possible or even impossible.” (Hintikka and Sandu 2007: p. 25. Spanish translation p. 35). I am thinking in sentences like “all mammals are viviparous”, utter at the time when platypuses were discovered, together with sentences like “all mammals have hair, big brain, warm blood”, but also with “no mammals has cloaca”. The immediate answer would be: «yea, but now we know that platypus are mammals although there are oviparous and have cloaca.» But the point is that at that moment we did not know that, and although later on we have changed that language game, the question remains as what was the winning strategy at that time. At the present time consider subjects like: Was democratic the approval of the “Patriotic Act” and the establishment of Guantanamo bay prison? Is it legal to keep it working? For me the answers to these questions are quite clear, yet I cannot deny that there are many intelligent and well intentioned people that have a complete different way of answering them; furthermore, they still seem to be the majority in the U.S.A. (otherwise --I assume-- the situation would have being changed), but yet: does this approval by the majority of the (north) Americans make their continuation a democratic decision? There are interesting examples in science, just to mention one: the inclusion, or not, as live-organism of several micro-organism.

At this point it is relevant to remember that the first paraconsistent logic was Jaskowski’s discursive logic.

5) Let me finish with two further questions: First, What are the grounds for Hintikka’s believe that there are two (or perhaps one) negations in natural languages? That surprises me, particularly considering that many negations can be established in formal systems and also that Linguistics has described several different negations in natural languages. Second, in relation to Hintikka’s restriction for contradictory negation as being external to any quantifier, consider sentences like: «All animal that are not vertebrate are invertebrates». Here the negative prefix «in-» ought to be internal within any quantified statement. Vertebrate are invertebrates are terms that jointly exhaust a universe of discourse and are mutually exclusive, using Brody’s terminology (cfr. Brody 1967), so they establish a contradictory opposition. So, is IF logic incapable of formalizing statements like this by means of any of its negations?

In my presentation I will try to address these issues (or some of them, depending on the availability of time). I will finish stressing that Hintikka’s conception and considerations on negation contains key contributions to a very long lasting logical, linguistic and philosophical debate.

REFERENCES:


ON ARBITRARY SETS IN LOGIC AND SET THEORY

It is commonplace in philosophical logic (as well as in formal approaches to epistemology, philosophy of science, etc.) to employ systems based on semantics that presuppose full powersets more or less explicitly. In the simple case of SOL, this is the so-called "standard" or "full" second-order logic. The purpose of this talk is to underscore that a strong commitment to arbitrary sets is involved in such systems ("standard" SOL in particular), and to recommend a more restrained understanding of logic. In order to do so, I shall analyze the motives behind arbitrary sets, emphasizing that they are mathematical (not logical) reasons, typically entangled with the notion of the real numbers. Reliance on the idea of arbitrary set (aka random set) is thus proposed as a clear criterion of non-logicality.

MATHEMATICAL AND LINGUISTIC PRACTICES FROM PEIRCE TO GRICE TO HINTIKKA

Peirce’s logic is fundamentally grounded on the ideas of game-theoretic semantics, where the players are all “feigned in our make-believe”. His pragmaticism is a theory of strategic aspects of meaning. Grice builds his theory on Peircean maxims (such as the principle of economy), and even more significantly, on the cooperation principle. But cooperation is the key activity of the model-building game. Cooperative model-building resorts to the same theoretical constructs of make-believe agents as the strictly competitive semantic activities do. The two kinds of games, the semantic and the model-construction games, are two sides of the same conceptual coin: from the winning strategies of the model-construction games we can construe a model set that guarantees the existence of witness individuals and witness predicates in the semantic game correlated with a sentence $A$; and conversely, if there exists a winning strategy in the semantic game correlated with $A$, we can construe a model set from which we get a model for $A$ by playing through all the positions allowed by the model set in question. In diagrammatic logic, the sheet of assertion represents “everything that is well understood to be taken for granted between the two parties”, and Peirce insists that “the two must come to an agreement of convention”. Such activities constrain the classes of models (e.g. to attain descriptive completeness). Such constrains are internal to the processes of construing the models. They cannot be axiomatised, since we cannot quantify outside the structures of the models. Consequently, general principles governing mathematical practices relate to model-building activities. Hintikka has proposed extremalism in the philosophy of mathematics, and we see this as related both to Peirce’s and Grice’s insights. The general theory of the method of discovery was named by Peirce as “methodeutic” or “speculative rhetoric”. In relation to the pragmatics of mathematical discovery in Hintikka’s sense we could call it “pragmatic logicism”.

Key words: GTS, pragmaticism, strategic meaning, model building, cooperative principle, mathematical discovery.

Extended Abstract

Peirce’s pragmatic logic is fundamentally grounded on the key conceptualisations of game-theoretic semantics (GTS) (Pietarinen 2006). According to Peirce, the players in the logical games of any stripe are all “feigned in our make-believe” (Peirce, MS 280). His pragmaticism, which is his logical (semantic and pragmatic) theory of meaning of intellectual concepts, is thus at the same time a general theory of strategic aspects of 2 meaning (Pietarinen 2010). The need for the theory of strategic meaning, in addition to that of abstract (material) meaning, has been pointed out by Hintikka (1987).

Influenced by Peirce (see Pietarinen 2004), Grice built his theory of meaning on maxims such as the principle of economy (Grice 1989). Even more significantly, Grice’s theory is grounded on the cooperation principle. But cooperation is the key property of the logical activities in the model-building games which Peirce also had and assumed. Cooperative model-building resorts to the same theoretical constructs of make-believe agents as the strictly competitive semantic activities do. The two kinds of
games, the semantic and the model-construction games, are in fact two sides of the same conceptual coin: from the winning strategies of the model-construction games we can construe a model set (Hintikka set) that guarantees the existence of witness individuals and witness predicates in the semantic game correlated with a sentence \( A \); and conversely, if there exists a winning strategy in the semantic game correlated with \( A \), we can construe a model set from which we get a model for \( A \) by playing through all the positions allowed by the model set in question.

In the diagrammatic logic developed by Peirce in the 1890s, the sheet of assertion represents “everything that is well understood to be taken for granted between the two parties”. He goes on to insist that “the two must come to an agreement of convention” (Peirce, MS 280) about what constitutes the universe of discourse, while the universe itself may be indefinitely extendible. Such activities constrain the possible classes of models (e.g. in order to attain descriptive completeness). Importantly, such constrains are internal to the processes of construing the models. Since we cannot in any reasonable sense quantify outside the structures of the models, such constraints cannot be axiomatised. They do not pertain to what can be captured by our language. Languagebound quantification does not reach beyond the classes of structures of models. Consequently, general principles governing mathematical practices are related to modelbuilding activities and to the qualitative features of mathematical discovery.

Hintikka (1989) has proposed an extremalist programme in the philosophy of mathematics, which takes the key principles governing model constructions to be the principles of parsimony and plenitude (such as minimality / Archimedean axiom and maximality / Hilbert-completeness). We see this project as related both to Peirce’s and Grice’s insights. Mathematical reasoning is not limited to deductive mechanisms. Grice’s theory concerns non-Bayesian abductive reasoning about speakers’ intentions and happens in the contexts of the discovery of intentions, not in the contexts of their justification. The general theory of the method of discovery was named by Peirce “methodetic” or “speculative rhetoric”, and abduction plays the key part in it. In relation to the pragmatics of mathematical discovery in Hintikka’s sense, we could call it “pragmatic logicism”, as Hintikka takes the interesting questions in the philosophy of mathematics to be those concerning the expressivity and meaning of mathematical propositions couched in certain suitable logical conceptualisations. According to such a general theory of the method of discovery, the task in the foundations of mathematics is to pin down the processes by which a mathematician is able to formulate his or her nonlogical axiomatisations.

Our observations concerning strategic aspects of meaning therefore suggest that linguistic and mathematical theorising are linked through logic, and that there is a hitherto undisclosed path from Peirce through Grice to Hintikka marking out those links.

**References**


MODELLING LINGUISTIC CONTEXT WITH HINTIKKA SETS

A general theory of meaning must offer interpretation models of linguistic expressions that go further than the frontiers of traditional semantics. Natural language meaning depends, in a very important manner, on syntactic aspects of discourse (functional aspects as much as relational ones), and on pragmatic aspects, intervening on the correct interpretation of linguistic utterances as well. Hintikka sets can be viewed as partial descriptions of a world and used to construct formal modal models of interpretation of discourse expressions into a concrete context or situation. Therefore linguistic contexts are defined by means of Hintikka sets as a system of frames which allows the hearer/speaker to assign a reference, for instance, to any anaphoric expression in a general way, picking up the best referential candidate in each fragment of discourse to get its interpretation.

EPISTEMIC PROOF AND INFORMATION-THEORETIC LOGIC

John Myhill (1960) endorsed what he called an absolute sense of proof which was neither syntactical nor semantical but epistemic. This epistemic sense of proof reflects on the objectivity of mathematical reality and the dynamic human enterprise of obtaining mathematical knowledge. In this paper, I discuss a sense of information due to John Corcoran (1998), which is neither syntactical nor semantical but genuinely intensional; a sense of information which is suitable for the proper understanding of the objective relation of logical consequence underlying the envisioned epistemic notion of proof. This purely intensional sense of information has been claimed to be implicitly defined in the Corcoran’s postulates of information-theoretic logic. From this perspective, the so-called “paradox of inference” (Cohen and Nagel 1962/93), or the so-called “scandal of deductive logic” (Hintikka 1970), vanishes; it becomes re-classified as an ontic-epistemic fallacy.

The info-theoretic concept of logical consequence is an ontic relation between a conclusion $c$ and a set of premises $P$. Its characteristic no added information postulate states that in order for an premise-conclusion argument to be valid it is necessary and sufficient for the information contained in the conclusion $c$ to be already contained in the premise-set $P$. In other words, an argument is valid if and only if the premise-set $P$ contains the information in the conclusion $c$. Deductive inference on the other hand, is a characteristic epistemic activity of information processing which, of course, does not add any information beyond the information contained in the premises. What deductive inference adds is knowledge that the information in the conclusion is [was] already contained in the premises. Thus, it is shown—in agreement with Hintikka—that the epistemic nature of deductive inference is neither exhausted nor totally explained by the unavoidable mental or psychological process of obtaining it.

Notwithstanding, Hintikka’s distinction between depth and surface “information” appears misleading from the information-theoretic perspective since it suggests there are two kinds of information where there is only one. His construction allegedly provides an explicatum in logical theory for which there is no problematic explicandum in mathematical practice. In addition, Sequoiah-Grayson, S. 2008 has shown that Hintikka’s construction misses its intended target. The detailed discussion arising will be sustained along the following lines:

-1 For Hintikka, logic is first-order and information is induced from constituents- construction. Furthermore, his semantic device is immanent in the sense of being severely restricted by the means of expression of the language under consideration, usually a fragment of first-order. Mathematical practice shows clear evidence that first-order logic is insufficient to characterize fundamental mathematical concepts. This appears to be a severe limitation for the Hintikka’s project.

-2 Hintikka’s discussion assumes the well-known fact that a deductive inference can be re-written as a tautological conditional whose antecedent is the conjunction of the premises and whose consequent is the conclusion. However, this should not mask the fact that deductions and tautologies do not share all their properties. In the present account deduction is information processing; a human activity, whereas a
tautology is a proposition of a certain kind. Inattention to this fact suggests a kind of process-product fallacy in need of correction.

3 Hintikka’s disproof procedure involved in surveying constituents to find inconsistency is not the deductive process of comparing propositional information content to determine containment relations. Information up-taking from a semantic construction is not intra-propositional information processing. The disproof procedure depends essentially on truth-values of the propositions involved. In this sense it is extrinsic. On the contrary, determining information-containment is independent of truth-values and hence intrinsic.

4 Hintikka’s disproof procedure appears more adequate to find consistency of true premises and false conclusion, namely, as a methodology to show invalidity as it is exemplified by the classical independence proofs of geometry and set-theory. On the other hand, establishing non-containment relations of logical independence appears prima facie less affordable when considering complex arguments in applied methodology.

5 Information-theoretic logic shows that the experiential meaning of a given proposition is not its information content. Two logically equivalent propositions share the same information content but they do not share logical form. Experiential meaning is form-dependent whereas information content is not. Likewise, compositionality applies to meaning but not to information. Understanding the experiential import of a given proposition is a pre-condition for using information-based deductive methodology.

References


THE EMPIRICAL TESTING OF STRUCTURALIST THEORY-NETS THROUGH GAME THEORETICAL SEMANTICS

In this paper, Jaakko Hintikka’s game theoretical semantics will be put in use to illuminate the process of theory testing. According to this approach, a semantic game for a proposition is a game played between two players (V, the verifier, and F, the falsifier) who try to find out, respectively, an example or a counterexample of that proposition. The proposition will be true if V has a winning strategy for the game, i.e., a way of playing the game which assures her victory independently of the moves made by F; and conversely, it will be false if F has a winning strategy. The structure of the game is inspired in that of semantic tableaux, and consists of the following rules:

a) if P is an atomic proposition, V wins if P is true and F wins if it is false;

b) if P has the form Q ∨ R, V chooses either Q or R, and the game continues with respect to that proposition;

c) if P has the form Q & R, F chooses either Q or R, and the game continues with respect to that proposition;

d) if P has the form ¬Q, the game continues with respect to Q, but changing the roles of V and F;

e) if P has the form ∀xQx, V chooses some object a and the game is continued with respect to the proposition Qa;

f) if P has the form ∃xQx, F chooses some object a and the game is continued with respect to the proposition Qa.

Game theoretical semantics was mainly developed to analyse several aspects of natural languages which were difficult to explain with other formal semantic tools; in fact, the ‘players’ are only abstract constructions which do not represent actual beings, and, as far as we know, the theory has not been systematically used as a means to analyse scientific method, in spite of Hintikka’s other works on this subject. Nevertheless, Hintikka’s idea of connecting the semantic analysis of propositions with the activity of searching for certain objects allows to think that it might be possible to reach some relevant conclusions about the testability of scientific theories through a game theoretical analysis of them. In particular, our proposal is simply to deploy the game associated to the ‘empirical claim’ of a theory (its ‘Ramsey-Sneed sentence’, in structuralist terms), and look for interesting consequences thereof.

There are several versions of the ‘empirical claim’ notion in the structuralist literature, and we will use a particularly simple one, which is apt to present the basic ideas of the game-theoretical semantic approach. Further studies may be devoted to analyse the games associated to more complex versions of ‘Ramsey-Sneed sentences’, as well as the empirical claims of specific theories. In what follows, ‘I’ will represent the set of intended applications of a theory (which is a subset of Mpp, the set of partial potential models of the theory), ‘Mpp’ will be the set of its potential models, and ‘M’ that of its actual models; ‘C’ (⊆ Po(Mpp)) will be the set of those subsets of Mpp which obey the theory's constraints; lastly, F is the set of all possible functions from Mpp into M; each one of these functions induces a corresponding function from Po(Mpp) into Po(Mpp). Now we can describe the empirical claim of theory T as the proposition:

(8) asserts that there is a way of completing the intended applications into structures which obey both the laws and the constraints of the theory. Figure 1 depicts the ‘normal form’ game associated to this assertion; numbered cells represent decisions by the ‘verifier’ or the ‘falsifier’; the bottom cells are the

---

13 Methodological writings by Hintikka are more inspired by his ‘interrogative model’ of scientific research. See, e.g., the papers collected in Hintikka (1999).
14 I.e., if X is a subset of Mpp, then f(X) is \( \{y \in M_p : \exists z \in X, y = f(z)\} \).
possible endings of the game, which is won by $V$ if the chosen cell is true, and by $F$ if it is false. Figure 2 shows the ‘strategic form’ of the game; columns represent the strategies available to $V$ (each possible function $f$), and rows the strategies of $F$ (which are pairs of the form ‘left, a’ or ‘right, A’, where $a$ is an element of $I$ and $A$ a subset of $I$). Each cell is to be replaced by the truth value of the sentence included into it; if there is at least a column all whose cells are true, then proposition (8) will be true, and if there is at least a row all whose cells are false, then (8) will be false. In the case of classical logic at least, it is warranted that one of these possibilities must take place.

In the case of classical logic at least, it is warranted that one of these possibilities must take place.

Figure 1.

<table>
<thead>
<tr>
<th>F</th>
<th>V</th>
<th>$g_1$</th>
<th>$g_2$</th>
<th>$g_3$</th>
<th>$g_4$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Left, $a_1$</td>
<td>$g_1(a_1) \in M$</td>
<td>$g_2(a_1) \in M$</td>
<td>$g_3(a_1) \in M$</td>
<td>$g_4(a_1) \in M$</td>
<td></td>
</tr>
<tr>
<td>Left, $a_i$</td>
<td>$g_1(a_i) \in M$</td>
<td>$g_2(a_i) \in M$</td>
<td>$g_3(a_i) \in M$</td>
<td>$g_4(a_i) \in M$</td>
<td></td>
</tr>
<tr>
<td>Right, $A_1$</td>
<td>$g_1(A_1) \in C$</td>
<td>$g_2(A_1) \in C$</td>
<td>$g_3(A_1) \in C$</td>
<td>$g_4(A_1) \in C$</td>
<td></td>
</tr>
<tr>
<td>Right, $A_i$</td>
<td>$g_1(A_i) \in C$</td>
<td>$g_2(A_i) \in C$</td>
<td>$g_3(A_i) \in C$</td>
<td>$g_4(A_i) \in C$</td>
<td></td>
</tr>
</tbody>
</table>

Figure 2.
Figures 1 and 2 have an obvious methodological reading. The falsification of a theory amounts to finding out one or more intended applications which do not fit, either the theory’s laws, or its constraints, whatever the values the ‘verifier’ assigns (through the $g$-functions) to their applications’ theoretical magnitudes. Its verification amounts to finding out a way of completing all theoretical applications, such that no counterexample can be actually presented. From this point of view, scientific method consists basically in a double set of strategies: \textit{theoretisation} looks for embedding the known empirical structures into bigger, more complex ones, such that certain laws are met; \textit{observation and experimentation} look for finding out empirical structures which might fail to obey those laws.\textsuperscript{15} This vision is clearly a Popperian one, although, contrarily to the falsificationist slogan, figure 2 allows to see that in general, \textit{scientific theories, besides being unverifiable, can be unfalsifiable as well.} The reason is that a theory is verifiable (alternatively, falsifiable) if and only if its game’s strategic form has a finite number of rows (columns), i. e., if its columns (rows) are finite in length; for only in this case are human beings able of confirming that all the cells of a certain column (row) are true (false). So, if the set $I$ is finite, the theory is verifiable, and if the set $F$ is finite, the theory is falsifiable. The problem is, of course, that both sets are usually infinite. For example, a theory’s intended applications include not only \textit{actual} empirical structures, but also \textit{physically possible} ones (this is specially clear when we think of the a’s as \textit{individual} realisations of experiments that the falsifier \textit{might} do, rather than as \textit{types} of experiments). The transfinite nature of $F$ is still clearer, since it comprises all the possible functions from $M_{pp}$ into $M_{p}$, which are even non-denumerable sets.

Although game theoretical semantics has the virtue of explaining semantic categories (e. g., truth) through pragmatic ones (e. g., the activities of searching and finding), a game for the sentence associated to the claim of a theory is so immense that human beings can not actually play it at all. Nevertheless, it is challenging to look at the competitive activities real scientists perform in their daily work as something essentially related to the game depicted in the previous section. Our suggestion is that we can understand the rules of scientific method as a set of \textit{mutual constraints} that players $V$ and $F$ put to each other in order to have a chance of playing that game with a limited amount of time and resources.\textsuperscript{16} According to this vision, each scientist adopts one of those roles (not necessarily the same every time) only under the proviso that the set of strategies available to each participant in the game has been dramatically reduced. The structuralist conception of theories provides again some insights about how this reduction can be performed; in particular, we will make use of the ideas that scientific theories are organised through a net of \textit{special laws}, and that the set of intended applications is itself organised into \textit{types of applications}. This ‘human-faced’ description of the semantic game for a scientific theory can show more transparently than usual structuralist expositions some methodological aspects of the construction and testing of theory nets. A semantic game for a scientific theory, whose strategies have been reduced in order to give each player a realistic chance of winning, can be called a \textit{‘research game’}.

Before analysing such a game with reduced strategies, we have to introduce some new terminology. Let $L_{1}, L_{2}, ... , L_{k}, ...$ be a series of subsets of $Po(M)^{\rightarrow}C$, i. e., collections of sets of models of the theory satisfying its global constraint, and which actually obey some additional condition (a special law or constraint, or both), and let us assume that this condition can be expressed through a finite formula. $L$ will be the set of all these $L_i$’s. On the other hand, let $I_{1}, I_{2}, ... , I_{j}, ...$, a series of subsets of $I$, i. e., types of intended applications, whose empirical identification is assumed not to be questioned by the players of the game. $I$ will be the set of all these $I_i$’s. It is important to take into account that neither the $L_i$’s nor the $I_i$’s are necessarily disjoint. Let $H$ be the set of all possible functions from $I$ into $L$, i. e., the set of all possible ‘theory-nets’ which may be constructed for the set $I$ using some of the laws contained into $L$. Lastly, if $x$ and $y$ are structures, let ‘$Eyx$’ represent that $y$ is an extension of $x$. We can then reconstruct the empirical claim of a scientific theory as follows:

\begin{equation}
\forall h \in H \quad \forall I \in I \quad \forall x \in I \quad \exists y \text{ such that } Eyx \tag{9}
\end{equation}

(9) asserts that there is a way of assigning a special law or constraint ($h(I)$) to each type of application ($I$), such that for each one of its individual application’s ($x$), the result of applying to it the corresponding special law ends with an actual model of the theory. Stated differently, to each type of

\textsuperscript{15} Obviously, the utility of theoretisation, observation and experimentation is not limited to their roles in these semantic games: it is important to take into account also what the \textit{point of the game of science} is. Perhaps it is to ‘discover the underlying truth’, or perhaps it is to help us to ‘control our environment’. In this paper, nevertheless, I shall be agnostic about this ultimate question.

\textsuperscript{16} See Zamora Bonilla (2002).
application can be successfully associated a theoretical formula (i.e., a formula which logically entails the theory’s fundamental laws and constraints), where ‘successfully’ means that this formula is logically consistent with the data included into any particular application of that type. The normal form of the ‘research game’ associated to (9) is depicted in figure 3.

For the strategic form, account must be taken that, if theoretical laws are well defined, then the last movement is no ‘choice’ at all, for the only thing \( V \) has to do is to apply the formula \( g(I^*) \) to the data contained in the application \( a \); if the formula is not consistent with those data, the proposition of the last cell will be false due to the non existence of \( b \), whereas if it is consistent with them, \( b \) will be uniquely determined by that formula. That is, the point of special laws is to allow to construct ‘actual models’ out of ‘empirical applications’ in a non arbitrary fashion.\(^\text{17}\) Hence, the strategies of \( V \) are the elements of \( H \) (she has to choose a theoretical law or constraint for each type of application), while the strategies of \( F \) consist of pairs of the form ‘\( I_i, a_j \)’, where \( a_j \) is an element of \( I_j \) (she has to choose a type of application first, and later a particular application thereof). The ‘real’ game actually ends after the third movement (at the cell marked with dotted lines), for usually the last cell is fully determined by the previous choices of the players, as I have argued. The strategic form of the game is, hence, as shown in figure 4. Each cell ‘\( \exists y \in g(I) \ Eyamj' \) amounts to the assertion that a theoretical model (\( y \)) can be constructed out of the chosen empirical system (\( a_mj \)) with the help of the special laws determined by the combined choice of \( g \) and \( I_j \) (i.e., by \( g(I^*) \)).

\(^{17}\) In some cases, nevertheless, the special laws do not determine uniquely an extension of the intended application (several theoretical models can be possible extensions of \( a \), all of them consistent with the special laws which correspond to \( g(I^*) \)), and in these cases \( V \) has still certainly a choice. However, I will restrict my discussion to the case where \( g(I^*) \) determines a unique theoretical extension.
With respect to the verifiability and falsifiability of a scientific theory, it is clear from the last figure that the theory will be falsifiable (i.e., a row of false statements can be found) if and only if there is only a limited number of functions $g_i$'s, and this occurs if and only if both the sets $I$ and $L$ are finite. On the other hand, the theory will be verifiable (i.e., a column of true statements can be found) if and only if both the set $I$ and all of its elements (which are sets of applications) are finite; the first one of these two last conditions is more reasonable than the second: there can be a limited number of types of applications, but, as we saw in the case of (8), each type includes an indefinite, probably non-denumerable amount of concrete systems. So, for scientific research being carried out as a game in which each player has a reasonable chance of winning, the following three conditions must obtain: a) researchers admit to consider only a limited number of possible theoretical laws; b) they also accept to consider only a limited number of possible types of empirical systems, and c) the applicability of those laws to these empirical systems can be decided. From a contractarian point of view, this can be seen as the result of the following 'negotiation': the falsifiers might make any theory unverifiable just by insisting that they have to examine all the theory's empirical applications, and the verifiers might make it unfalsifiable just by leaving open the set of special laws they can employ; besides this, both the falsifiers and the verifiers might make the theory both unverifiable and unfalsifiable by insisting in applying the theory to an open set of types of empirical situations; hence, the theory becomes both verifiable and falsifiable just by the mutual agreement of not using these 'defensive' strategies. In a nutshell, $V$ accepts that the theory can in principle be falsified in exchange of $F$'s acceptance that it can in principle be verified. So, in contrast to Popper's thesis that theories are unverifiable by their logical form and falsifiable by convention (i.e., by the conventional decision of accepting a 'basic statement'), our approach suggests that theories are both unverifiable and unfalsifiable by their logical form (as it is clear from figure 2), but can become verifiable and falsifiable by agreement.

Besides this reasoning, it can also be argued that the outcomes of experiments and systematic observations are not usually singular statements (e.g., of the type of the fourth and fifth propositions in figure 3), but regularities about kinds of empirical situations (e.g., of the type of the third proposition: $\forall x \in l \exists y \in g(l) Eyx$). This has been cogently defended by Hintikka, who asserts that what is known as 'induction' in scientific practice is not the inference 'from the particular to the universal', but rather the extension of a regularity from a limited domain to a wider one. It is important to notice, as well, that it is the third proposition in figure 3 what has the logical form Hintikka ascribes to the outcomes of controlled experiments. On the other hand, the very idea of establishing kinds of empirical applications

---

18 See Popper (1959), section 29.
19 See again Hintikka (1999), esp. chapters 7 and 8. He calls 'the atomistic postulate' the assumption that the basis of all knowledge are propositions without quantifiers.
presupposes that some regularities have been found about them: those serving to define that type. After all, we employ concepts to identify those kinds of systems, and, as we have seen in section 2, concepts only have an empirical meaning if they are ‘parasitic’ of some publicly perceived regularities. Hence, if some regularities must have been found in order to construct a classification of empirical systems, then there is no reason why further regularities concerning these systems might not be empirically established as well. Of course, all these regularities are fallible (recall section 2.E).

On the other hand, some comments can be made about the restriction of the size of sets $L$ and $I$. In the first place, this restriction is a desideratum rather than a logical constraint, and probably there are many scientific controversies where no limits are established a priori to the types of laws or applications; what I want to stress is that the verification or falsification of scientific theories can only take place when this restriction is agreed upon by competing researchers.

In the second place, $L$ can be organised in the form of a coherent classification tree of types of laws, containing ‘at the top’ the most general types of symmetries that theoretical models can obey (or fail to do it) within a given theoretical framework. In this case, scientific research can be strongly furthered at both the theoretical and the empirical level, because some empirical regularities may serve to falsify or verify very wide ranges of possible theoretical laws, and not only particular hypotheses.

In the third place, $I$ can also have the form of a classification tree, which allows to organise empirical research systematically, beginning by establishing empirical regularities for very restricted, ‘low level’ types of applications, and ending (with a little bit of luck) with much more abstract laws which are applicable to a wide range of systems. Nevertheless, in many cases no such ‘unification’ is reached, and scientists end simply with a compilation of more or less general regularities, having only quasi-tautological ‘laws’ at the top.

In the fourth and last place, and perhaps more importantly, the strategy of restricting $L$ and $I$ can be seen as the game-theoretical counterpart of two common methodological strategies, usually known as ‘eliminative’ and ‘enumerative’ induction. Eliminative induction is possible just if there is only a limited number of alternative combinations of special laws, and so empirical research can lead either to the rejection of all of them (in which case the full theory becomes falsified) or to the rejection of all combinations save one (which becomes confirmed); traditional expositions presented this methodological strategy as if the relevant combinations of laws were all the conceivable ones, but under this paper’s approach it suffices that the players have agreed on any limitation of them, no matter the criteria employed to do it. Enumerative induction amounts to examining all the possible types of empirical applications, and this allows to verify whether the theory is applicable to all of them or not. The other classical sense of ‘induction’ (say, Baconian induction) is that of making a generalisation from the observation of singular events to a regularity about a certain type of situation; in figure 4 this would correspond to ‘collapsing’ the information obtained from a number of systems like $a^1$, $a^2$, ..., $a^n$, ..., into an empirical law of the form $\forall x \in I^* \exists y \in g(I^*) Eyx$. Figure 5 resumes these methodological readings of the research game: ‘theory building and eliminative induction’ consists in identifying all the possible alternative systems of hypotheses (i.e., possible theory-nets), and using later the results of the cells to decide whether some of these systems is true, or if none is; ‘enumerative induction’ consists in studying all the possible types of empirical applications, in order to test whether they obey the laws assigned to them by a particular theory-net; lastly, ‘Baconian induction’ amounts to the production of the statements contained in each cell, which assert the applicability of some concrete laws to all the individual systems contained within some concrete type of empirical applications.

---

20 This is simply a quick way of speaking. I do not want to enter a discussion about whether Francis Bacon would actually defend this type of induction or not.

21 Similar methodological conclusions are reached in Balzer (2002), where, besides ‘enumerative induction’, a method called ‘hypothesis construction induction’ is suggested, which is closely related to the ‘theory building and eliminative induction’ method of figure 5. Nevertheless, I think that the use Balzer makes there of structuralist categories is basically unrequired by the rest of his arguments.
<table>
<thead>
<tr>
<th>PLAYERS</th>
<th>THEORY BUILDING AND ELIMINATIVE INDUCTION</th>
</tr>
</thead>
<tbody>
<tr>
<td>F</td>
<td></td>
</tr>
<tr>
<td>I_i</td>
<td>a_i^j</td>
</tr>
<tr>
<td></td>
<td>...</td>
</tr>
<tr>
<td></td>
<td>a_n^i</td>
</tr>
<tr>
<td></td>
<td>...</td>
</tr>
<tr>
<td>V</td>
<td>g_1</td>
</tr>
<tr>
<td></td>
<td>...</td>
</tr>
<tr>
<td></td>
<td>g_i</td>
</tr>
<tr>
<td></td>
<td>...</td>
</tr>
</tbody>
</table>

\[ \forall x \in I_i \exists y \in g_i \in I_i \quad \exists y \in g_i \in I_i \]

\[ \text{Baconian Induction} \]

\[ \text{Enumerative Induction} \]

References:


PLAYING CARDS WITH HINTIKKA. AN INTRODUCTION TO DYNAMIC EPISTEMIC LOGIC

This contribution is a gentle introduction to so-called dynamic epistemic logics, that can describe how agents change their knowledge and beliefs. We start with a concise introduction to epistemic logic, through the example of one, two and finally three players holding cards; and, mainly for the purpose of motivating the dynamics, we also very summarily introduce the concepts of general and common knowledge. We then pay ample attention to the logic of public announcements, wherein agents change their knowledge as the result of public announcements. One crucial topic in that setting is that of unsuccessful updates: formulas that become false when announced. The Moore-sentences that were already extensively discussed at the conception of epistemic logic in Hintikka's 'Knowledge and Belief' (1962) give rise to such unsuccessful updates. After that, we present a few examples of more complex epistemic updates. Our closing observations are on recent developments that link the 'standard' topic of (theory) belief revision, as in 'On the Logic of Theory Change: partial meet contraction and revision functions', by Alchourron et al. (1985), to the dynamic epistemic logics introduced here.

ÁNGEL NEPOMUCENO-FERNÁNDEZ (Sevilla University),

THE FUNDAMENTAL PROBLEM OF CONTEMPORARY EPISTEMOLOGY

1 Introduction

Hintikka has studied the Peircean notion of abduction and qualified it as the central problem in the contemporary epistemology. As it is known, Peirce distinguishes three kinds of inferences, namely induction, deduction and abduction, which was first called “explanatory hypothesis” by the American philosopher and it must be taken as different from induction and deduction, though, as it has been pointed out22, W. Whewell considers a form of induction that could be taken as a clear precedent of abduction, but Peirce knew the works of the former. In fact, it so happens that, for both authors, Kepler is the best example of the ideal of scientific method. As abduction is a form of induction as it is not, it should be noted, abduction is a differentiated kind of inference.

In the process of constructing scientific theories, certain system of reasoning is adopted, the underlying logic. Sometimes some facts arise in a way that they should have been a consequence of the corresponding postulates, but they are not, which would be surprising, then new postulates are often added in order to obtain an extended theory or a change of logic may be convenient. That is to say, given a theory Θ, a set of sentences of a language, and a fact represented by a sentence φ of the same language, in the framework of the logical system I-, (Θ, φ, I-) is an abductive problem if Θ and φ have some characteristics because of which the inferential relation I- is expected between them, but Θ ⊧¬φ. We may also require that Θ ⊧ ¬φ. We consider an abductive problem as a triple: (Θ, φ, I-) instead of (Θ, φ) only, since we should take into account three parameters: the background theory, the fact, and the underlying logic. A logical explanation is then another sentence α such that Θ, α ⊧ φ or perhaps the solution could be a change of logic instead of an extension of the background theory. Such change may consist of a new set of inferential rules: given the abductive problem (Θ, φ, I-), the solution may be a new logic I-* such that Θ I-α * φ.

---

OBJECTIVE BELIEF AS A BASIS FOR MINIMAL BELIEF

A long surviving feature of Jaakko Hintikka's approach to logics of knowledge and belief is the idea that relations on possible worlds can be used to define epistemic and doxastic alternatives. In many areas of knowledge representation the logic S5 has been used to model knowledge and, in non-monotonic reasoning, the intended epistemic models are based on universal S5 relations. Since knowledge implies truth in such models, they are less suitable, however, for representing doxastic reasoning about beliefs. In this talk I describe some recent joint work with Levan Uridia in which it is shown how it is possible to define a non-monotonic logic for minimal belief without the assumption that belief implies truth. As a doxastic counterpart to epistemic logic based on S5 we study the modal logic KSD that can be viewed as an approach to modelling a kind of objective and fair belief. We apply KSD to the problem of minimal belief and develop an alternative approach to nonmonotonic modal logic using a weaker concept of theory expansion. This corresponds to a certain minimal kind of KSD model and yields a new type of nonmonotonic doxastic reasoning.
Among the leading philosophers of the present, Hintikka is unique for his interest and his output on historical figures and subjects in philosophy. Husserl and phenomenology have figured prominently among his study targets, having published at least two major pieces about Husserl's phenomenology, with a 20-year gap: one, "The Intentions of Intentionality", appeared in 1975; the other, "The Phenomenological Dimension", in 1995. One recent new piece, "The phénoménologues ou les aventuriers de la forme perdu", in relation to which he tentatively declares his improved understanding of phenomenology, has appeared in a Festschrift volume for Paul Ricoeur around ten years later.

It seems to me that Hintikka has shown a stimulating understanding of phenomenology, better that he has confessed in moments of doubt, and often also better or more revealing of its difficulties that some of the recognized scholars in the field. Thus, in general, I will try to argue that Hintikka is largely right about the doubts he has expressed on the interpretation and value of Husserlian phenomenology.

Two general features are outstanding in Hintikka's work in this area: his campaign against the view of intentionality as directedness, and his emphasis on intuition as central for understanding Husserl. He is almost unique on both counts. In "Intentions", he opposes that view to his own: intentionality as intentionality; in "Dimension", the theme is the limitations of a major interpretation of the first view ("self-sufficient intentionality"); it should be shown that Husserl aims at showing that a "bridge" exists between consciousness and object; and a corollary of Hintikka's view of phenomenology in "Aventuriers" is that if one adheres to intentionality as directedness, then "an authentic phenomenology" is "a theory of non-intentionality" (in the end, no bridge is really needed).

The central perspective in "Dimension" is of Husserl as a theoretician of intuition, both sensory and non-sensory intuition (the so-called "categorial intuition"), in which consciousness confronts directly something which is given to it. Hintikka holds that, for Husserl, what is given in perception (sensory intuition) are never material objects, but raw, unstructured sensations. He contrasts this view with the one by Moore and Russell, with whom he also points out illuminating similarities. In fact, he holds that "the only objects that are completely self-given to Husserl ... are essences". Presumably, it is in pursuing further this clue that he has arrived to what he regards as a deep similarity of the viewpoint of phenomenology in general with Aristotle's theory of thought—the theme of "Aventuriers".

In this last work, Hintikka uses the analogy to discrediting effect: the advance of knowledge abandoned Aristotelian forms for good a long time ago, and nothing much on the positive side should be expected from their near-resurrection in phenomenology. Hintikka connects here his critical overall stance about phenomenology with the criticism he expressed at the end of "Dimension". There he raised the question of whether the articulating and form-giving activities we admittedly perform are accessible to phenomenological reflection or whether they are performed under the surface of our intentional consciousness. The same sort of worry is expressed at the end of "Aventuriers", leading to a desfavorable comparison of phenomenology with the neurosciences in the last pages of that paper (in other work, he had talked of the irrelevance of phenomenology for the neurosciences). My contention is that, Husserl's analysis of a typical case of non-sensory intuition gives us a good illustration of what seems to be completely reasonable in these sort of worries about phenomenology.
From Parmenides to Aristotle the core of the most general philosophical questions evolved as “ontology”. This means that the philosophical thinking and debate on reality as such, on language (in so far as it refers to reality) and on truth (as a relevant relationship between them) developed in Greece within the framework of the verb ‘be’ (εἰμί) in its various forms and usages. In my presentation I shall try to explain and underline some clues to the understanding of the constitution of such an “ontology” and of its powerful development in Aristotle.

No doubt, the first clue for this is to be found in the various usages and meanings of the verb εἰμί. Therefore, I shall start by offering a general view of the main meanings and usages of the verb ‘be’ (εἰμί) from Homer's language on. The results of this survey can be summarized in the following statement: (1) that the verb ‘be’ (εἰμί) originally includes the features of presence (“to be” connotes being present) and that of duration (“to be” connotes lasting); (2) that the notions of reality, of language and of truth are closely interconnected in the different usages of the verb ‘be’; (3) that its copulative and existential usages are not originally disconnected from each other (or at least, there is not the kind of split later emphasized by logicians); and, finally, (4) that in some specific lexical philosophical oppositions (like “being vs. appearing” or “being vs. becoming”) the sense of ‘be’ as “being really” or “being truly” becomes emphasized. In this context we find sometimes some reduplicative expressions like ὄντως ὄν, ὄντως εἶναι (Plato), etc., in order to oppose “being” to “becoming”.

After this, I shall focus my attention specially on Aristotle. To my mind, it is really characteristic of Aristotle that his views on the verb ‘be’ (and on the related ontological issues) are dependent on his general conception of verb (ῥήμα). Therefore, I shall consider his doctrine on both, on the verb as such and on the verb εἰμί. From this specific perspective I shall try to throw some light on his doctrine of predication as well as on his conception of the categories “of being”.

As Prof.Hintikka has written on issues directly related to the content of my paper, along my presentation I shall propose some remarks on the proximity of my own hermeneutics of Aristotle’s ontology to some of his views on the topic.
THE METAPHYSICAL STATUS OF THE OBJECTS OF WITTGENSTEIN'S TRACTATUS

Jaakko Hintikka has intimated that the concept of a simple object is the royal road to the ontology of Wittgenstein's *Tractatus*. What kind of entities are Tractarian objects? Are they point-masses, as James Griffin has contended? Are they phenomenalistic sense-data, as David Favrol dt and others have assumed? These interpretations have been almost universally discarded. However, in the later 70's and in the 80's sophisticated Russellian readings of Wittgenstein's early philosophy have emerged. We can find a vigorous variety of them in Merrill and Jaakko Hintikka's fascinating book *Investigating Wittgenstein* that opened a new avenue to the study of the *Tractatus*. The late David Pears also made insightful probatures into the historical background of Wittgenstein's early philosophy. Both the Hintikkas and Pears have focused on the Russellian ancestry of some key Tractarian ideas. Specifically, they have made interesting comparisons or contrasts between the simples of the *Tractatus* and Russell's objects of acquaintance.

In this paper I will briefly survey some aspects of these suggestive interpretations. They share the claim that Tractarian simple objects are items given in immediate experience. This common tenet does not prejudge whether they are subjective phenomenal entities –sense-data– or objective entities –phenomenological objects, as the Hintikkas have it, or phenomena in the Kantian sense, in Pears' terminology. I critically examine the force of some alleged evidences put forward in support of the view that Tractarian objects are phenomenal items of any vintage. In the course of the exposition, I deal with two thorny issues which should be harmonized with those interpretations and I explain why it seems to me that it is hard to discharge this duty in a satisfactory way. These two issues are the infamous problem of the incompatibility of colours and the evidences from Wittgenstein's texts and testimonies.

The topic of the paper is introduced in section I. Section II includes a critical examination of some consequences the Hintikkas derive from Pears' reconstruction of Wittgenstein's criticisms of Russell's *Theory of Knowledge*. They claim that the developmental story told by Pears is predicated on the assumption that Wittgenstein retained the view that simples were Russellian objects of acquaintance. I argue that it is doubtful that this alleged assumption can be found in Pears' writings. Moreover, it seems to me that it is not clear that it is made mandatory by the role played by the objects in the *Tractatus*. Wittgenstein's treatment in the *Tractatus* of the problem posed by the incompatibility of colours is cursorily presented in section III, and in section IV I pay attention to some internal and exegetical problems with the functional analysis of colour statements essayed in *Investigating Wittgenstein*. The final section contains a brief look into Wittgenstein's own texts and testimonies.
One of the most suggestive lines in Jaakko Hintikka's philosophical trajectory is the one which explores the idea that contemporary philosophy is shaped by a largely tacit confrontation between two ways of understanding language and its relation to the non-linguistic world. On the one hand, language is conceived of as a *universal medium*; on the other hand, language is perceived as a *calculus*. "[...]

the contrast between the two assumptions (the assumption of the universality of language and the assumption of language as calculus) has played a role in the history of philosophical logic, philosophy of language, and analytic philosophy of the last hundred years which is commensurate with the status of this contrast as a Collingwoodian ultimate presupposition" (Hintikka 1997: 21). Anyone who thinks of language as a universal medium takes it to be impossible as it were to place himself outside language and reach a vantage point on it. Such an intuitive grasp has deep implications, among which some can be briefly pointed out. First, the links between names, predicates and the rest of basic descriptive resources, and things, properties, relations in the world become ineffable and elude to be explicitly scrutinized. Second, the project of reinterpretating language as a whole, i.e., of framing what would it be like for their basic expressions to change their meaning, loses its sense. Only those changes that affect local features are acknowledged as such. Even learning a language or acquiring competence in the use of a new symbolism are processes that should be construed as extending the one and only language that exists. Third, the concept of truth, as applied to sentences of a language, plays no independent theoretical role. To predicate truth of any sentence boils down to making an assertion by uttering that very sentence. Fourth, given that, as logical semantics teaches, a systematic definition of the truth predicate provides the basis to establish a rich range of metatheoretical notions, if language is seen as an universal medium, not only semantics but metalogic are judged to be illusory enterprises. Meaning researchers in general, Hintikka says, are forced to be "semanticians without semantics" (Hintikka 1997: 163).

On the contrary, for those who side with the conception of language as a calculus none of these doctrines cut any ice. Among their contradictory claims there is one that has to be defended in the first place, since the rest can be vindicated with its mediation, namely, the legitimacy of thinking of language as made up of bits and pieces that can be interpreted and reinterpreted, and of considering all that work as a subject of discursive treatment. Once this step is taken, nothing stands in the way of carrying out the project of construing semantics in the style of nowadays model theory. Indeed, this is what presumably has happened in the studies on the foundations of logics and mathematics and on the theory of meaning through the twentieth century. In Hintikka's view the progressive rise of model theory has meant "a slow transition from the view of language as the universal medium to the view of language as calculus" (Hintikka 1997: 28), the abandonment of the *universalist* perspective on language and the emergence of the *model-theoretic* tradition. He has convincingly shown that Frege, the early Russell, Wittgenstein, and later analytic philosophers, like Quine, have given form and substance to the universalist tradition's various features. Moreover, he has suggested that there are universalist elements that feature in so apparently unrelated approaches to language as Chomsky's *view of syntax* and semantics and in Fodor's language of thought doctrine. Hintikka has also pointed to logicians like Schroeder, Löwenheim, Gödel and Tarski and to the mathematician David Hilbert as the model-theoretic tradition's founders. If the name of the philosopher and logician Rudolf Carnap does not appear beside Tarski's, that is due to the ambiguous role played by Carnap in building up those model-theoretic ideas that give form to contemporary semantics. Which place does it correspond to this author in the twilight of the universalist tradition and the dawn of the model-theoretic tradition? Is it true that Carnap's philosophical roots belonged to the model-theoretic orthodoxy?

Hintikka has some interesting remarks to contribute to answering this question. In his 1975 paper, "Carnap's Heritage to Logical Semantics", he underlined Carnap's work in model theory, mainly his insights into setting up the foundations of modal logic's semantics. However, he has subsequently argued that there are two Carnaps, the one who wrote the *Aufbau* — a traditionalist — and the author of both *Introduction to Semantics* and *Meaning and Necessity* — a member of the model-theoretic tradition. More to the point is the fact the Carnap who designed *The Logical Syntax of Language*’s Language I is among those "semanticians without semantics" (Hintikka 1997: 193 y ss.), while the designer of *The Logical..."
Syntax of Language's Language II has already, or is about to have, made the leap. If the fact that both Carnaps inhabit even the same work were not enough, universalist and model-theoretic features are difficult to set apart in his work on semantics (Hintikka 1997: 208). I share this analysis and intend to go more deeply into it by closely examining Carnap foundational project in Meaning and Necessity. I do not share the general view that in this work Carnap was on Gödel and Tarski's side. Hintikka disagrees too: “Ironically, Carnap […] remained handicapped by important restrictive assumptions, relics of the universalist position” (Hintikka 1997: 198). However, on bringing to light those assumptions he only mentions the one-domain assumption, i.e., the requirement that “we cannot vary the interpretation of one language, at least not realistically, on a large scale. Hence, we can speak of only one world in our language, namely, that actual world to which our expressions refer in the first place” (Hintikka 1997: 198). “[…] the totalities of the inhabitants of two possible worlds must ultimately be the same” (Hintikka 1997: 199). This diagnosis says less than one should say. My goal is to bring to light several further aspects of Meaning and Necessity's semantics that evince that even in his semantical phase Carnap's work is shaped by universalist constraints.

References

