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Comments

Jaakoo Hintikka

COMMENT ON GARCÍA SUÁREZ

Professor García Suárez's able and interesting paper gives me a welcome opportunity to clarify the issues concerning the status of the objects postulated by Wittgenstein in his *Tractatus*. The main point I want to make is that these issues become tractable if we take into account Wittgenstein's wider background, not just in Russell but in Mach, Husserl, G.E. Moore and in the British realists in general. Purely historiographically, this background is discussed not only in those papers and books of mine that deal directly with Wittgenstein but also in such papers as "The Phenomenological Dimension" [Hintikka (1995a)], "The Longest Philosophical Journey" [Hintikka (1995b)], and "Ernst Mach at the Crossroads of Twentieth-century Philosophy" [Hintikka (2001)]. For one thing, this background, especially the actual meaning and the semantical history of the key notions involved, needs to be much more carefully examined than what has been done in earlier discussion. For one thing, the actual meaning of the key terms, "phenomenology" and "phenomenological" as well as "sense-data" at Wittgenstein's time has not been appreciated. This meaning is thought to be related to "phenomenalism" and "phenomenalistic". García Suárez notes my earlier warning against confusing the two, but still confuses the two in characterizing the logical positivists' reading of *Tractatus* as "phenomenalistic" with atomic propositions consisting of private sense data. For one striking thing, for the Russell who was Wittgenstein's immediate background, sense-data were not private. Russell insisted very strongly that sense data belong to the physical world. They are the objects of immediate sensory awareness, but there is nothing private about them.

This points to a feature of Wittgenstein's background that present-day commentators do not pay enough serious attention to. It is the view prevalent among the British realists that we have direct access to reality in immediate experience. A paradigmatic formulation of this view is Moore's doctrine that in any experience we can distinguish the immediate object of the experience from our having the experience. [See e.g. his "Refutation of idealism" Moore

(1903)]. The crucial point is that that object is, well, objective at the least in the sense of not being mind-dependent.

I believe that Wittgenstein adopted this view, and never gave it up. Otherwise it is, for instance, hard to understand his subsequent claims that we could in principle dispense with expressions like "It seems to me that *p*" and say simply "*p*".

Here we come to a fascinating question about Wittgenstein's relationships to his predecessors. The famous list of ten thinkers that according to him "influenced" Wittgenstein has been misunderstood. It does not list those thinkers whose views he adopted, he lists those who inspired and challenged him. For this reason, he does not include Moore or Mach there. What he took over from them were the obvious (for him) assumptions that did not merit special acknowledgment.

Moore and Russell did not call their views "phenomenological". However, I have argued that in the relevant respects Russell's views were strikingly close to Husserl's, with Russell's "acquaintance" matching Husserl's "Anschauung", with "reduction to acquaintance" matching "transcendental reduction". In spite of smaller differences, Russell could have called large parts of his ideas "phenomenological".

Husserl notoriously did so. Indeed calling a philosopher "phenomenological" in our day and age is likely to be taken to mean bracketing her or him with Husserl and his followers. But this was not the case when Wittgenstein was writing *Tractatus.* This can be seen by asking: what did the term "phenomenological" mean for Husserl? In the beginning of his Amsterdam Lectures he tells us that his phenomenology is further development and radicalization of certain tendencies in the philosophy of science represented by Mach and Hering plus analogical developments in the philosophy of mind. And he adds, that is where he got the term "phenomenology".

This shows what the force of the term "phenomenological" was at the time of the *Tractatus*. It was the sense in which this term is still being used in scientific literature. There it means a theory using only observational concepts, as in a phenomenological thermodynamics using measurable concepts like, volume, pressure and temperature. In his famous controversy with Boltzmann, Mach defended such a theory against a statistical thermodynamics that postulates unobservable – unobservable at Mach's and Boltzmann's time – entities like atoms and molecules and heat as in terms of their unobservable motions. Thus the connotations of the term "phenomenological" were almost the opposite to what a widespread bias involves to-day. It was objective mind independent existence of phenomenological objects that is unproblematic, while theoretical terms could be construed as human artifacts. Wittgensein's remarks on science toward the end of *Tractatus* are not so far from such Machean ideas – or are they Husserelian?

So why didn't Wittgenstein confess his phenomenology in the *Tractatus*? There is a curious but at the same time curiously compelling historical explanation. Wittgenstein had strong likes and dislikes that were often a matter of intellectual style rather than doctrine. One of them was a strong distaste of Mach. In a letter to Russell, Wittgenstein says that reading Mach makes him "sick". Yet, as we saw, there are important similarities between some of their philosophical tenets.

So what would have happened if Wittgenstein had called his philosophy in the *Tractatus* "phenomenological"? Undoubtedly most philosophers would have pidgeon-holed him as a Machean. But Wittgenstein did not want to be thought of as doing philosophy like Mach. Hence he kept affinities with Mach strictly under the lid. He does not mention him publicly.

Only after Wittgenstein rejected in 1929 the primacy of phenomenological languages did he begin to refer to Mach, for now he could criticize Mach as he did in such works as his *Philosophical Remarks*.

The strongest proof of Wittgenstein's phenomenology in *Tractatus* is his self-acknowledged phenomenology in the twenties. If you claim that these two positions are not basically the same, you assume an onus of explaining how and why he came to change his mind in this way. We know that his thought changed in the late twenties. I have studied aspects of this change, and found the crucial, sometimes specific sharp changes, but they support the identity of Wittgenstein's phenomenology in the twenties with what to me is the obvious phenomenology in *Tractatus.*

Wittgenstein was himself misleading in that he overemphasizes in the *Tractatus* his disagreement with Russell. García Suárez illustrates well the fact that there was one and only one fundamental difference. It was the status of logical forms as objects of acquaintance in Russell, especially in the *Theory of Knowledge*. Wittgenstein, unlike Russell, did not have to postulate "logical experience". But there is precious little to suggest that there were other metaphysically fundamental differences.

García Suárez is right in arguing that Pears' account of the change does not support our phenomenological interpretation. But I do not agree with Pears. He has not offered an account of the nature of the picture theory which is a much subtler thing than he thinks, something Wittgenstein arrived at by some sort of "osmosis".

As to the foundation of logic, García Suárez could have been more explicit about the connection between the three main issues, connections between simple predicates, the tautological character of logical truths and the logical form of simple objects. The crucial idea implicit here is the idea of the universe of discourse. For Russell, we do not understand a language unless we know what the universe of discourse that is being presupposed. If so, the kind of truth-functional logic that Wittgenstein assumes is the whole story about logical (a priori) relations among propositions if and only if the basic

objects do not depend on each other, that is, if and only if all distribution of truth values is simple propositions are possible. The issue is in a sense not the correctness of a language with conceptual truths other than tautologies, but the exhaustiveness of the truth-functional logic. This is why Wittgenstein could later refer to the logic of the *Tractatus* where all basic objects are logically independent of each other as a "logic of tautologies".

García Suárez mentions the possibility of "saving" the independence in Wittgenstein's paradigmatic case of color incompatibility through an interpretation of the concept of color as a function (mapping from points in visual space into color-space). This has only illustrative value, however, for Wittgenstein did not countenance functions as "objects", that is, nonlogical primitives.

However, a correction is needed to what García Suárez says. He says that the referents of *r* (for red) and *g* (for green) "do not qualify as simple objects". But in his explanations to Desmond Lee in the early thirties he lists in so many words colors as examples of simple objects.

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Comment on Calvo

Professor Calvo's rich and perceptive paper shows once again how illuminating close philosophical and/or logical analysis can be in understanding other thinkers including older ones. Calvo's analysis relies more than mine on philological expertise, but the two are in the last analysis intertwined.

Indeed, one of the most central earlier ideas of mine, with which Calvo agrees, concerns the precise meaning of Aristotle's words. It is his conscious rejection of the Frege-Russell ambiguity thesis. Everybody agrees that verbs for being like the English *be*, or the ancient Greek *εἰμί* are used differently in

different contexts. The Frege-Russell thesis is that the reason for this difference in use is that these verbs are ambiguous. It is customary to distinguish them from each other at least the *is* of identity, predication, existence, and class-inclusion. Some analysts also list an *is* of facticity (as in "so *be* it") and on closer scrutiny we also have to distinguish from each other the plain identity (coincidence), functional identity as in (*f*(*a*) *is* b) and identifying identity ("That man *is* Alfred Tarski"). My first systematic observation was that the ambiguity thesis is dispensable in semantics, including arguably the semantics of English quantifier phrases.

Once this systematic dispensability became clear, it was not difficult to see that no major thinker before the 19th century assumed the Frege-Russell ambiguity thesis. Aristotle was exceptional in that he considered the thesis but ended up rejecting it. [See *Met.* Γ 2, 1003b22-30].

For Aristotle, *εἰμί* was not ambiguous between the different Frege-Russell senses. Rather, these senses were components of a single concept. There was according to Aristotle a difference in the precise force of the verb on different occasions, but his distinction was between allegedly different categories, not between different Frege-Russell meanings.

This categorial distinction was not an outright ambiguity or "homonymy" (accidental use of the same word in different senses). The categorially different uses of being were connected by their respective relationships to one and the same "focal meaning". The interpretation of this part of Aristotle's metaphysics has been intensively debated ever since Gwilym Ellis Lane Owen's seminal papers "Logic and metaphysics in some early works of Aristotle" and "Aristotle in the snares of ontology" [Owen (1960); (1965)]. Owen relies heavily on this focal meaning but does not fully explain what precisely it is conceptually.

The way Aristotle in effect construes the different Frege-Russell meanings as different components of a single meaning poses the interpretational problem of understanding their relationship and interaction. For one thing, one of these component senses may be or at least may seem to be absent. Aristotle did not have available to him a separate verb for existence. The existence of A was expressed by the absolute construction that literally translates as A *is* ("period, end of story"). Then a predicative sense does not seem to be present at all.

Conversely, Aristotle points out himself at *De Int*. 21a25-29 that from "Homer is a poet" we cannot infer "Homer is". This idea is connected with what Calvo interestingly points out about the most literal meaning of a verb like *εἰμί* expressing present being.

Calvo also discusses the delicate relation of the predicative *is* to the other senses of being. I found his discussion of the relationship between the predicative *is* (as in "a man is running") to predication simpliciter (as in "a man runs") extremely illuminating.

Likewise, Calvo's analysis clarifies the relationship of predication ("A is pale") to the existence of what we would call a fact. ("there is paleness in A") and also to the existence of a compound entity ("a pale A exists"). Understanding these relationships in Aristotle is indispensable for interpreting what Aristotle says, especially in *An. Post.* B.

I also found illuminating Calvo's discussion of verbs for being sharing the characteristics of verbs in general in Aristotle, especially in relation to time and time-reference. I have earlier discussed the concept of time and the ways of time reference in ancient Greece in my paper "Time, Truth and Knowledge in Ancient Greek Thought" [Hintikka (1967)]. There I emphasized the primacy of time-keeping in relation to the present in ancient Greek thinkers like Aristotle.

This role of the present moment ("now") leads to interesting further questions. A modern (post-Russellian) philosopher would be tempted to express Aristotle's view by saying that for him the proper universe of discourse consisted for Aristotle of all the presently existing entities. This can be taken to be what Aristotle is doing but it cannot be the whole story, for Aristotle certainly found himself speaking of objects that do not presently exist.

If we insist on talking about universes of discourse (in the teeth of the fact that this concept was introduced into logical theory only in the 19th century), we therefore have to think of Aristotle's universe of discourse as comprehending objects that do not exist (now). This would mean that quantifiers can for him range over non-existing entities. In some sense that is not easy to capture precisely, this is true of Aristotelian quantifiers. By "some A's are B's" he does not mean that some actually existing A's are also B's. Thus for one thing the link between the "existential" quantifier "some" and actual existence is severed.

I have indeed examined how Aristotle expresses existence in a syllogistic context. [See my paper "On Aristotle's Notion of Existence", Hintikka (1999)]. My conclusion was that in a syllogistic theory existence comes in as a part of the force (in a given context) of the predicate term. That is almost a corollary of what was said earlier in this comment. In that in "A is B" the "is" may or may not include the existential component, depending on the context. This not only seems to capture Aristotle's meaning but throws interesting light on later history, including the genesis of modern logic. (See my paper with Risto Villkko, "Existence and predication from Aristotle to Frege", [Vilkko and Hintikka (2006)].

But what is the universe of discourse of an Aristotelian syllogistic science? If "is" does not necessarily imply existence, what does it basically mean? Which of the Frege-Russell component senses of "is" if any, is indispensable?

It seems that there is a definite answer to this crucial question. The indispensable common element is the *is* of identification. The basic idea is eminently natural. We must always know what we are talking about. Hence A *is*

B always – that is, independently of the rest of *is* – implies that A *is* in the sense of the *is* of identification. I cannot elaborate this point sufficiently here, but there seems to be plenty of evidence for such an interpretation. The best evidence comes from Aristotle's theory of the structure of a syllogistic science. (See *An. Post.* A10, 76b1-11, A24 etc.) In any one science, the meaning of the genuine term defining the scope of that science must be known. That meaning is presumably just what an *is* of identification expresses.

If this is so, then an Aristotelian "universe of discourse", in so far as we can speak of one, is not the totality of existing entities but a totality of objects we know, not in the sense of being acquainted with them but in the sense of being able to identify them as separate objects with their own identity. Thus roughly speaking an Aristotelian universe of discourse is the totality of epistemically possible objects, objects we can in principle recognize. In brief, known objects rightly understood. Aristotle's logic has in this sense an epistemic element.

This interpretation might at first sight strike you as a highly contrived reading. It nevertheless has a solid backing in epistemic logic and then has also striking indirect historical evidence for it. A substantial part of Greek mathematics consisted of studies of what in geometry is "given," a datum, assuming that a certain geometrical object or configuration is "given". The entire book by Euclid called *Data* is devoted to such questions. The only reasonable interpretation is to take this "given" to be what is known. (In the middle ages this was recognized even terminologically.) In my paper "The method of analysis as a paradigm of mathematical reasoning" [Hintikka (2011)] I show how from the vantage point of this interpretation we can understand various characteristic features of ancient Greek geometrical literature. If this is correct, Aristotle's quantifiers are not any stranger creatures than Greek geometers' quantifiers that sometimes range over known objects. Greek mathematicians were fully aware that not all existing objects could be assumed to be "given". A square with the same area as a given circle might be a case in point.

The primacy of the identificatory sense of being is somewhat obscured by his additional view that one can only know in i.e. full sense what exists. This idea of Aristotle's nevertheless seems to be a conclusion from arguments, not a conceptual assumption.

In the light of what has been said we can also approach the puzzling question of the relation of different categories to each other. What is the mysterious looking, the "focal meaning" that is supposed to unify the categorially different *is*es so that Aristotle can after all have a unified science of being *qua* being? I want to suggest a simple answer to these problems. According to it, the *is* that is the basic meaning of being in different categories is the *is* of identification. This is the focal meaning of being for Aristotle. Furthermore, this sense of being is the substantial being, the variety of being characteristic of the

being of substances. [See here my paper "*Ta meta tâ metaphysica*", Hintikka (2006)].

This interpretation differs sharply from the conventional view according to which the substance of any A is what makes it operate the way it does, not what makes it possible to identify it. I believe that at the end of Aristotle's argument in his *Metaphysica* these two ideas are supposed to coincide, so that the traditional view cannot be said not to be Aristotle's. What I want to show is that we can understand Aristotle's line of thought in his *Metaphysica*, including his assumptions, the alternative answers he considers, and the import of his own solution, without assuming the traditional interpretation of Aristotle's notion of substance.

I cannot argue for this interpretation fully here, beyond presenting an example of how it illuminates Aristotle's meaning. If I am right for Aristotle every assertion "A is B" has a potential elaboration

"A is such-and-such a substance C, and that C is B".

Here the first *is* is the is of substantial being and therefore ipso facto the unavoidable is of identification. The second *is* can be predicative or in fact the *is* of any other category. This shows in what way the focal meaning of being (as the being of substance) is involved in the attributions of being.

An exception is present if A already expresses a substance. The term C and the second 'is' disappear and we have a use of is *simpliciter* or ἁπλός, as Aristotle sometimes says. (See here e.g. *Met.* Z1, 1028a30-31.) Another way of expressing the same for Aristotle was to speak of being qua being, which for him is the subject of metaphysics, the study of substances.

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COMMENT ON ACERO

My old friend Juan Acero's paper has several different kinds of significance. It is not only a perceptive discussion of Carnap's central work in logical semantics. It relates what Carnap does in his *Meaning and Necessity* [Carnap (1947)] to one of the most important issues in our language theory, the contrast between approaches to language as a universal medium as distinguished from language as a calculus-like communicative and expressive toolkit. There is little doubt of the historical and systematic importance of this contrast.

Juan Acero's paper is much more than a study of *Meaning and Necessity* in relation to this grand contrast. It offers an instructive methodological example of how conceptual and other systematic insights can be brought to bear on the history of ideas, especially of philosophical ideas.

A historiographical analogy perhaps illustrates what I mean. Isaiah Berlin gave his essay on Tolstoy the title "The Hedgehog and the Fox" [Berlin (1953)], referring to a proverbial distinction between one-idea or one vision thinkers he labelled "hedgehogs" and the "foxes" whose strength lies in mastering a variety of different methods and perspectives. For a while, classifying sundry thinkers and writers into "hedgehogs" and "foxes" was something of an intellectuals' parlor game. However, Berlin's point was far deeper than pidgeon-holing Tolstoy. His brilliant insight was that Tolstoy was in terms of his distinction a fox who believed that he was a hedgehog.

This is what happens to neat dichotomies in real history. However clarifying and however fundamental a conceptual distinction is, in actual life the interesting and intriguing cases are the ambivalent ones. They are the ones that bring out the dynamics of the conceptual situation. Thus it is for instance instructive to realize that Quine was an unremarkable instance of a believer in language as universal medium, but just because of this simplicity the observation does not tell much of any inner tensions in his thinking. Much more interesting is an ambivalent logician like Tarski. In the great friendly controversy in the 1940's he surprisingly sided with Quine against Carnap's project of logical semantics. This is surprising because he later ended up as the architect of what is the technical core of the "calculus" view viz. model theory as a discipline of logic. This reveals, for one thing, the complexity of Tarski's intellectual motivations and preferences.

Similarly, as Acero's study helps to bring out, Carnap's attitude toward the "universal medium" tradition was highly mixed at the different stages of his career. Under the influence of Wittgenstein's *Tractatus* [Wittgenstein (1922/1961)], Carnap was in the twenties suspicious of all talk about language in language. Wittgenstein himself claimed that Carnap's idea of "formal mode of speech" did not constitute a single step beyond the inexpressibility of language dealt with in the penultimate paragraph of the *Tractatus.*

Later, Carnap's conception of a logical syntax of language included elements that later philosophers would have called semantical. Still later, as Acero ably spells out, Carnap shows an unmistakable tendency to prefer syntactical notions such as state- description (especially truth in a state description) to their semantical counterparts like model and truth in a model.

Thus Acero's paper is not only an exemplary study of an aspect of Carnap's philosophy of language. It is an instructive example of how conceptual insights can and must be applied to actual historical material.

One thing about such applications is that the mixed cases typically bring out further conceptual distinctions. For instance, the universal medium vs. calculus contrast manifests itself as Acero's discussion illustrates in the rejection or acceptance of genuine independent model theory. But it can have other manifestations. One of them can be the rejection or acceptance of the idea of metalanguage. For example, Wittgenstein's continued commitment to the universal medium idea manifested itself in his sharp rejection of the idea of metalanguage.

Notwithstanding Carnap's lingering commitment to the universality view, in this respect he was in his later thought using certain "calculus" ideas, especially the object language vs. meta-language distinction, without any reservations. I hope Acero will next attend to this aspect of the history of logical thinking. For the object language vs. metalanguage distinction was not a traditional one. It is for instance not always appreciated what a drastic change it was in Russell's thinking when he advocated in his preface in *Tractatus* a hierarchy of languages as a solution to Wittgenstein's inexpressibility dilemma.

Another way in which model-theoretical or at least semantical ideas are smuggled into Carnap's later thought was via the notion of intension as in the method of extension vs. intension. For to speak of intensions is unavoidably to speak of language-reality relations and even to reify certain aspects of those relations into separate meaning entities. In this direction, too, the different ideas that together could be called model-theoretical play different roles in Carnap's thinking.

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COMMENT ON SAGÜILLO

Some philosophers maintain that the proper task of philosophical activity is the explication of our concepts. If so, the state of art in the problem area discussed in José M. Sagüillo's interesting, able and timely paper will have to be taken as an indication of a failure to reach a satisfactory explication of some of our most central concepts, including information, probability and deductive inference. A couple of historical examples illustrate this confusion.

In the early fifties, John von Neumann was intensively searching for a new concept of probability, better suited than our current ones, for the purposes of physical theorizing. (This search is mentioned in my comment on Sandu.) A characteristic feature of the new concept was to be that it is based on an appropriate logic.

At the same time, Rudolf Carnap was developing his theory of logical probability. But when he explained his ideas to von Neumann during his sojourn at the IAS, von Neumann and other physicists rejected them altogether, in spite of his concept's being logic-based, as von Neumann demanded.But von Neumann never spelled out what was wrong in Carnap's approach (besides its limited applicability) nor developed a more satisfactory explication himself.

The other conceptual mess concerns the "scandal of deduction". A long standing continental tradition culminating in Ernst Mach and (the early) Ludwig Wittgenstein maintained that all deductive inference is tautological, yielding no new information. In Wittgenstein's *Tractatus*, this idea was in effect pointed out to be a consequence of his idea that all logic is ultimately truth-functional.

Alas, it is not and it is not obvious how the idea of tautologicity can be made to apply in richer logics. The logical positivists tried to escape this problem by weakening the claim of misinformativeness to involve only "analyticity" not tautologicity in any literal sense. But the notion of analyticity introduces more problems than it solves. As a result, most philosophers seem to operate on the assumption that deduction somehow produces new information – after all, deductive inference is a mode of information-processing – without being able to tell what that new information is.

In my paper, "Who has Kidnapped the Notion of Information?" [Hintikka (2007)] and in related papers, I took the Wittgensteinean idea of tautology and tried to extend it to the entire first-order logic. The technique I used was that of constituents and distributive normal forms. Up to a point, the ex-

tension works, but at the cost of having to distinguish between two kinds of information and probability, surface information and depth information.

The latter can be thought of as the total information that can be extracted purely logically from a given proposition. But that information is not given to me directly by a written or spoken proposition. The proposition only gives in effect a list of possibilities that the proposition excludes and admits. But some of those possibilities can be only apparent ones. Their elimination does not increase the depth information of the proposition, only its surface information.

Logical inference is then tautological as far as depth information is concerned, but not for surface information. But in a sense we cannot directly deal with depth information because of its limit characteristics. The amount of depth information that a proposition carries is not in general an effective function of its Gödel number.

In this whole area, the situation has changed sharply. Our basic logic has turned out to be (suitably extended) IF logic and as a consequence the right concept of probability is IF probability. A constituent *C* that seem to express a merely apparent possibility is now interpreted as descriptions of possible worlds in which *C* is neither true nor false, but has a definite probability.

As Sagüillo ably points out the depth vs. surface distinction does not solve all problems. For one central thing, in some obvious but complicated sense the two kinds of information are at bottom identical. Deduction is processing information as such, not some particular variety of information. But in what sense? Is the common element the idea of excluding possibilities? But the most apparent possibilities that have to be postulated for surface information are problematic. Not only is their semantical status puzzling. Their numerical values cannot be assumed to be known, for such values are not effective functions of the Gödel numbers of the propositions in question.

Sagüillo nevertheless maintains – correctly – the objectivity of both kinds of information but considers the distinction essentially as an epistemic one.

This entire problem complex is put to a new light by the discovery of IF probability discussed by Sandu in his contribution to this volume and by myself in my comment on Sandu. At once, we obtain an overview of the problems. No ultimate distinction between two different kinds of probability and information is needed. In so far as the information of a proposition *S* is measured by 1 – P(*S*) the two kinds of information are measured simply by P(~*S*) and P(¬*S*) respectively. There P is IF probability and ~*S* and ¬*S* the two kinds of negation.

Hence, the objectivity of the depth vs. surface distinction is vindicated. It has not only epistemic but logico-semantical objectivity.

A "merely apparent" possibility now becomes a "real" possibility that a given proposition is neither fully true nor fully false but only true with a certain probability. These possibilities turn out to be represented by those constituents

that are internally consistent in the sense explained informally in my comment on Sandu but nevertheless not true in any actual model. The need not be actually false, either, but true with a certain IF probability.

The analysis of such possibilities in terms of the corresponding constituents that I carried out in my "Who has kidnapped ..." [Hintikka (2007)] paper now yields a useful by-product. It can be seen from these constituents how one can define purely logic-based and hence a priori probabilities of the kind von Neumann seems to have been looking for. In the purely monadic case with A, B, … as the nonlogical primitives, natural measures assign equal a priori probabilities to the different kinds of individuals characterized by different constituents with a free variables, as in

$$A(x) \,\&\, B(x) \,\&\, \ldots$$
$$\sim A(x) \,\&\, B(x) \,\&\, \ldots$$
$$A(x) \,\&\, \sim B(x) \,\&\, \ldots$$
$$\sim A(x) \,\&\, \sim B(x) \,\&\, \ldots$$

Now a constituent of depth d is a ramified list of all the kinds of sequences of d individuals that one can draw from one's model. Natural a priori measures are obtained by maintaining symmetry between those different kinds of sequences in the same sense as the symmetry between the different kinds of individuals in the monadic case.

Doing so might not seem to be a great feat in the case of the concepts of probability and information. However, we can in a similar way generalize the intriguing notion of entropy from the monadic case, where it is easily defined in the usual way, in the general first-order case where relations are also involved. This seems to be useful for discussions about the foundations of thermodynamics.

Besides thermodynamics, logic-based probabilities can be useful in the theory of Bayesian inference. All told, we have here a rich lode of applications of IF probability.

What is new is the dependence of such measures of probability, information, etc. on the depth of the analysis, reflected by the quantificational depth d of the relevant constituents. The new perspective or perhaps the new dimension this introduces remains to be investigated. The new questions that have to be raised concern IF probability itself.

In the new perspective, we can sort out the multiple relationships between probability, information and excluded possibilities on the one hand and deduction on the other. I see little hope at the present time for an adequate inferential-practice oriented account of deduction. For one major thing, philosophers have failed to develop an adequate account of the "inferential practices" that people actually use. It has turned out that mathematicians were already at Frege's time using concepts and modes of reasoning that cannot be

captured in the traditional first-order logic that has generally been taken to be our basic logic. Typically, additional modes of reasoning such as the "axiom" of choice are supposed to be found in set theory. But the current axiomatic systems of set theory are a disaster area, as spelled out in my forthcoming paper "Axiomatic Set Theory *in memoriam"*.

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COMMENT ON NEPOMUCENO-FERNÁNDEZ, SOLER-TOSCANO AND VELÁZQUEZ-QUESADA

Why have I called (with some intentional exaggeration) abduction "the fundamental problem of contemporary epistemology"? We humans do not have an innate idea of abduction in the way we have – or seem to have – intuitive ideas about probability. The notion of abduction is central in Peirce's thinking, but he cannot be claimed to have solved the problems connected with it, let alone all the problems of contemporary epistemology. The attempt to understand abduction as an inference to the best explanation fails, as one can perhaps see best in a Bayesian situation. For an application of Bayes' formula, you need both priors and likelihoods. An inference to the best explanation amounts to using likelihoods only, as in Fisherian statistics. This is not so much fallacious as wasteful. Some available information, the kind of information that could be codified in the priors, remains unused.

It is not clear either, how abduction should be approached logically. It is supposed by Peirce to be an all-comprehensive procedure of theory formation. Hence an explication of the idea of abduction that relies on a "background theory" is not likely to get to the bottom of things.

The deep problem of abduction is to understand the rationality of scientific theory formation and even more generally hypothesis formation. In earlier discussion the failure of the naïve inductivist project led to the idea that "contexts of discovery" do not allow any rational logical or epistemological treatment. The discovery of a genuinely new theory is a matter of intuition, guesswork and serendipity, not of any rational rules. Even though such a bland view is no longer popular, a satisfactory analysis of ampliative steps of reasoning, a satisfactory account can scarcely be said to be available.

The purpose of my comment is to point out – or perhaps rather remind and underline – that the "abductionist's dilemma" can be made to disappear

when ampliative reasoning is considered as a questioning process. The true nature of the interrogative approach to inquiry still is not generally appreciated. The authors I am commenting on are on the right track in mentioning and using the technique of semantical *tableaux*. But in their hands it is only one particular deduction technique. Semantical *tableaux* really come into their own when they are used as a book-keeping method for interrogative inquiry in general. This use means going beyond the deductive use in one respect only. At any stage of the *tableau* construction, the inquirer may ask a question whose presupposition has been established, i.e. is present on the left column, to ask a question whose answer, if available, is added to the left side of the *tableau.*

Here the notions of question and answer have to be understood in a wide sense. This is legitimate because any new item of information can be thought of as an answer to a question in the sense of being its desideratum. [See here my paper "Second-generation Epistemic Logic and its General Significance" [Hintikka (2003)].

The interrogative model will have to include in any case moves whose purpose is to test critically the answers that an inquirer receives, if then. Because of this, rational guesses can be epistemologically fully respectable answers to questions as any others.

Of course, there are better and worse guesses. But such an evaluation is a matter of strategic rules, not definitory ones.

If abduction is thought of as rational guessing, the interrogative model automatically provides a slot for it as a legitimate kind of step in any rational inquiry. Whether you want to call such conjectural question-answer steps inferences or not is partly a matter of terminological taste.

Admittedly, we have to make a fundamental distinction between two different kinds of steps in inquiry. The distinction can be explained with reference to *tableau* building. At any stage, the inquirer can choose one of the propositions on the left to be the presupposition of the next question-answer step. Or else the inquirer can choose one (or two) of the same propositions to be used as a premise (or premises) of a deductive step of *tableau* construction.

By way of definitory rules, all we have to do is to allow rational guessing of answers. In both kinds of moves, the evaluative aspect comes in the form of strategic rules.

Now the deep insight here is that strategically the two kinds of steps in reasoning are entangled. From the point of view of definitory rules, a choice of a proposition to use as a presupposition of a question and the choice of a proposition to serve as input of a deductive step are entirely different. In both choices, it is impossible in general to formulate mechanical (effective) rules for choices. But the strategies to be followed in the two choices are related. The precise relationship is complicated and would deserve a careful discussion. The main connection is nevertheless clear. If we are in a context of pure

discovery, in the sense that all the answers are known to be true, then the optimal choice is the same for both kinds of moves, other things being equal. Roughly speaking, for the purposes of pure discovery the best interrogative strategies match the best deductive strategies. Hence the strategies of informed guessing are substantially like strategies of deduction. No matter what terminology you use, whether you choose to speak of deduction or not, this is a solution to the true problem of abduction: What is it that makes informal guessing rational?

This solution to the problem of abduction is not new. It goes back at least as far as my paper (with Merrill B. Hintikka) "Sherlock Holmes Confronts Modern Logic" [Hintikka and Hintikka (1982)]. A reader might also want to consult my *Socratic Epistemology* [Hintikka, 2007].

I will not try to relate this approach to abduction to the details of the paper I am commenting on. The main similarities and connections are in any case obvious. I would suggest, for the sake of theoretical clarity, an exclusive use of the interrogative model as the logico-epistemological framework in discussing abduction. For instance, abduction is not a matter of theory change; it is a matter of theory formation.

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Comment on Salguero-Lamillar

The specific details of Salguero-Lamillar's well-informed and able paper are difficult for me to comment on. The reason is that I am increasingly finding the conceptual foundations of the discussion in the entire literature on the issues he is dealing with in need of clarification. I will list here a few major problems. Salguero-Lamillar is in fact clearer than many others, and hence my comments should not be taken to be targeted on his paper alone.

(1) The use of the process of interpretation as a paradigm for language understanding in general seems to me misleading. When one is using language for a purpose, one already has to master the representative (semantical) relations between language and reality. As Wittgenstein once expressed this point: if I am asked how I can understand what you mean, in that all I actually have are your symbols, then I ask you how can I know what I mean, in that all I have are my symbols.

Salguero-Lamillar is right in thinking that language understanding involves often assigning fresh references to certain linguistic symbols, including anaphoric expressions and free variables. Such interpretative assignments of references are nevertheless best understood as taking place in some particular play of a language game. The context that is involved in such cases is neither of an expression nor the context in which something is uttered or otherwise proposed. Rather it is the context of a move in a language game.

(2) This is connected with the idea of an application of a language. Here I am making some of the same points as were made in my paper, "A distinction too many or too few?" [Hintikka (2003)]. Quine has suggested looking at a language in its relation to reality in the same way as the relation of a scientific theory to the world. Now such a theory is in basic sciences virtually never applied to the world at large, unless one is doing cosmology. It is applied to what a physicist might call a system, that is, to some part of the real world or a postulated world that is sufficiently isolated so that the relevant laws can operate in it independently of what is outside of it. This isolation can be conceptual, accomplished by assuming certain boundary conditions which are in the application of the laws of nature are taken for granted. This fragment of the universe can be tiny. One of the most intensively scrutinized "systems" in physics is a single hydrogen atom.

The point is that the language (symbolism) used in studying such a system has to be taken to be an entire language, not a specialization of some overall system of relationships between one language at large and the world.

This is related on the logical level to the characteristic feature of the first-order languages that are often considered as being typical by philosophers, namely the fact that, such a language is not fully given until its universe of discourse is given. The abstraction that some philosophers have indulged in is thinking that all the different universes of discourse we used can be pooled together into a single one of "everything that is". The idea is nevertheless hopelessly unrealistic.

(3) On a meta-semantical level this problematic manifests itself among other things as a partial overlap of the concepts of possible world on (possible) context. It is one of the reasons why I find Salguero-Lamillar's paper hard to comment on in detail. There is nothing wrong, however, with such

usage in his paper or anywhere else. Already Richard Montague moved between what he called "possible worlds" and context of use. For instance, model sets can be thought of as descriptions of either.

There are differences between the two, however. If the idea of possible world is taken seriously, the only "world" we can have information about is the actual one. The others are simply possible alternatives to the world as a whole. If you were suddenly moved to another possible world, you could not know where you came from or make any reference to the nature of your original (actual) one.

It follows that in conceptualizing possible worlds, we cannot rely on any comparisons as the closeness (similarity) of two possible worlds. For instance the counterpart of an individual in another world cannot be defined by such comparison. I have myself indulged in the past in such notions as "identifying functions" that conceptually presuppose inter-world comparisons. However, I have grown increasingly skeptical about their interpretability. For instance, the notion of rigid designation is totally vacuous. For saying that an expression designates the same individual (or other entity) in different possible worlds would make sense only if we already had a way of comparing entities in different worlds for their identity.

What has happened is that possible worlds are taken to be like different application contexts which can be compared on a meta-level. But comparisons are a posteriori matters, not a part of the a priori structure of the language in the strict sense that is being applied on any one occasion.

What really happens in our actual language and what should be studied also in the modus operandi of logical language is that those alternative worlds we consider have (from a purely logical point of view, happen to have) a map-like structure in common that can be used to identify individuals by locating them in it. This kind of identification system is largely independent of whatever system of references is being used. In fact in our actual discourse we operate with a variety of different identification systems in the one and the same discourse. The rich – both linguistically and philosophically rich – problematic that opens up here largely waits for a detailed investigation.

The role of context in logical semantics offers an abundance of open questions. Some of them are mentioned in my comment on the other papers in this volume.

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COMMENT ON PIETARINEN

Ahti Pietarinen's paper is a source of a special pleasure for me, and not just because I happen to be a *Doktorvater* of his father. It is because Pietarinen's paper is an example of historical studies at their best. He points out an interesting tradition in logic and philosophy and examines it. Once again, I find myself enjoying the distinction of being anticipated by Peirce. For Pietarinen is right in his first and foremost thesis that game-theoretical semantics was genuinely present there in Peirce. I admire among other things Peirce's sensitivity to the conceptual status of semantical games not as being themselves played in knowledge-seeking but constituting the language-world links that have to be mastered in all language understanding.

Pietarinen is also talking up an important subject when he discusses the history of the fascinating role of model construction of logic.

It is thus no reflection on the value of Pietarinen's paper to say that it cries out loud for further discussion. The problem is to put Pietarinen's results in a wider historical and systematic perspective. For one central question, how should we understand the history of the idea of game-theoretical semantics? Was Peirce's discovery just an anticipation of a later theory? In order to do systematic and historical justice to what happened, we must realize that something much deeper was involved. What was at issue was the meaning of the most central logical concepts, the basic quantifiers.

Pietarinen's paper helps to bring this out through a comparison with Frege. Frege is commonly credited as the main founder of modern logic, especially its basic part, quantification theory. Peirce figures as having made essentially the same discovery independently, albeit with much less rigorous formulation.

On closer scrutiny, this popular view is seriously mistaken. [See here my paper, "Which Mathematical Logic is the Logic of Mathematics?", Hintikka (2012)]. Frege did not understand adequately the logic of quantifiers. He failed to realize the semantical role of quantifiers as expressing through their formal dependence and independence relations the actual 'material' (in)dependence relations between their respective variables. As a consequence, he did not understand the unformalized logic of quantifiers that working mathematicians like Weierstrass were using in their cutting-edge mathematical work, especially in analysis. As a further consequence the logic of quantifiers Frege created was inadequate. It was too poor conceptually even to capture the actual concepts and modes of reasoning that were being used in mathematical practice. What is more, in traditional first-order logic that later logicians used and is generally considered as the central part of modern logic is equally defective.

The deeper significance of Peirce's semantics is that this semantics gives logicians the means of formulating a richer and more adequate logic. In

fact, Peirce was fully cognizant of the methods of reasoning that Frege failed to understand. He did not (as far as I know) formalize those conceptualizations, but if he had had to face the task, he could easily have done so.

This fact puts the entire history of logic in the last 150 years in a new light. Frege's influence notwithstanding, he has a much more modest claim to be the founder of modern logic than Peirce.

In general, this should change historians' approach to Peirce's logic. It is usually studied from the vantage point of later developments in logic, for instance as anticipating the traditional first-order logic and perhaps offering various suggestions as to improving it in different ways, especially ways of graphical representations of logical reasoning. In reality, Peirce's logic should be considered as being more advanced than much of twentieth-century logic (before game-theoretical semantics). Instead of asking to what extent Peirce was anticipating of later advances in logic, historians should ask to what extent later logicians have recovered Peirce's insights. One thing that makes Pietarinen's paper valuable is that he does not follow the usual interpretive trend in this respect.

Somewhat similar things can be said of another fascinating idea considered by Pietarinen, viz. the idea of model construction. One of Quine's most telling mistakes was to dismiss model theory as being little more than a clever method of studying the dependencies and independencies in axiom systems. A less misleading perspective would be to consider our entire logic as being little more than a science of model construction and thought experimentation with models. Logic is usually thought of as the science of inference. But an attempted inference (from *A* to *B* say) is worthless unless it is known to be truth–preserving. And how do you come to know that? By trying to construct a model in which *A* is true but *B* not. If such an attempt leads to a dead end and in all directions, you will be happy to acknowledge the validity of the inference from *A* to *B*.

As Pietarinen points out, this idea of model-building is especially visible in the *tableau*-method introduced by Beth (as well as in the tree method I published slightly earlier).

A bit of history perhaps illustrates my point. Nearly half a century ago the *dernier cri* in the psychological study of human reasoning was the insight by Johnson-Laird and others that in our spontaneous deductive reasoning we humans use mental models. For a logician using the *tableau* method this would not have been news. Rather, it would have posed the intriguing question of why in our practice of logical reasoning we ever use methods other than mental (or some other kind) of modeling. This question is not frivolous, but has to do with the meaning of the so called cut-elimination results central in proof theory.

Hence in discussing the idea of model building in logic Pietarinen touched on a subject that is even more important than what he brings out.

He could also have said more about Grice's ideas systematically and historically. Historically, Grice was tremendously influential, but systematically considered something of a side-show. Grice had the deep and fruitful idea that much of the time the linguistic nature of discourse can be understood in terms of the demands of the rationality of our linguistic enterprise. This is an excellent idea but it is not and never was a monopoly of Grice's. On the contrary, the entire mathematical theory of games can be seen as a systematic study of strategic rationality. This rationality need not be a competitive one, for a rational strategy can be a strategy of cooperation.

Game-theoretical semantics came about when the basic concepts (ideas rather than results) of von Neumann's game theory were applied to Wittgenstein's "language games"; Grice never used game theoretical concepts and ideas in a similar way.

This is not a matter of scholarly priority or other kind of historical questions. It left Grice's work unnecessarily unsharp logically. For instance one thing that the mathematical game theory brings forcefully to light is the precise dependence of rational strategy reflection on the so-called utilities of the players, which roughly speaking means the purpose of the entire "game". In his "conversational maxims" Grice seems to assume that the purpose of the conversation is in some rather loose sense information-conveyance. But this is not the only possibility. For a historically (and systematically) important example of the conversation were a question-answer dialogue. For instance, in such an application, Grice's maxims of quantity would have to include criteria as to when a response to a question qualifies as an answer.

Larger-scale perspectives are in fact at issue here. I have suggested that the basic rules of logic are rules of model construction, so that a total failure to carry out a model construction for $(A \,\&\, \sim B)$ amounts to a proof of the implication $(A \rightarrow B)$. By the same token, if the very same logical construction rules do not lead to a dead end, we ipso facto have a demonstration of how a proposition and the state of affairs it expresses are possible. Once we see this we realize that much – most? – actual use of logic in reasoning is not inferential, but reasoning calculated to show how something is possible. By not attending to this use of logical rules most logicians (and practically all authors of logic) textbooks – in effect overlook one half (at least) of the actual uses of logic. If you do not first see it, consider the reasoning of a Sherlock Holmes. He does not, in using what he calls logic and deductions, try to show that a crime was inevitable (on assumptions), but how it could have happened. This is even more obvious in the argumentation of a defense attorney. He or she does not have to prove the innocence of the accused person, only to present a plausible enough an account of how things could have happened compatible with the innocence of the client so as to make a reasonable doubt of guilt possible.

Of all the language theorists that I have known, Grice struck me as best attuned to the nuances and overtones of English language. That sensitivity non-the-less does not compensate for the lack of sharp systematic logical theory in his work.

In general Pietarinen's reader might profit from a clearer distinction between different kinds of games with different "utilities", thus putting Pietarinen's paper in a broader systematic and historical perspective does not in any way reflect on its significance but rather enhances it.

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Comment on San Ginés

In her fascinating and ingenious paper, Aránzazu San Ginés deals with certain puzzling issues related to the notion of an arbitrary object. Now no one has ever seen, heard, touched or otherwise interacted with an arbitrary object. Talk about arbitrary objects is naturally taken to be but dramatization of certain features of certain logical and semantical theories, such as Kit Fine's theory of "reasoning with arbitrary objects". The present comment is not a natural occasion to discuss those theories at length, however. I believe they at the very best need further discussion, which would serve to present a better account of the puzzles that prompted the idea of an arbitrary object in the first place. Instead I am here presenting an independent systematic examination of some relevant problems.

The simplest puzzling context is an existential instantiation of a sentence-initial existential quantifier $(\exists x)$. The underlying idea is intuitive enough. One has reached in one's reasoning an existentially quantified proposition $(\exists x)\ E\ [x]$. Yet one may not know of any particular individual b such that $F\ [b]$. In order, to continue the argument, one can say (and think), "Since there are individuals satisfying $F\ [x]$, we can consider arbitrarily one of them and assume that its (his, her) name is b (here b is the instantiating term). This is like a judge in a court of law deciding to call an unknown suspect (or a litigant whose identity should not be known) "John Doe" or "Jane Roe". What such terms stand for might naturally be called "arbitrary (arbitrarily selected) objects".

Such steps of reasoning were acknowledged already in ancient Greek mathematics and logic under the term *ekthesis*. They are eminently natural,

but the justification and applicability of such inferential steps is not obvious. Indeed, this justification was the main task of Kant's philosophy of mathematics. (See my forthcoming paper "Kant's theory of mathematics – what theory of what mathematics?". The history of the *ekthesis* problematic is dealt with by Judson Webb in his contribution to the *Library of Living Philosophers* volume on Hintikka.)

But why restrict ourselves to sentence-initial existential quantifiers? The intuitive idea of an arbitrarily chosen individual exemplifying one's existential knowledge seems to apply to any existential quantifier, sentence-initial or not. Yet a straight-forward application of the "John Doe" idea leads to paradoxes. For instance, if $(\forall x)\,((\exists y)\,L\,(x, y)$ ("everybody loves someone") is so instantiated, we obtain a proposition of the form $(\forall x)\,L\,(x, b)$ which alleges that everybody loves one and the same individual b.

I will discuss the problem first in terms of an example different from those San Ginés uses. Consider the sentence

(1) There is someone such that if she cannot prove Riemann's hypothesis, no one can.

This seems to have the logical form

(2) $(\exists x)\,(\sim C(x) \supset \sim (\exists z)\,C(z))$

Furthermore (2) and hence (1) seem to be logically true. For if some genius *b* can prove the hypothesis, she can serve as the truth-making value *x*. If nobody can, anyone (say *d*) can serve as the value of *x* by making antecedent in (2) false.

But *d* cannot be the same person as *b*, for the former but not the latter can prove the famous hypothesis. Hence the value of the quantifier $(\exists x)$ in (2) cannot be thought of independently as a single object, arbitrary or not. Accordingly the "arbitrary object" idea does not seem to work.

However, this line of thought seems to work only because it violates an important but previously unrecognized feature of the semantics of quantifiers. In context like (2), propositional connectives like $\supset$ are independent of them.

Interpretationally, this is entirely natural. The independence in question means that in trying to construct alternative countermodels to a proposition, the different attempted models must have the same individuals (i.e. the same universe of discourse). This applies both to natural and to formal languages. Hence the true logical form of (1) could be expressed by

(3) $(\exists x)\,(\sim C(x)\,(\supset /(\exists x) \sim (\exists z)\,C(z))$

This is indeed how (1) is intended to be understood: as an assertion of the existence of someone such that she can prove the theorem independently of whether others can. And after the modification the "arbitrary object" idea obviously works as well as in other cases. Instead of a failure of this idea, we have found a flaw in the usual semantical representations of the meanings of quantified sentences. As one can easily see, it violates also Tarski-type truth definitions.

The entire subject matter of "arbitrary objects" should be discussed by reference to the semantical regularity we have here discovered. How would this apply to San Ginés' discussion? His examples include the following:

(4) Everyone ends up helping a problematic relative of a loved one. For John, it was Maria's.

This has the *prima facie* logical form

(5) $(\forall x)\,(\exists y)\,(((\exists z)\,H\,(x, y)\ \&\ R\,(z, y)\ \&\ P\,(z))\ \&\ ((x = John) \supset (y = Maria)$

But here the "arbitrary object" idea seems to fail in a different way. For the only value of the y in $(\exists y)$ is Maria and not any "arbitrary individual".

Yet the same insight that worked in the case of (1) helps us again, this time interpretationally rather than semantically. It suggests replacing the second & in (5) by $(\supset / \forall x)$. This does not affect the force of (5), but it has the effect of dividing the semantical game with (5) into two subgames. In a winning strategy (for the verifier) in the former we associate with each x an arbitrary individual y. In the second one, this y is compared for identity with the non-arbitrary individual Maria. In this way, the "arbitrary object" idea can be maintained.

How the explanatory strategy just explained is related to the ideas of Kit Fine and with the ideas of San Ginés is here left to be examined in further papers, hopefully by San Ginés herself. She already deserves credit for directing our attention to especially interesting semantical phenomena, which in turn have led us to a surprising and surprisingly general feature of the logical interplay of quantifiers and connectives.

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COMMENT ON SANDU

Gabriel Sandu's paper deals with an extremely important and promising development, the genesis (largely in his hands) of a new concept of probability. This is but another aspect of the larger change that replaces traditional first-order logic by IF logic, Tarski-type recursive semantics by game-theoretical semantics, the earlier axioms of set theory by purely logical principles, etc. By now it has turned out that the logic of quantifiers that mathematicians have used since nineteenth century is richer than Frege's logic or the usual first-order logic. It is essentially the so-called IF logic that Sandu and I launched around 1990.

A few years ago it was realized by Sandu that using IF logic rather than traditional first-order logic has a bargain: a new concept of probability. Gabriel Sandu's paper is about this new concept of probability. The importance of the new concept of probability is illustrated by a curious precedent. Even though John von Neumann created the most widely used mathematical formalism for quantum mechanics, the Hilbert space formalism, he did not think that it was the right one. [For this subject, see Rédei and Stöltzner (2001)]. And this was not a mere opinion; von Neumann spent a lot of time and effort to develop a better one. He never found one, but he outlined some basic desiderata for it. He realized that the new mathematical theory would have to be deeply different from the familiar ones, to the extent of requiring a new logic and a new concept of probability based directly on the new logic.

I suggest looking at IF logic and at the IF probability as a realization of the von Neumann project. In logic, von Neumann wanted a counterpart in logical space to the geometrical notion of orthogonality as distinguished from contradictory negation. The strong (non-truth-functional) notion of negation in IF logic serves this purpose.

Even though many things remain in the unpublished state of development, there are indications that the IF concept of probability does put things in a new light. For one thing, Bell's notorious inequality does not hold in IF probability calculus. This is only one of the many indications suggesting that the problem of entanglement in quantum mechanics disappears if IF probability is used instead of the traditional one.

In more general terms, the had probability that von Neumann envisaged was supposed to be purely logical. This would enable us to define such evident notions as entropy and information for more complicated and more realistic cases. For instance, entropy is easy to define for monadic languages, but if relations are involved it is not obvious what precisely the correct definition would have to be.

Some of the implications of IF probability can be seen by applying it to the constituent of distributive normal forms. A first-order proposition of quantificational depth d is logically equivalent with a finite disjunction of

constituents of the same depth d. Such constituents are mutually exclusive and collectively exhaustive. The probability of S or $P(S)$ is in the sum $\Sigma^{P}(c_i)$ of the probabilities of its constituents.

A constituent of depth d and with the free variables $y_1, y_2 \ldots y_e$ is a ramified list of all kinds of ramified sequences of individuals of length d that can be chosen from a given domain from the syntactic form of a constituent. Hence a constituent (d) of depth d and with $y_1, y_2 \ldots y_e$ as its free variables is of the form

$$\forall_i (\exists z)\, C_i^{(d-1)} [y_1, y_2, \ldots, y_e, z] \,\&\, (\forall z)\, \exists_i C_i^{(d-1)} [y_1, y_2, \ldots, y_e, z] \,\&\, C^{(o)} [y_1, y_2, \ldots, y_e]$$

Here $i \in I$, I being an arbitrary index set, and $C^{(o)} [y_1, y_2, \ldots, y_e]$ is a conjunction of negated and unnegated atomic formulas involving the free variables $y_1, y_2, \ldots, y_e]$.

All the draws of individuals (represented by existential quantifiers) are from the same pool of individuals in the individuals. In order to be coherent a constituent must satisfy certain obvious effectively verifiable consistency conditions. We may call such constituents coherent. They have several interesting properties.

First and foremost, a coherent constituent C can still be inconsistent. One way of establishing such inconsistency is to expend the constituent in question into a disjunction of deeper constituents. If C is inconsistent, eventually all of them become incoherent.

The most remarkable fact here is that if any coherent constituent C is interpreted as an IF proposition (consistent or not in the received first-order logic), it becomes consistent. It can have a model in which it is not false, even though it is not true either.

It follows that the probabilities of coherent constituents are the building-blocks of IF probability. Since they have been used in analyzing such philosophical and other theoretical questions such as the nature of information, especially surface information and depth information and logical omniscience, IF probability becomes an excellent tool of conceptual analysis. For one example, information is generally taken to be a mirror image of probability. But of which probability? IF probability is arguably the best candidate for this role.

Even on a technical level constituents offer a useful approach to probability and related notions. This is not accidental. Constituents are structurally a kind of map of the semantical games playable with them. A play of a semantical game with a constituent traces (through the players' choices) one branch of the tree structure that a constituent is. A strategy for one of the players tells that player which immediately higher node to choose when the

play has reached that node, strictly in the case of pure strategies and probabilistically of mixed strategies are allowed.

Thus mathematically the sets of strategies available to the two players are previously the same which make the application of the basic ideas of game theory easy. What is especially interesting is that constituents offer a natural way of defining the a priori probability version of different choices of constituents. What one naturally does is to use the relative number of winning (for that player) branches passing through the node in question. I suspect that this is how von Neumann thought his logic based probabilities to be determined by that logic. It follows that we can define equally natural a priori notions of information, entropy, etc.

An intriguing conceptual structure arises when two variables x, y depend mutually on each other, so tightly that one of them cannot be represented as a separate function of a third variable, as a

$$x = f(t, y) \qquad y = g(t, x)$$

It looks as if this kind of situation is impossible. For if so, we could substitute, obtain equations in terms of x and t or

$$x = f(t, g(t, x)) \qquad y = g(t, f(t, y))$$

When we solve these equations for x and y, respectively, we do obtain non-mutual dependencies of the form

$$(x = h(t) \qquad y = i(t))$$

Yet irreducible mutual dependence is found in quantum theory and also in choice theory, when the optimal choices of two agents depend mutually on each other.

The solution is to allow the dependence between x and y to be probabilistic. Then IF probability becomes an invaluable tool of conceptual analysis.

Gabriel Sandu has in fact applied IF probability successfully to game-theoretical situations. The purpose of this comment is to call my readers' attention to other applications.

References

RÉDEI, M. and STÖLTZNER, M. (eds.), (2001), *John von Neumann and the Foundations of Quantum Physics*, Dordrecht, Kluwer.