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INTERVIEW WITH HANS KAMP*

Alice ter Meulen and Klaus von Heusinger

AtM: First on your background: you grew up in the Netherlands, where you got your bachelors degree in physics in Leiden and studied logic with Evert Beth in Amsterdam.

HK: When I went to Leiden University in 1958, I wanted to study theoretical physics. As the completion of my undergraduate studies got nearer, I felt an urge to do something different for a year before disappearing behind the doors of the Institute for Theoretical Physics. Dick de Jongh, who was also studying mathematics and physics in Leiden, had told me about Professor Evert Beth who had founded his Institute for Formal Logic and the Foundations of Science at the University of Amsterdam just a couple of years before. Dick himself had decided to go there to do a Masters degree and then, eventually, a Ph.D. Dick whetted my appetite so much that we ended up going to Amsterdam together. As far as I was concerned, this was just a kind of time-out to explore a world of science I might never belong to in earnest. But, as the year at Beth's Institute progressed, I was offered a student assistantship and became increasingly integrated into the research group Beth had established. So the temptation to stay and make formal logic and its applications the subject of my further academic work became harder and harder to resist. In the end I gave in and decided to stay.

During those years in Amsterdam I developed a strong interest in the relation between logic and language, one of Beth's many interests. Open as he was to new developments, he organized, during my second year, a seminar on Noam Chomsky's book ***Syntactic Structures***, which had just appeared, but was already widely hailed as heralding a completely different way of looking at grammatical structure.[1] Young linguists also participated, among them Albert Kraak, Wim Klooster, Pieter Seuren, Hugo Brand Corstius, some

* This text derives from a prolonged, in-depth interview with Hans Kamp that took place in the Azores, Stuttgart and Köln. The transcription of the audiotapes was edited, extensively rewritten and often expanded in various ways. We'd like to acknowledge our gratitude for help with its content and form from Barbara Partee, Antje Roßdeutscher, Dana Scott, Blanca and Emily Kamp, Malte Willer, Daniel Altshuler and Tillmann Pross.

[1] Chomsky, Noam, 1957, ***Syntactic Structures***. Mouton, The Hague.

of whom had been told by their more established colleagues not even to consider Chomsky's new work (or else ...), so we were all sworn to some kind of secrecy as to their presence. For me this was an early opportunity, rare in Europe in those days, to become familiar with the basic ideas of generative grammar. Beth did not only have an abiding interest in the relation between formal logic and natural language, but also in its relation to computation and thought. These interests came together in the project for which Beth had been able to find a sponsor in Euratom, the European joint project for the development of peaceful uses of nuclear energy. The project's aim was to explore the possibilities of using Beth's Semantic Tableau Method—then still quite new—as the basis for the development of automated systems capable of effective and psychologically plausible logical reasoning. The computer was to find proofs for deductively valid inferences using the tableau-based deduction algorithms that the group was to design and make available in machine-readable format. Being part of that group was an important factor that kept me in Amsterdam, although I don't think I ever contributed anything to this project. But I didn't work any less hard than the others. The project provided a wonderful opportunity to be part of a larger unit with a clear scientific purpose and a strongly felt, joint commitment. Besides, it provided a certain material comfort.

During my second year in Amsterdam I met Richard Montague, who had chosen to spend part of his sabbatical year there. (The other part was spent with Andrzej Mostowski in Warsaw.) The reason why I got to know Montague better than I otherwise would have had little to do with logic. Besides a superb logician, Montague was also a very accomplished organist. Someone at my father's office happened to be a relative of the verger of the St. Laurens church in Alkmaar, the site of one of Holland's most famous organs, at that time already used for widely hailed recordings by highly regarded organists. Since it was within the verger's discretion to decide who would be admitted to the organ outside regular services, it was possible to arrange for Montague to come along to Alkmaar and try it out. The event was a very big thing for me and I think it also meant something to him. In any case, he did remember me when I wrote to him a couple of years later, which led to my being accepted eventually as a Ph.D. student in the Philosophy Department of UCLA, where Montague was a professor.

I got my Masters in Philosophy and Logic from Beth's Institute about a year and a half after Beth had died—he died during my third year in Amsterdam. The topic of my master's thesis was a paper by Alfred Tarski on the calculus of formal, deductively closed systems, ***The Calculus of Deductive Systems***, in which he develops a type of algebra whose points are deductive theories from some formal language and whose operations are complement, union and intersection of deductive theories.[2] These algebras have many of the properties of Boolean algebras, but only some of them are Boolean. More precisely, they are Boolean if and only if the formal language that provides the points of the algebra is sufficiently simple, for instance, a language of classical propositional logic with a finite number of atomic sentences. One of the interesting challenges for this approach to the deductive structure of formal languages is to determine how the properties of the languages generating the algebras manifest themselves as properties of the algebras they generate. Tarski wrote this paper in the nineteen thirties in his native Poland, but he did not include any proofs. He had a hard time getting his work published—he once told me that in the twenties and thirties, when Poland made stellar contributions to mathematical and philosophical logic, the prevalent anti-Semitism made it hard for Jewish scholars to become successful academics and that publishing in top journals was one of the difficulties someone like him had to cope with. In the case of this paper he was eventually given just enough space to state the definitions and theorems, but had to suppress all the proofs. So what I did, partly following a suggestion from my advisor and partly because I really wanted to understand the paper, was to figure out these proofs. My masters thesis was really nothing more than a list of proofs for all the theorems of the paper with a little bit of commentary. It certainly wasn't a very original piece of work; but doing it was very useful for me. And it confirmed Tarski as my hero-of-logic Number 1. (He shared first place with Gödel, who, I still regret to this day, I never met in any way or even set eyes on.)

In my second year in Amsterdam Beth asked me who I wanted to be supervised by during my studies in Amsterdam. I chose Karel de Bouvère, because I wanted to work mostly on model-theory and that was what de

[2] Tarski, Alfred, 1983, ***Logic, Semantics, Metamathematics***, 2nd edn., edited by John Corcoran, Hackett Publishing Company, Indianapolis, IN.

Tarski, Alfred, 1930, Fundamentale Begriffe der Methodologie der deduktiven Wissenschaften I, ***Monatshefte für Mathematik und Physik***, 37, 361–404. English translation under the title 'Fundamental concepts of the methodology of the deductive sciences' in: Tarski 1983, 60–109.

Tarski, Alfred, 1935, Grundzüge der Systemenkalküls. Erster Teil, ***Fundamenta Mathematicae***, 25, 503–526. English translation by J.H. Woodger under the title 'Foundations of the calculus of systems' in: Tarski 1983, 342–383.

Tarski, Alfred, 1936, Grundzüge der Systemenkalküls. Zweiter Teil, ***Fundamenta Mathematicae***, 26, 283–301. English translation by J.H. Woodger under the title: 'Foundations of the calculus of systems' in: Tarski 1983, 342–383.

Bouvère did, whereas model-theory had become less prominent in Beth's own work. When my masters degree came in sight and I had to make a decision whether to go on to do a Ph.D., I decided to look for a place as Ph.D. student in one of the California strongholds of mathematical logic. My reason was, apart from a sense of adventure, that I had made up my mind to work on the interface of model-theory and proof-theory to see if Beth's method of Semantic Tableaux could be used to define new decidable fragments of predicate logic. I felt that, after Beth's death, nobody in Amsterdam was an optimal Ph.D. supervisor on that topic.

My time at UCLA (1965–1968) was, both by the standards of today and by those at the time, quite pampered. I received a University Fellowship, which liberated me from any TA work, so that I could devote myself entirely to my dissertation. However, I did have to take courses, some of which later proved invaluable to my further career and development. Crucial input that I received in this way, which had an immediate effect on my work, came from Arthur Prior, who was at UCLA as Visiting Professor in the Philosophy Department during my very first semester. Prior was offering an upper division course on Tense Logic, covering most of the material that appeared not long afterwards in his book ***Past, Present and Future***.[3] Priorean Tense Logic—the system obtained by extending classical propositional logic with the two tense operators ***P*** ('it was the case that') and ***F*** ('it will be the case that')—and the many variants of it that were formulated by Prior himself and by others subsequently, is purely 'topological' in that it can only express non-metrical temporal relations such as temporal precedence or the relation of temporal overlap. Prior raised the question whether it was possible to use such topological systems also as a basis for the expression of metrical relations, e.g. relations having to do with *how long p* was the case before *q* was the case. This problem became the starting point for what was to become my dissertation. I decided at that point, some time in the Fall of 1965, to shelve the project with which I had come to UCLA—that of using Semantic Tableaux to try and discover new decidable fragments of classical predicate logic, which at that point had not led to any significant results—a topic that, to my regret, I have never quite managed to get back to. The main result of my dissertation was that, if you want to express some form of temporal metric within a topological calculus like Prior's (***P***, ***F***)-calculus, you need a logical system with more powerful operators. The 2-place propositional operators ***S***(***ince***) and ***U***(***ntil***) not only made it possible to express

[3] Prior, Arthur, 1967, ***Past, Present and Future***, Oxford University Press, Oxford.

metric relations, it also proved to be in an important sense 'optimal' in that—provided the temporal order is a linear, order-complete relation—they make available all that a topological tense logic could possibly give you: Any tense operator that can be defined semantically can also be expressed by a formula of the (***S***, ***U***)-calculus. The (***S***, ***U***)-calculus eventually found a place within Computer Science, first as an ingredient in certain methods of program verification and, more recently, as a building block in specification languages in chip design. But honesty obliges me to stress that these applications were entirely unforeseen and that they are utterly different from what motivated the development of the calculus at the time. Prior thought that the tenses of the verb should formally be analyzed as modal operators. He and others took their inspiration from the formal work in modal logic that, in the wake of Saul Kripke's achievements of the late fifties and early sixties, had reached a remarkable degree of popularity and sophistication. The philosophical drive behind this work was the widespread perception that, even if modality might have something to do with truth in possible worlds, the modal expressions that are found in natural languages should not be construed as involving reference to possible worlds. If possible worlds were to play any part in the analysis of such expressions, they should do so only indirectly and implicitly, not as part of the logical forms of those expressions, but only in the meta-language, in which their semantics can be given a model-theoretic explication. Prior and others not only held such a view with respect to modality, but also with respect to temporal reference. The tenses, they argued, involve no more explicit reference to times than modal adverbs like *necessarily* or *possibly* involve explicit reference to worlds. Hence there should be a way of explicating the role time plays in our understanding of those expressions without construing them as explicitly referring to or quantifying over times. And, moreover—and a good deal less plausibly—this point of view was then extended to also cover other kinds of temporal expressions, such as calendar terms and temporal adverbials. Priorean Tense Logic was meant to accomplish this. What matters most for the present story is that Prior thought of Tense Logic as relevant to the analysis of natural language and that I saw the work of my dissertation in that same spirit—as part of a general project of providing logical analyses of linguistic meaning. That temporal expressions of natural languages do not refer to times any more than *necessary* and *possible* refer to possible worlds was a central motive behind my dissertation no less than I took it to be a central motive for Prior. And the fact that words like *since* and *until*, with their apparent operator-like syntax, are as expressively powerful as the formal results of the thesis seemed to establish carried a great temptation: I was only too eager to interpret it as evidence that Tense Logic gave us the right means to explain how natural languages generally handle time. It took me much of the following decade to persuade myself that this simply wasn't right: there is no justification for such a nominalistic attitude towards temporal reference in natural language. For me the most obvious reason for this was that from a linguistic point of view expressions like (*at*) *ten minutes to five* can lay as much claim to referring to times as noun phrases like *Arthur Prior*, *The University of California at Los Angeles* or *the father of Tense Logic* can lay claim to referring to people and institutions. But there are a good many other reasons as well.

AtM: Are there other people you met during those years at UCLA who you feel had an important influence on your work or intellectual development?

HK: One of them was Alfred Tarski. Tarski came to UCLA as a visiting professor for (I believe) the Spring Semester of 1967. That was the first time I met him. I got to know him personally then, up to a certain point, partly through his Ph.D. student Judith Ng, who came with him during his semester at UCLA. But I had already come to see Tarski as one of the fathers of modern logic during my years in Amsterdam and that he has always remained. I still remember that in the class he was offering that semester at UCLA he also discussed the Calculus of Deductive Systems. That gave me a sense that the work I had been doing on my masters thesis in Amsterdam hadn't been a complete waste. I did mention to him once that I had spent some time with that paper in Amsterdam and it was then that he told me about the difficulties he had encountered back home before he escaped to the US.

But there were many other outstanding logicians at UCLA at the time, in particular C.C. Chang and Yanis Moschovakis in mathematics and David Kaplan and David Lewis in philosophy. Kaplan was away for most of the time I was at UCLA as a graduate student and I got to know him better only later. Another person, for whom I had already formed a great admiration before I got to UCLA, was Dana Scott. At that time Scott taught at Stanford, but he would occasionally come down to UCLA, partly in order to work with Montague on a monograph on set theory by Montague, Scott and Tarski, that had been in the making for some time, and was nearing completion then.

AtM: But your Ph.D. advisor was Montague?

HK: Yes, Montague was my advisor. As a point of fact, I wrote the dissertation essentially on my own. I had even had some difficulty in getting Montague to sit down to read my results before I got the dissertation typed and submitted.

But if that suggests I was one of those graduate students who are ignored or neglected by their advisors, nothing could be further from the truth. As a matter of fact, I was immensely lucky and privileged in the kind of tutelage I did get from Montague. The form it took had to do with his often wanting somebody around when he did his own work, as a kind of sounding board or sparring partner. During most of the three years I was at UCLA I happened to be the only person whom he considered suitable for this role. In particular, during 1966 and 1967 I was present for quite a bit of the time in which he developed the ideas that can be found in some of the papers that were collected in ***Formal Philosophy*** after his death.[4] That gave me a unique opportunity to see from up close how new ideas emerge, how they develop and mature, and what can and should be done by way of moulding them into a form that makes them not only useful to oneself, but also capable of being communicated to others. To witness how some of these papers took shape—I am thinking here in particular of: *On the Nature of Certain Philosophical Entities*, *English as a Formal Language* and *Pragmatics and Intensional Logic*—and to be able to watch Montague's truly extraordinary ability to combine penetrating intuitions with an uncanny gift for formalization was the best graduate education anyone could ever have had. I am immensely grateful to this day that my thesis supervision took this particular form.

AtM: Alonzo Church was there at UCLA. How did the development of lambda calculus influence Montague's work?

HK: Church came to UCLA in the fall of 1966, upon his retirement from Princeton. UCLA had offered him an attractive deal, to set up an infrastructure that would enable him to continue his work as editor of the review section of the ***Journal of Symbolic Logic*** (JSL). It made it possible for him to accommodate his archives as well as much of his own personal papers. In those days, when there wasn't yet any kind of electronic support for archiving and data collecting, that was a *sine qua non* for the continuation of the review section work. Without this, I doubt very much that Church would have come to UCLA. He had devoted a good part of his life to the JSL's review section, work he considered extremely important to a new field, which Mathematical Logic certainly was when he first took the review

[4] Montague, Richard, 1974, ***Formal Philosophy: Selected Papers by Richard Montague***, edited and with an introduction by Richmond Thomason, Yale University Press, New Haven, CT.

section under his wings and certainly still was in the sixties, especially when compared with other, well-established branches of mathematics like number theory, analysis, geometry, topology or abstract algebra.

But, as far as I remember, there was little, if any, connection between Church's coming to UCLA and the influence of the lambda calculus on Montague's work. Of course, the presence of the lambda calculus in Montague's work is self-evident. Montague's Higher-order Intensional Logic (HOIL) is an intensional system of the typed lambda calculus. But Montague was closely familiar with Church's work on the lambda calculus well before HOIL was given its final form and used in the linguistic and philosophical applications that we know from the papers in ***Formal Philosophy***. When I came to UCLA, the lambda calculus was a prominent focus for Montague in two quite distinct contexts. On the one hand, there was the question of the consistency of the untyped lambda calculus, a problem that was solved by Scott in 1969. This was a problem in which Montague too had taken an acute interest, as it was closely related to his own work on set theory and on at least superficially similar problems there, such as the consistency of Quine's system known as ***New Foundations***.[5]

On the other hand, in the mid-sixties Montague was in search of a formal system that could serve as a general formal framework for addressing philosophical questions, including the formulation of model-theoretic semantics for fragments of natural languages. Here it was important to build on the typed lambda calculus—the possibility to refer to higher-order and not just to first-order entities was an essential desideratum for such a general framework. But another desideratum that Montague had come to see as essential was that the system admit of forms of non-extensionality—in this regard Montague was part of the modal logic revolution within philosophy that is now mostly identified with Kripke's work, but to which Montague himself had also been one of the early contributors.

This constituted perhaps the most important opposition between the general perspective represented by him and his colleagues in Southern California, on the one hand, and the persisting extensional commitments of Quine and his followers, on the other hand. Montague saw HOIL as an optimal answer to the question what a formally precise, consistent and practically viable logic of intensionality ought to be like. The importance he

[5] Quine, Willard van Orman, 1937, New foundations for mathematical logic, ***American Mathematical Monthly***, 44, 70–80. Reprinted in: Willard van Orman Quine, ***From a Logical Point of View***, 2nd rev. edn. Harvard University Press, 1980, Cambridge, 80–101.

attached to HOIL is illustrated by the following remark I still vividly remember him making to me at one time in 1966 or 1967: “I always thought until now that the right framework for doing philosophy properly was Zermelo-Fraenkel set theory—I now realize it is higher-order intensional logic.”

No one intending to develop a non-extensional logic could have ignored Church’s contributions to this, his work on the ***Logic of Sense and Denotation***, in which he tried to formalize the ideas that Frege had formulated about sense and reference and the relations between them in ***Über Sinn und Bedeutung*** and other writings.[6,7] All versions of Church’s ***Logic of Sense and Denotation*** are very complex. A measure of the complexity of Church’s original formulation of the system is that even someone of his extraordinary intellectual powers and his proverbial carefulness could have overlooked an essential flaw in it. It collapses into a purely extensional system, which is, of course, precisely what it was meant not to do.

Montague had played an active role as member of the doctoral committee of David Kaplan, whose dissertation was about the ***Logic of Sense and Denotation***. I suspect that some of the decisive ideas about HOIL may have taken hold at that time, as a result of the intensive interactions between him and Kaplan. But, as far as I remember, it wasn’t the interaction with Church himself that was immediately relevant to HOIL’s final formulation. Of direct importance to that formulation was a lively correspondence in 1965/66 between Montague, Kaplan, Scott and John Lemmon, mostly over the handling of constants and variables (both first- and higher-order) within such a non-extensional system. I very much doubt that Montague consulted Church, in correspondence or in person, over the exact formulation of HOIL. In retrospect this lack of contact between two members of the same department, who were aware of the closeness of their interests, may seem odd, especially if they respected each other as much as I know Montague respected Church. I suppose Church must have had a high respect for Montague as well, though this is just an assumption. But the explanation might well be that Church was a very private person. He would not be one to go to someone as much his junior as Montague for a chat or for some informal

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[6] Church, Alonzo, 1951, A formulation of the logic of sense and denotation, in: Paul Henle, Horace M. Kallen and Susanne K. Langer (eds.), ***Structure, Method, and Meaning: Essays in Honor of Henry M. Sheffer***, Liberal Arts Press, New York, 3–24.

[7] Frege, Gottlob, 1892, Über Sinn und Bedeutung, ***Zeitschrift für Philosophie und philosophische Kritik***, NF 100, 25–50. Translated by M. Black under the title ‘On sense and reference’ in: Peter Geach and Max Black (eds.), ***Translations from the Philosophical Writings of Gottlob Frege***, 2nd edn., Oxford, 1960, 56–78.

advice. And on the other hand, but by much the same token, he wasn't either the sort of person that one would feel comfortable approaching about something, unless one had worked out what one wanted his advice on in pretty much all relevant detail.

I mentioned that Montague saw HOIL as a new formal framework for addressing philosophical questions and that questions of natural language semantics were just one item on his much longer agenda. Even so it is probably right to say that it has been through its applications to natural language semantics—the application that became known as Montague Grammar—that HOIL has become most widely known and used. Since Montague Grammar was, almost from the time it became established through Montague's seminal contributions to natural language semantics, the concern of linguists and only marginally of philosophers, HOIL (and some of its variants like Gallin's Ty2), are much better known in linguistics than they are in philosophy.[8] This is one reason why Montague Grammar stands out among the possible and actual applications of HOIL and it is a reason for dwelling on this one of Montague's accomplishments a little longer.

AtM: A sideline: the richer notion of an individual concept was already in Carnap's ***Logische Aufbau***, right?[9] How did that work tie into Montague's HOIL?

HK: The notion also played a role in Carnap's ***Meaning and Necessity***.[10] Carnap's work was a very important input to what was going on at the time when Montague and Carnap were both on Kaplan's dissertation committee and later on, when HOIL was honed into final shape. I do not know how much interaction there was between Montague and Carnap at that time. Carnap was the official advisor of Kaplan—but Montague once told me that he did most of the actual advising. How much actual personal interaction there was between Carnap and Montague in the early sixties or before I do not know. My impression is that there wasn't much, probably due to similar barriers that I suggested earlier may have limited direct interactions between Montague and Church. By the time I came to UCLA Carnap was retired and lived a quite secluded life, partly because of his failing eyesight.

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[8] Gallin, Daniel, 1975, ***Intensional and Higher-Order Modal Logic***, North-Holland Publishing Company, Amsterdam.

[9] Carnap, Rudolf, 1928, ***Der logische Aufbau der Welt***, Weltkreis-Verlag, Berlin-Schlachtensee, Neuaufl. Meiner, Hamburg, 1998.

[10] Carnap, Rudolf, 1947, ***Meaning and Necessity: A Study in Semantics and Modal Logic***, The University of Chicago Press, Chicago, IL.

As regards individual concepts, yes, these did play a crucial part in the discussions of what would be the best way to define a non-extensional type-theoretical system. One feature of HOIL that is anything but self-evident against the background of extensional systems like ordinary predicate logic or the extensional typed lambda calculus is the different treatment of variables and constants. This is a difference that in HOIL holds at all levels, but it is easiest to explain in relation to individual constants and variables. Individual constants behave in such a way that they can be used as designators of (non-rigid) individual concepts such as *the US president*. They can be used that way because the semantic value of a constant in an intensional model can vary from world to world. Variables, on the other hand, are assigned values independently of worlds; that is, variable assignments are defined in such a way that they assign individuals to individual variables, and not individual concepts. To handle variables and concepts in this way constitutes one from among several options that present themselves when we pass from an extensional to a non-extensional system. It is now common place that the option Montague chose is a viable one, which among other things is compatible with a uniform assignment of types to argument slots. For instance, a 1-place predicate P of individuals has the type $\langle e, t\rangle$; both '$P(v)$', where v is an individual variable, and '$P(c)$' where c is an individual constant, are well-formed formulas and their semantics works out as intended. But at the time this wasn't obvious—not until one had seen things spelled out and been able to verify that it was actually possible to proceed in this way. Whether the choice made by HOIL is the conceptually right one is, of course, another question. That question is connected with another, truly fundamental question: Is intensionality the right kind of non-extensionality, and, more specifically, is it the kind of non-extensionality we find in the languages we speak? In a way the answer to that question was already known at the time when HOIL was given its definitive characterization: it is 'No, not in general'—a point made at the time in a review *Is Necessity the Mother of Intension?* of some of Montague's work by Fred M. Katz and Jerrold J. Katz.[11] I thought then, as I do now, that this review was unsympathetic beyond the call of duty. But its title was spot on. The difficulty it points at is that intensionality isn't the right instrument for dealing with the propositional attitudes, because its inability to deal with the problems of logical omniscience—an inadequacy of Intensional Logic that

[11] Katz, Fred and Jerrold Katz, 1977, Is necessity the mother of intension?, ***The Philosophical Review***, 86(1), 70–96.

strictly speaking vitiates all its applications to the attitudes and the semantics of attitude reports. The merit of Church's ***Logic of Sense and Denotation***—in all its consistent versions—is that it contains safeguards against this intensional oversimplification. But the price, as we noted earlier, is that the system is very difficult to use in applications. Because of this, HOIL is at a significant advantage even there where it is strictly speaking wrong to apply it. Here too natural language semantics has adopted a policy of closing an eye to the snakes in the grass; for otherwise it would be very hard to discern the insights that an intensional analysis *can* give, even in cases where the grass isn't snake-free.

In the Fall of 1966 Montague gave a seminar on the application of logic to the semantics of natural language. From the start the perspective that informed what he presented in the seminar was the one epitomized in his often quoted remark that 'there are no fundamental differences between natural languages and the artificial languages of formal logic'. After nearly half a century of intensive work on the syntax and semantics of natural language much of which would not have been possible without his contributions, it is easy enough for us today to come up with a whole catalogue of differences between natural languages and the formal languages that Montague must have been thinking of then—enough, it would seem, to hold Montague's slogan up to ridicule. What we should not forget is how bold and fruitful this position was in those days, and to make good on that position in the way that Montague did. The common attitude among logicians and logically minded philosophers was that natural languages are ever so many Heath Robinson machines, which we somehow manage to make do the things that we need them for, but which only hang together through endless numbers of *ad hoc* devices, without rhyme or reason and that look so fragile when we observe them through the glasses of a formal logician that it is a lasting mystery why they do not break down more often than they do. I mention the seminar because it was in its course that what was to become Montague's own form of Montague Grammar took on more specific contours. A crucial occasion that I still remember clearly was when one of the seminar participants—Bob Mattison, a graduate student from Berkeley who had come to UCLA to work with Montague and who had become closely integrated in the group of devotees that attended the graduate seminars Montague offered—had unearthed one of the papers on categorial grammar by Adjukiewicz, quite hard to get hold of then, and brought it to the attention of the seminar. To my knowledge this is how Montague became aware of this work on categorial grammar. After that the seminar took a clear turn, in the direction of Montague Grammar as we know it, in which semantic values of complex linguistic expressions are described as compositionally determined by the semantic values of their smallest meaningful parts (that is, of the words and the semantically contributing morphemes from which they are made up) via a series of immediate dependencies that are made visible by the expression's syntactic tree. Montague recognized right away the value of Adjukiewicz-type grammars for the project of giving a compositional account of the form-meaning relation—because the syntactic analyses they assign to complex expressions make the compositional relation between syntax and semantics especially transparent.

AtM: In the formal work in natural language in the fifties and sixties, the two figures that stand out are Chomsky and Montague. In certain respects their aims can be seen as quite similar. Yet, there seems to have been little contact between the two and, in fact, not much by way of mutual appreciation. Why do you think this was so?

HK: The lack of mutual interest between Chomsky and Montague—to the extent that there was any perception of the other's achievements, it seems to have been largely negative—may seem strange in one way, since they were motivated by the same deep insight and concern: that natural languages, for reasons that we now find commonplace, but that weren't at the time, were subject to far greater systematicity than had been assumed until then or at least than anyone had been prepared to openly assert. The relation between form and meaning, they both saw clearly and posited emphatically, had to be essentially lawlike, and for natural languages this had to be in some important sense by 'design'—just as for formal languages, even if the designing and the designers were different.

But for Montague, coming from formal logic and, even more importantly, from the model-theoretic school of Tarski, where it is the received view that the conceptually transparent way of defining correct inference and entailment should be given in model-theoretic terms—a theory of form alone could not possibly suffice by itself. Logical form, whether close to or distant from surface form, is only (more) form, and therefore cannot provide an explanation of the form-meaning relation, unless it is backed up by an account of the semantics of the logical form language in model-theoretic terms.

Closely connected with this was a firm anti-psychologistic methodology. The only trustworthy evidence for or against a theory of the form-meaning relation for natural languages was supposed to be given by speakers' judgments about entailment relations between sentences. Exactly how a theory should account for the positive and negative entailment pairs that can be ascertained in this way for a given language on the basis of speakers' judgments is pretty much a matter of what works best. And Montague would not have wanted to draw any conclusions from the details of a theory that describes a language successfully according to this criterion to what is actually going on in the minds of speakers that produce or interpret the expressions of that language and come up with their judgements of what follows from what. He did allow for the relevance of syntactic simplicity or its faithfulness to basic syntactic intuitions. For instance, he thought the syntactic structures that such a theory assigns to the sentence of a given fragment of English should be close to our central intuitions about English sentence structure. But as far as I can recall, Montague wanted to see those judgments as the only points of contact between his theoretical descriptions of the semantics of natural language fragments and the reality of how those languages are actually used and understood.

For Chomsky such considerations about giving a separate account of the semantics and/or logic of 'logical forms'—i.e. those forms that the theory presents as the direct harbingers of meaning—never seem to have had any purpose. This is not because he saw intuitions about what follows from what as irrelevant in general, but because he thought they could not tell us much about that specific aspect of human cognition that enables us to acquire and use a human language. Too many other cognitive factors enter into logical and semantic judgments and into the cognitive processes that are the substance of such judgments to allow any conclusions vis-à-vis the language capacity as such, as distinct from all the other cognitive modules that are presumably involved.

None of this explains why Montague should not have adopted Chomsky's proposals for the theory of grammar for his purposes, i.e. as the syntactic basis for a model-theoretic account of semantics. But Montague felt very strongly that the theory of grammar that Chomsky proposed at the time—the Extended Standard Theory of ***Aspects of the Theory of Syntax***—lacked the formal rigor he needed as syntactic basis for his model-theoretic definitions of semantic values.[12] I have sometimes speculated whether Montague's attitudes might have changed had he lived longer. But I am not sure. By the time he died there already seems to have been a kind of entrenchment; quite a bit would have had to change somewhere along the line.

[12] Chomsky, Noam, 1965, ***Aspects of the Theory of Syntax***, MIT Press, Cambridge, MA.

AtM: Was the fragment strategy suggested by the need to avoid circularity, having no truth predicate and hence no threat of paradoxes?

HK: No, I don't think that was the real reason. The basic reason is very simple. If you want to describe a language in the rigorous way that Montague wanted there is little else you can do, short of giving a rigorous treatment of the syntax and semantics of the language as a whole, with all its different constructions and its entire vocabulary. Though I guess that too would count as a fragment—an 'improper' fragment, just as a set is an improper subset of itself. But it is one that is just too big to handle to start with, or for that matter, whenever you want to explain how some part of the language really works. Giving such a complete account for a language in its entirety is unattainable even today. It was totally out of the question at the time when the enterprise was just getting under way, and as a way of showing how such a theory could be made to work, it would probably have been the wrong thing to try, even had it been a realistic possibility. And besides, it isn't even clear that the notion of a human language in its entirety is well-defined.

It is true that most work today, both in syntax and in formal semantics, looks like it has abandoned the principle that all that we can do is to describe the syntax, or the syntax and semantics, of certain fragments of the languages we study. What such studies typically present are analyses of individual words, morphemes or constructions, which take the form of particular syntactic or syntactic + semantic principles. The implicit assumption behind such proposals is that those principles proposed in such individual studies can be integrated into a single comprehensive syntax, or a single comprehensive syntax-cum-semantics, for the language as a whole. The worry of those who complain about the failure of such studies to present explicitly stated grammars, consisting just of a syntax or of a syntax and semantics, for fragments that include the particular phenomena to which the study is devoted is that this implicit assumption may be illusory. Putting together a grammar which integrates all such principles, for a fragment that includes all these constructions, morphemes and words, is anything but a straightforward exercise. The ways in which the different principles interact can be just as intricate as the principles themselves and these interactions have as a rule not been considered by those who have stated and studied the principles involved. How tricky it can be to integrate rules that were designed to deal with particular constructions into a single system is something that is all too familiar to grammar developers in Computational Linguistics. I have heard and seen a little bit of this in connection with the LFG grammar and parser for German that have been developed over a number of years by a group of my colleague Christian Rohrer. One problem with such parsers and the grammars they implement is that unforeseen interactions create the possibility of assigning certain strings syntactic structures that they simply cannot have.

You also raise a question about the notion of truth: is it, or is it not, one of the tasks of a grammar for a natural language to specify under what conditions any declarative sentence of the language is true? This is an important and delicate issue—one whose importance and difficulty we have been aware of since the work by Gödel and Tarski in the nineteen thirties. Their results have taught us that you cannot have a consistent semantics, of the sort that nearly all current formal approaches to natural language semantics are trying to put into place, for a language that has certain expressive powers, and these powers are present almost before you know it—not only in human languages as they exist, but also in most reasonably comprehensive fragments of them. It is curious that this point is so rarely discussed in semanticist circles. It is part of our overall semantic project that the accounts we propose for certain parts of the language should eventually be put together into a semantic account for the language as a whole. But if that is the ultimate goal, then at some point some much more drastic adjustments will have to be made than many of us ever bother to articulate even to ourselves. Since natural languages do have the means to provide structural descriptions of their own expressions, and since they do have overt truth predicates, such as *is true, is a truth* and so on, they are among those languages for which a syntax and semantics of the sort we are trying to provide for them are impossible.

Interestingly it was Montague himself who, in his justly famous paper ***Syntactical Treatments of Modality, with Corollaries on Reflexion Principles and Finite Axiomatizability***, demonstrated that this problem goes deeper and affects many more expressions than just those that refer to truth.[13] All these results—Montague's as well as Tarski's and the many other semantic paradoxes that have been discussed since the early days of the 20-th century—go to show that in the syntactic and semantic descriptions of most and probably all human languages there comes a point where safeguards have to be put in place against the semantic paradoxes. There are

[13] Montague, Richard, 1963. Syntactical treatments of modality, with corollaries on reflexion principles and finite axiomatizability, ***Acta Philosophica Fennica*** 16, 153–167. Reprinted in: Richard Montague, ***Formal Philosophy: Selected Papers by Richard Montague***, edited and with an introduction by Richmond Thomason, Yale University Press, New Haven, CT, 1974, 286–302.

various proposals for how this might be achieved, for instance, via Property Theories of the sort proposed by Açzel, Turner, Chierchia and Fox, or by partial definitions of semantic notions, in the spirit of Kripke's ***Outline of a Theory of Truth*** or of the stabilization approach of Herzberger, Gupta and Belnap.[14]

AtM: Did Barbara Partee take part in Montague's seminar at UCLA at that time?

HK: No, she did not. During the years I spent as a graduate student at UCLA I met her only once, in my last year there. She was known as this very bright young linguist, from the first crop of graduate students who got their degree with Chomsky at MIT. Barbara had been invited to give a talk in the philosophy department; if I am right, that was in 1968 and the topic was negation. I believe that was the only time I saw her before I got my Ph.D. from UCLA and left. The collaboration between Montague and her started after that, somewhere between 1969 and when Montague died in 1971. So it was sadly short-lived. Joint advising was I believe an important part of it. I can think of only one case right now, viz. the advising of Michael Bennett, but for all I know there may have been others. But, of course, such cooperations could not have been enough by themselves to turn Barbara into the champion of Montague's method that she soon became. For Barbara the discovery of the formal approach to natural language semantics Montague had just put on the map must have been a pivotal experience, which proved to be decisive for her whole further career. Unless I am mistaken the term 'Montague Grammar' is due to her. In any case, if it hadn't been for her, it is quite certain that Montague would not have had the impact within linguistics he did have; and I am not sure whether he would have had more than a fleeting impact. The fact that Barbara was already established as one of the new leading generative syntacticians, and the persuasive arguments she was able to

14 Açzel, Peter, 1988, ***Non Well-founded Sets***, CSLI Publications, Stanford, CA.

Turner, Ray, 1987, A theory of properties, ***The Journal of Symbolic Logic***, 52(2), 455–472.

Chierchia, Gennaro, 2006, Broaden your views. Implicatures of domain widening and the "logicality" of language, ***Linguistic Inquiry***, 37(4), 535–590.

Fox, Danny, 2007, Free choice disjunction and the theory of scalar implicatures, in: Uli Sauerland and Penka Stateva (eds.), ***Presupposition and Implicature in Compositional Semantics***, New York, Palgrave Macmillan, 71–120.

Kripke, Saul, 1975, Outline of a theory of truth, ***Journal of Philosophy***, 72(19), 690–716.

Herzberger, Hans, 1982, Notes on naïve semantics, ***Journal of Philosophical Logic***, 11(1), 61–102

Gupta, Anil and Nuel Belnap, 1993, ***The Revision Theory of Truth***, MIT Press, Cambridge, MA.

give for why formal semantics was needed, were essential to its acceptance within a community where acquiring an operative mastery, or even just a passive mastery, of the formal tools used in MG was a non-trivial matter—then even more so than today.

AtM: How did Rich Thomason get involved with this?

HK: David Kaplan had asked him to take charge of the posthumous edition of the papers of Montague: ***Formal Philosophy***. I am not sure how much of an interest Thomason had at that point in Montague's work. He was more known as a logician with among other things an interest in modal logic and there is the well-known joint work with Robert Stalnaker on adverbs, which appeared at roughly the same time as ***Formal Philosophy***. The choice of Rich as editor was evidently a very good one. For one thing he got the collection out in a remarkably short period of time, especially if one keeps in mind that he cannot have been familiar with more than a few of the papers in it, when he was put in charge.

AtM: Linguists, and perhaps also many philosophers nowadays, tend to think of Montague as the creator of Montague Grammar. But does that image do justice to his own perception of the contributions he had made?

HK: You are right to ask that. In fact, one almost certainly should answer the question with 'no'. For most of his professional life Richard was a logician, with important contributions to his name in proof theory, set theory, the paradoxes (really an offshoot of his work on proof theory and on Gödel), recursion theory and model theory. His formulation of HOIL, another important contribution, came comparatively late in his short career, and it was only this that enabled him to do the work on natural language semantics. But even within the narrower confines of this part of his total oeuvre he himself seems to have thought of his work in natural language semantics as just one of the different possible purposes to which HOIL could be put. As things have turned out, there is only one paper from his hand in which HOIL is applied to questions that do not directly belong to natural language semantics, *On the Nature of Certain Philosophical Entities*. Whether more such papers would have followed had he lived longer can only be a matter of speculation.

AtM: That paper never had much of an impact neither in the philosophy community, nor in linguistic community, right? Since Montague's paper *English as a Formal Language* with its direct model-theoretic interpretation was too hard to read for many people, the paper *On the Proper Treatment of*

Quantification in Ordinary English became like the gold standard for natural language semantics.

HK: I think that one reason why *On the Nature of Certain Philosophical Entities* didn't have more of an impact on the philosophical community than Montague's papers on natural language semantics is that the paper remained an isolated contribution. There never were any follow-ups with more concrete applications of the ontological and logical proposals the paper makes to problems that preoccupy philosophers working in the respective areas to which the paper intends to make a contribution. Montague didn't have an opportunity to do so himself, and nobody in any of the relevant branches of philosophy felt compelled to pick up the torch and show the value of these proposals for that branch of philosophy. Some of the contributions made by Montague's papers on natural language are remarkably salient. Apart from the fact that these papers show that something can be done for significant parts of natural languages that many had doubted could be done at all, they contain at least one specific proposal that was an instant hit. I am thinking of Montague's insight that you can treat all noun phrases—both the referential and the quantificational ones—as denoting properties of properties. That idea has made us rethink some fundamental questions about reference, predication and truth. It opened our eyes to a new range of ways in which the syntax of a language can map onto its semantics and it resolved a puzzle that had been with us ever since Frege had pointed out that the grammatical similarity of sentences like *Socrates is mortal.* and *Every man is mortal.* should not mislead us about the fundamental differences in their logical forms. There is no question but that to perceive that difference and to integrate it in the design of a formal theory of reference, predication and quantification in the way Frege did has been one of the signal achievements of our civilization. But it left (or should have left) a sense that something was still missing: if the logical forms of these two sentences are as radically different as predicate logic seems to tell us, then why should natural language grammar be so radically misleading—how can it get away with misleading those whom the language, with its overtly recognizable grammatical structure is after all designed to serve? I think it is fair to say Montague has given us most of the answer to that question.

AtM: The history of generalized quantifiers started in mathematical logic. Was it an independent development at that time?

HK: As far as I can tell, the answer is 'yes'. For instance, the work on generalized quantifiers of Mostowski goes back to well before Montague's work on

Montague Grammar.[15] And some of the signal results on generalized quantifiers in mathematical logic, such as Keisler's theorem that adding *there are uncountably many* to first-order logic preserves its axiomatizability, do not seem to have anything to do with natural language.[16] In fact, the overlap between results about generalized quantifiers that might interest a mathematical logician and those that can be seen as relevant to natural language semantics isn't all that large. Barwise's result about non-axiomatizability of first-order logic + *most* with its standard semantics might qualify as an example, as might Johan van Benthem's results about the semantics of generalized quantifiers with finite domains, or the results of Keenan, Westerståhl and others about polyadic quantifiers, assuming we count these as generalized quantifiers too.[17] But as far as the impact of Montague's ideas about noun-phrase semantics is concerned, the seminal contribution is no doubt the paper *Generalized Quantifiers and Natural Language* by Barwise and Cooper.[18] In fact, this paper signals a historical rapprochement between natural language semantics and mathematical logic. Barwise and Cooper were colleagues at the University of Wisconsin. Barwise, an outstanding mathematical logician with an acute interest in foundational questions, had become convinced that fundamentally new directions were needed to make progress with certain problems in the foundations of mathematics. In search of alternative directions he decided to attend one of Cooper's seminars on natural language semantics. Cooper, who had done his Ph.D. with Barbara Partee, was one the world's experts on Montague's work, and his own work at the time can be roughly described as embracing some variant of Montague Grammar. The methods he taught and exemplified in the seminar must have persuaded Barwise that here was a way of describing natural language meaning that was capable of revealing the ways in which we express things in natural language faithfully enough and that at the same time

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[15] Mostowski, Andrzej, 1979, ***Foundational Studies. Selected Works***, I–II, North-Holland Publishing Company, Amsterdam.

[16] Keisler, Jerome, 1970, Logic with the quantifier "there exist uncountably many", ***Annals of Mathematical Logic***, 1, 1–93.

[17] Barwise, Jon, 1979, On branching quantifiers in English, ***Journal of Philosophical Logic***, 8(1), 47–80.

van Benthem, Johan, 1986, ***Essays in Logical Semantics***, Kluwer, Dordrecht.

Keenan, Edward and Dag Westerståhl, 2010, Generalized quantifiers in linguistics and logic, in: Johan van Benthem and Alice ter Meulen (eds.), 2010, ***Handbook of Logic and Linguistics***, 2nd rev. edn., Springer, Berlin, 859–910.

[18] Barwise, Jon and Robin Cooper, 1981, Generalized quantifiers and natural language, ***Linguistics and Philosophy***, 4, 159–219.

satisfied the standards of rigor that a mathematician considers a *sine qua non* if anything tangible is to be achieved. In this way the basis was laid for a remarkably fruitful cooperation, which lasted until Barwise's death and of which *Generalized Quantifiers and Natural Language* was the first major result.

AtM: Is there any connection you can see between *Generalized Quantifiers and Natural Language* and the development of Situation Semantics (SS) that conquered the world soon after and that was part of Barwise's central focus till his untimely death in 2000 and has also been very prominent in Cooper's work since the early eighties?

HK: The first paper that was truly about Situation Semantics that I recall was *Scenes and other Situations*.[19] When I read this paper—if I remember correctly, that was shortly before it appeared in print—I was very impressed by it. Because of its situation-theoretic approach it is of course quite different from *Generalized Quantifiers and Natural Language*, but, on the other hand, it shares with that paper many methodological and technical assumptions that are distinctive of Montague Grammar—so much so that it could not possibly have been written by someone who wasn't thoroughly familiar with Montague's work. In fact, the continuity between Montague Grammar and Situation Semantics should not be overrated any more than it should be underrated.

Situation Semantics soon became the joint project of Barwise and John Perry. In fact, as far as I know, one of the reasons why Barwise came to Stanford on a permanent basis was for him and John Perry to be able to work together in that way which is only really possible when you work and live in the same place. But at that point SS had already taken shape to a considerable extent. That was already so in the academic year 1981–1982, when Barwise and Perry were both part of a group of people, convened by Stanley Peters, who spent a year at the Center for Advanced Study in the Behavioral Sciences. The other members were Stan himself, Lauri Karttunen, Robin Cooper, Manfred Bierwisch, Johan van Benthem and some younger people, some of them brought along by Stan and Lauri from the University of Texas, where they were both working at the time, of whom I remember in particular Edit Doron, Elisabet Engdahl and Hans Uszkoreit. During that year at

[19] Barwise, Jon, 1981, Scenes and other situations, ***Journal of Philosophy***, 78, 369–397. Reprinted in: Jon Barwise, ***The Situation in Logic***, CSLI Lecture Notes 17, CSLI Publications, Stanford, CA, 1989, 5–36.

the Behavioral Sciences Center, SS was already widely discussed throughout the US and beyond.

AtM: What do you see as the fundamental step from PTQ and what I call ‘plain vanilla’ Montague Grammar, on the one hand, towards a more dynamic semantics on the other? And what was the role of SS in this transition?

HK: Whether SS is a form of dynamic semantics, or a step in that direction, depends on how you look at it. The SS of *Scenes and Other Situations* is no more dynamic than classical MG. The difference is just that in SS the semantic realities of certain expressions—including in particular naked infinitival complements of perception verbs—are situations rather than any of the entities made available by the ontology of HOIL. In SS properly speaking this assumption is integrated into a theory whose central semantic notion is that of a situation supporting a formula, instead of that of a formula being true at a possible world, or ‘index’ in the technical sense adopted in possible worlds semantics. However, you can also think of SS as a theory of how utterances can incrementally build the situations that support them, and from this perspective it can be seen as dynamic in a way that MG is not. But I am not sure to this day whether this second perspective does justice to the way in which Barwise and Perry themselves saw SS, whether at the time when SS got under way in the early eighties or later.

AtM: Speaking of dynamic semantic theories, you yourself have spent a lot of your time developing Discourse Representation Theory (DRT). Was DRT the result of your year at the Stanford Center?

HK: No, DRT already existed. In fact, it goes back to a problem that Christian Rohrer once put to me in Stuttgart about the difference between the French tenses ‘imparfait’ and ‘passé simple’. Christian had become interested in the use of formal methods when working for his Ph.D. under Coseriu in Tübingen, first in the context of machine translation. After a couple of years in the US and another two to write his ‘Habilitation’ in Germany he had become Professor for Romance Linguistics at the University of Stuttgart (at the age of 29!) and obtained a grant from the German Science Foundation for the application of formal logic to the semantics of natural language. By the end of the seventies he and his group had begun to concentrate on problems having to do with the use of the tenses in French and it was in that context that in 1978 he draw my attention to one of the classical problems in this domain: How do French speakers and authors choose between PS and Imparfait and what are the theoretical principles that govern such choices? French philologists had come up with various largely informal accounts of the difference between PS and Imparfait, among other things that the PS conveys punctuality whereas the Imparfait conveys durativity of the events they are used to describe. Christian at that point thought that the model-theoretic approach of Montague Grammar might be the right framework for capturing this and other distinctions that had been proposed in the literature. But when you start thinking more carefully about what this punctuality-durativity contrast could actually amount to, you come to see that the standard methods of mathematical model-theory are not really the right tools for capturing the insights that the more traditional philologists who had been thinking about tenses in Romance had undeniably had. For instance, what could it be for an event described with the help of the PS to be 'punctual'? Surely not that such events last for only one 'point of time'—one instant of the time we use in physics to describe physical processes such as atomic or astronomical motion. The right perspective, it seemed, which could make coherent sense of the claim that PS events are punctual and Imparfait events are not, is one according to which discourse, with its choice of particular time forms, creates its own ontology, and, as part of this, its own 'discourse time'. It is in the sense of this discourse time that the distinction between the 'punctual' PS events and the 'non-punctual' Imparfait events can be given what seemed to me a plausible explication. In particular, it is possible for an event to be punctual in the sense of discourse time and yet cover an extended stretch of real time.

AtM: That sounds like it is more aligned with Montague's program to model philosophical entities than with the Davidsonian theory of events as arguments of verbs.

HK: It is really quite different from what either Montague or Davidson were trying to do. Distinctive of an account of tensed discourse along the lines I was indicating is that it postulates a level of semantic representation of discourse distinct from the syntactic representations of the sentences from which the discourse is made up, on the one hand, and from the reality that the discourse is describing on the other. It is the difference between the discourse representations of this intermediate level and the reality described by the discourses represented by them that is responsible in particular for the difference between the discourse time generated by a discourse representation and the 'real time' of the physical world that is being described. And the relation between discourse time and real time is such that an event from the discourse representation may be punctual in the sense of discourse time, while the real event that it represents is extended in the sense of the real time of the described world. This was the conceptual nucleus which after a couple of years led to DRT as a general method for doing natural language semantics, in which discourse representations (DRT's Discourse Representation Structures) act as intermediaries between natural language discourse and the real or fictional worlds described by it. Because of the central role played by its DRSs DRT can be characterized as a logical form theory, much like earlier, usually not very systematic attempts to capture the meanings of natural language sentences by specifying their logical forms in predicate logic. In both these types of logical form account, the logical forms proposed have independently grounded meanings (the grounding is either model-theoretic or proof-theoretic or both). It is the logical form which specifies the meaning of that to which it is assigned as logical form. But compared with that kind of logical form approach DRT is a logical form theory with an extra twist, in which the representations are in general representations of larger units than single sentences. In fact, it was the use of DRSs as logical forms, and thus as intermediaries between natural language and reality, that seems to have provoked quite a strong resistance in the early years of DRT.

KvH: What were these problems that people had with accepting this additional representation layer?

HK: Actually, DRT soon came under attack from different quarters. One of these was the point of view of those that became the core of what is now often considered the 'true' Dynamic Semantics, starting with the work from the late eighties of Groenendijk and Stokhof (G&S). They also advocated a form of incremental interpretation to deal with donkey pronouns, but criticized DRT for making what they saw as the unwarranted claim that such an account had to involve a distinct level of semantic representation. And it seems that since then quite a large part of the community sees their alternative proposal and its many subsequent variations as Dynamic Semantics in the true sense of the word. It is also often said in conjunction with this that DRT is a kind of pseudo-dynamic theory, a doubly unfortunate combination of unwarranted representationalism on the one hand and a failure to distill the central dynamic concepts on the other. It should be stressed in this connection that Groenendijk & Stokhof's critique was directed at the theory presented in the paper *A Theory of Truth and Semantic Representation* (TTSR).[20] That paper focuses on the phenomenon of donkey pronouns—anaphoric pronouns whose anaphoric antecedents

[20] The paper is included in this book in Part Four.

are indefinite noun phrases in the same or an earlier sentence. G&S were right, or almost right in their claim that, when it comes to accounting for the donkey pronoun phenomena that TTSR discusses, a DRS interface between natural language syntax and described world truth conditions isn't needed. What we can do instead is to complicate the semantic values that are assigned to natural language expressions, such as information states and Context Change Potentials in lieu of propositions. Arguably, though, that isn't quite enough. One also needs a record of what variables have already been used and at which embedding levels; usually that information can be recovered from the information states. When a variable has not yet been used at a given level, the assignment functions that are part of the corresponding information state impose no restrictions on the possible values to that variable, but if the variable has been used, some values will have been excluded. But there is no *a priori* reason why used variables should always come with such a restriction on their possible values. It could be a feature of particular natural languages or fragments, and thus something one could rely on when dealing just with those. But it isn't something that can be taken for granted and, if true, that should be explicitly and carefully argued for.

AtM: A complaint one sometimes hears from people who have adopted Dynamic Semantics in the spirit of Groenendijk, Stokhof, Veltman and others is that DRT isn't really a dynamic theory of semantics at all—that it is just the combination of a purely static model-theoretic semantics for the logical form language whose formulas are the DRSs and some translation algorithm for converting sentences and texts from the natural language into the logical form language.

HK: Yes, that is of course a way you could describe DRT, and it is one that doesn't make it sound very dynamic. But what makes DRT a dynamic approach in my own eyes even so has to do with two things that this description leaves unsaid. First, the algorithm for forming sentences and discourses into DRSs is incremental. This is most obvious for the case of multi-sentence bits of discourse. Here the algorithm starts with constructing a DRS for the first sentence, then uses this DRS as a guideline (or 'discourse context') for the construction of a DRS for the next sentence, which gets integrated into the first DRS to form a DRS that represents the first two sentences together; this last DRS then acts as discourse context in dealing with the third sentence and so on. The role that DRSs play in the applications of the translation algorithm is thus a double one: on the one hand, a DRS represents the content of what has been translated already—it identifies that content via the truth conditions that it has by virtue of the model-theory of the DRS language; on the other, the DRS serves as discourse context for what it is to be interpreted next. DRSs thus emerge as a kind of unification of content and context. They function as links between what has already been processed and what is still to come. And the construction algorithms that make use of them are 'dynamic' in that they build DRSs incrementally, sentence by sentence.

At the same time the double role that DRSs play in DRT may be taken as an argument for an approach that makes use of them as constituting a separate level of semantic representation. The fact that they can at the same time capture the content of the bits of language from which they have been derived and play an effective part in guiding further interpretation can be seen as in indication that they capture some of the reality of incremental processing that goes on when human speakers interpret a discourse. Seen from this angle DRT takes on the status of a theory of mental processing of language and of the mental representations that are produced and manipulated in the course of those processes. That is, of course, no refutation of the argument G&S launched, but it is making a case for the logical form approach of DRT that is largely independent from their critique.

AtM: When one thinks of DRT as a logical form theory which transforms sentences and sentence sequences into 'logical forms' belonging to some logical formalism, one is reminded of Game-theoretical Semantics (GS) as proposed by Jaakko Hintikka in the nineteen seventies.[21] Was GS a source of inspiration for you?

HK: I had looked at GS quite closely when it became popular, partly through Hintikka's own publications and partly through those of his student and collaborator Esa Saarinen. So I was certainly aware of GS when I was working on the first explicit DRT construction algorithms, that of TTSR and that for a small fragment of French which focuses on temporal matters and deals specifically with the Passé Simple and Imparfait. (This second paper appeared, in French, in the French journal ***Langages*** and doesn't seem to have had much visibility.[22]) But what struck me as unsatisfactory about GS from the start, and still does so today, is that it was quite casual and

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[21] Hintikka, Jaakko and Gabriel Sandu, 2010. Game-theoretical semantics, in: Johan van Benthem and Alice ter Meulen (eds.), ***Handbook of Logic and Language***, 2nd rev. edn., Elsevier, Amsterdam, 361–410.

[22] Kamp, Hans, 1981, Référence Temporelle et Représentation du Discours, ***Languages***, 36–64.

inexplicit about the syntax of the natural language sentences for which game-theoretic analyses were being proposed. That makes it very difficult to see exactly what the theory predicts, especially in cases where the crucial questions have to do with the interactions between pronouns and their quantificational antecedents. One way in which you become aware of this difficulty is when you want to prove for certain fragments of natural language that a given game-theoretical semantics proposed for them is first-order in that it assigns all sentences of the fragment first-order truth conditions. In GS such proofs are often desirable even for fragments where it seems intuitively clear or plausible that the game-theoretical treatment proposed for them is first-order. This is because the truth conditions directly assigned in GS are always second-order on the face of it: they always involve quantification over game strategies, which are functions from finite sequences of individuals to individuals. But in order to carry out such a proof —that a certain game-theoretical treatment of a certain natural language fragment *is* first-order—it is necessary that the treatment provide you with a fully explicit definition of the truth conditions it assigns to each of the sentences of the fragment. And that presupposes, among other things, that the syntactic structures of the sentences to which the game-theoretic algorithm assigns these truth conditions must have been made fully explicit. So, for me this was not an example to follow. If I got anything from GS that was directly relevant to the development of DRT it was a sharpened awareness of how important it is to be fully explicit about syntactic structure when you do natural language semantics. But, of course, that lesson is also plain from Montague's work, with which I had been imbued as a graduate student.

AtM: What you say about the second-order form of game-theoretically defined truth conditions reminds me of other work of Hintikka's from the sixties and seventies which addresses the question which parts of natural language are first-order and which irreducibly second-order. I am thinking in particular of what Hintikka has said about branching quantifiers.[23]

HK: Yes, that is a point on which Hintikka has been proved right many times over. At the time many people did not find his own examples of irreducibly second-order sentences—examples involving branching quantifiers that he claimed arise in sentences with more than one complex noun phrase of the form *a relative of every villager and a relative of each townsman*—wholly convincing. But at least in my own case that problem seemed no longer decisive,

[23] Hintikka, Jaakko, 1973, Quantifiers vs. quantification theory. ***Dialectica***, 27(34), 329–358.

once Barwise had come up with quite different examples involving quantifying determiners like *most* and *few*. In fact, today we are aware of another category of natural language sentences that are not first-order, because they involve plural indefinites and plural pronouns anaphoric to them. A classical example of this is Kaplan's *Some critics only admire each other*, for which he showed quite early on that its truth conditions are essentially second-order. That natural languages like English are not first-order, and for reasons other than overt quantification over properties, sets or predicates, is a point of the first methodological importance. But the moral isn't necessarily that in order to get things right we have to move to GS.

KvH: There were claims that when you formulated DRT you were influenced by AI and knowledge representation?

HK: No, I don't think that is true. What is true is that I participated in some of the meetings that the Sloane Foundation organized in the late seventies and early eighties as part of its effort to establish Cognitive Science as a distinct scientific discipline. And it is also true that in Cognitive Science the existence of mental representations isn't really questioned. The issue here is what representations are like and how they are used. I think that that probably had an influence and that it kept in check whatever sense I might otherwise have felt that I was out in left field. But at that point DRT was already in place. I do not think that contact with cognitive scientists had much, if anything, to do with its conception.

But one doesn't really have to look towards AI or Cognitive Science to find support for the idea of a representational approach to semantics. Something that has puzzled me for many years about the resistance against representation within formal semantics is that most of the fiercest opponents that I have come across have been people with a background in formal logic. This seems odd to me insofar as formal logic has traditionally been concerned as much with logical form as with anything else. Perhaps the most important and fruitful single insight in the history of logic, due to Aristotle, that from premises that tell you something about a given domain or situation you can arrive at further conclusions about that domain or situation through the application of purely formal operations, in which the content can be completely ignored. Logical form—how the given contents are represented rather than what content is represented—has therefore been an essential ingredient of formal logic since antiquity.

So why should people with a background in formal logic be suspicious of a logical form approach such as DRT to the semantics of natural languages? One reason, no doubt, is the intuition that appears to have moved

Montague: the 'logical forms' of natural language sentences ought to be the very structures that are ascribed to them by the syntax for the natural language itself. This is part of Montague's claim that there is no significant difference between natural languages and the artificial languages of formal logic. And we do know quite a bit about what that syntax is like on independent grounds, to some extent from intuitions that can be elicited from native speakers, from traditional work on grammar and arguably also from the more sophisticated methods of modern generative syntactic theory. In fact, this would be an attractive position and potentially an argument against the logical form approach of DRT, if only the 'logical forms' provided by the natural language syntax could pass muster as logical forms in the traditional sense attached to the term in formal logic, viz. as forms that can serve as premises and conclusions in formal accounts of logical deduction. But unfortunately no one has ever succeeded in showing convincingly that such grammatically motivated 'logical forms' can be made to serve in natural systems of formal logical deduction.

A related factor in the resistance to DRT as a logical form theory may have been the rise of model theory as a complement or alternative to proof theory and as providing the conceptually correct and universally applicable way of defining the basic concepts of inference, first and foremost that of logically valid consequence. The conviction that natural languages are not first-order may have further contributed to the conviction that the model-theoretic method is clearly to be preferred. Of course, that is not a good argument against the logical form approach, for there is nothing wrong with higher-order logical forms. In fact, none of the considerations I have mentioned seem to be good reasons against representational alias 'logical form' approaches as such. When it comes to what could have motivated the aversion against logical form approaches that seemed to be abroad at the time, I really feel at a loss.

AtM: As far as early criticism of DRT is concerned: it didn't come just from Dynamic Montague Grammar, but also from those who reject Dynamic Semantics in any form, and advocate some form of E-type analysis of donkey pronouns, right?[24]

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[24] Evans, Gareth, 1980, Pronouns, ***Linguistic Inquiry***, 11, 337–362.

Cooper, Robin, 1979, The interpretation of pronouns, in: Frank Heny and Helmut S. Schnelle (eds.), ***Syntax and Semantics 10: Selections from the 3rd Groningen Round Table***, Academic Press, New York, 61–92.

HK: The E-type analysis in its original form—specifically the versions proposed by Gareth Evans and by Robin Cooper—antedates DRT by a few years. A legitimate reproach that could be made to the early presentations of DRT is that they did too little to compare the proposal they put forward with these alternatives. Let me try to say very briefly what I take to be the essence of the E-type account and the original motivation for it. The E-type account of donkey pronouns analyzes them as a kind of shorthand for definite descriptions. Evans' original motivation, I believe, for trying to deal with donkey pronoun phenomena in this way was the then widely held view that a satisfactory account of definite descriptions was already in place, in the form of Bertrand Russell's Theory of Descriptions.[25] In his view replacing donkey pronouns by certain definite descriptions provided an account of them, since it reduces sentences containing them to sentences for which a good account was already in place.

There are two empirical assumptions that come with such an account. The first is that donkey pronouns, in spite of the fact that they are anaphorically linked to an indefinite antecedent, nevertheless convey the kind of unique identifiability that is part of the meaning of Russellian descriptions. The second is that there is a systematic way to recover the definite descriptions that are to be substituted for the donkey pronouns to which the analysis is being applied. Evans was clearly aware of both these assumptions. He argues extensively that the first assumption is correct. And he shows his awareness of the difficulties with the second assumption: it isn't always possible to extract the substituting description by means of a strictly linguistic algorithm from the surrounding text, and in such cases an appeal to the context is necessary. Such cases are also a problem for Cooper's version, which offers a text-based algorithm for the construction of the substituting description. Even with this concession, however, the E-type theory makes a definite empirical commitment through its first assumption. Some of the predictions it makes in view of this assumption are different from those made by DRT and other, non-representational dynamic accounts of donkey phenomena. One such difference relates to discourses like *John owns a donkey. He keeps it in the stable next to his farmhouse*. Evans' E-type account makes the prediction that this discourse is felicitous only if the speaker has some specific donkey in mind in connection with her use of *a donkey*, viz. that donkey to which the definite description that is to be substituted for *it* picks out.

[25] Russell, Bertrand, 1905, On Denoting, ***Mind***, 14(4), 479–493.

In its original form, DRT predicts no such constraint: it assigns to a two-sentence discourse like the one mentioned simply the truth conditions of an existential quantification, contributed by the indefinite *a donkey* with the predications from both sentences within its scope. For sentence sequences of this kind, that are used and understood as describing situations in the real world, the E-type prediction seems right, so here dynamic theories are in need of correction. But the matter seems different for donkey sentences like Peter Geach's *If a farmer owns a donkey, then he beats it* or Irene Heim's telling example *If you are buying a sage plant here, you have to buy eight others with it*, said by one person to another who is contemplating buying a sage plant from a market stand, where they only sell sage plants in squares of nine. In these examples the kind of specificity that the pronoun in a donkey discourse seems to be imposing on its indefinite antecedent appears to be absent, and the absence of a prediction of such an effect is therefore desirable.

Subsequent refined versions of the E-type theory, initially by Irene Heim in her 1990 paper on donkey anaphora and by Stephen Berman in his dissertation, were designed to do justice to both these intuitions.[26] They combine the distinguishing features of the E-type approach—donkey pronouns are analyzed through reduction to non-pronominal terms and indefinites, including the indefinite antecedents of donkey pronouns analyzed as existential quantifiers—with a Situation Semantics of the kind developed by Angelika Kratzer.[27] To give a satisfactory account of all cases, however, especially of the recalcitrant conditionals with two similar indefinites in the antecedent, such as Jan van Eijck's *If a man lives with another man, he shares the housework with him*, or the bishop sentence *If a bishop meets a bishop, he blesses him*, even more complex situation-based analyses are needed, which bring substantial numbers of distinct situations into play. One problem with those analyses is that the situations they appeal to are in part very abstract. This has become more pronounced as the situation-based account has developed further, especially in the work of Paul Elbourne.[28] In these versions it becomes increasingly difficult to see exactly what differences remain

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[26] Heim, Irene, 1990, E-type pronouns and donkey anaphora, ***Linguistics and Philosophy***, 13, 137–177.

Berman, Stephen, 1991, ***On the Semantics of Wh-clauses***, Ph.D. dissertation, University of Massachusetts, Amherst, MA. Published with Garland, New York, 1994.

[27] Kratzer, Angelika, 2002, Facts: Particulars or information units? ***Linguistics and Philosophy***, 25, 655–670.

[28] Elbourne, Paul, 2005. ***Situations and Individuals***. MIT Press, Cambridge, MA.

with DRT-based theories, which capture the relevant semantic properties in terms of discourse referents and DRS structure. A further important consideration is the role of event semantics: if, as increasing numbers of semanticists are taking for an established fact, events are an essential part of the ontology of natural language, then a situation theorist who accepts this fact, has to ask himself what the relation is between events and situations, and in particular, whether events, needed in any case, could not shoulder some or all of the burden of accounting for the local donkey phenomena we find in conditionals.

KvH: Returning to event semantics, any anecdote you recall about the work you did with Christian Rohrer?

HK: Nothing particularly amusing. One thing I should say first. The one who brought me to Stuttgart in 1972 was Franz Günthner. Franz was one of the members of Rohrer's group at the University of Stuttgart at that time. But it wasn't until 1978 that I spent more time in Stuttgart, long enough to do some real work. That is when the foundations were laid for the work that Christian and I did together in the eighties.

One thing I do recall is that in 1972 Rohrer's group was housed in the Schlossstraße in Stuttgart, in the attic of some rather gloomy building that, as one of only a few in that part of town, had survived the war. It is there that I developed the ideas about the role of discourse time in the processing of tenses and temporal adverbs as the basis for a linguistically meaningful distinction between punctuality and durativity. In particular, the manuscript of what subsequently appeared as the paper *Events, Instants and Temporal Reference* was written there, quite quickly by my standards, in the course of a weekend with much writing and not much sleep.[29] I still quite vividly recall that Rohrer's group shared the attic floor with the son of the caretaker of the building, who must at that time have been in his early twenties, or very late teens. He was a rather gloomy and taciturn character, whose impression on all of us was all the more sinister because he never said anything and looked like he thought the world would soon come to an end, and that when the time came, he personally would have nothing against helping it over the edge. Spending all or most of the night with this character next door and nobody else in the building was a little disconcerting, but fortunately I was too busy to give such thoughts much room to evolve. In any case, nothing ever happened and the man was probably perfectly harmless. Perhaps he

[29] This paper is included in Part One of this book.

would even have proved to be quite nice, had we made more of an effort to be nice to him.

I remember that summer, as well as the next one, when I returned for a slightly shorter stay, with great fondness. Christian had succeeded in getting a number of people to come to Stuttgart for short periods in the summer, including Dov Gabbay and Lennart Åquist. Together with the permanent members of the group we had regular meetings in which we took turns telling the others about our recent work and sometimes it was very recent, meaning from the week or the day before. All in all these summers were remarkably productive and a lot of fun.

KvH: You mentioned Heim's work. Were the two of you working independently? When did you first hear from her?

HK: The first time Irene and I met was at a conference at the University of Konstanz in 1978, where I presented material from the paper *Events, Instants and Temporal Reference* already mentioned. Irene was at that point a graduate student in linguistics at the University of Massachusetts, but her reputation had already reached beyond the places where she had been a student and I remember that I knew of her before we set eyes on each other at the meeting. Then, during the academic year 1981/82 that I spent at the Stanford Behavioral Sciences Center, Irene was at Stanford as holder of one of three fellowships that had been given to Julius Moravçsik. The other two fellows were Mürvet Enç and Susan Stucky. At that point the foundations of Irene's File Change Semantics (FCS) and DRT were already firmly in place. As I said, the first version of FCS goes back to 1979 and thus antedates the first explicit version of DRT by about a year. During the year at Stanford Irene and I talked a fair number of times, though not on a regular basis.

KvH: So your ideas developed in different places, but they were based on the same sources?

HK: That may depend on what you understand by 'the same source'. Irene was a Ph.D. student of Barbara Partee at the time, and I had talked to Barbara in the spring of 1980, a couple of days before that year's Amsterdam Colloquium (or 'Montague Colloquium', as it was then still called), which on that occasion took place some time in March. The talk I gave at the Colloquium discussed some of the ideas of DRT, but without the formal definitions, which came together only a few months later—some time in May, if I remember correctly—during a research stay at the Cognitive Science Center of the University of Texas. On the day before the meeting Barbara gave me her famous ball-example: (i) *One of the ten balls is not in the bag. It is* *under the sofa*. (ii) # *Nine of the ten balls are in the bag. It is under the sofa*. In (i) *it* can be straightforwardly understood as referring to the missing ball, but in (ii) it cannot. The moral of this can be summarized as follows: (i) and (ii) have the same second sentence, and, while their first sentences are different, they express the same proposition, i.e. are true in the same possible worlds. But the interpretation of the second sentence that is possible in (i) is not possible in (ii). So the difference between (i) and (ii) cannot be explained in terms of the propositional contents of their first sentences, since they express the same proposition. Nor can it be explained in terms of the content or form of the sentence containing the pronoun, since that sentence is the same in (i) and (ii). Therefore the decisive difference must be the forms of the first sentences, as distinct from their contents. It is the form of the first sentence, and not just its propositional content, which provides the context that enables the pronoun to refer to the missing ball. This ball-example came to play an important role in the general argumentation that something is needed beyond standard Montague Grammar to give an adequate account of donkey phenomena. Barbara's instantaneous reaction, when she gave this example in response to an alternative illustration of largely the same point that I put to her showed to me that she had been thinking about similar problems, and probably more deeply than I had. In retrospect it seems very likely that it was connected with her advising of Irene's dissertation. So she may have been an important 'common source'.

AtM: Another possible common source that comes to mind, though, is David Lewis's account of unselective binding from the first half of the seventies. Did you see the connection at the time when FCS and DRT were developed between unselective binding and what you were after?

HK: There is no question but that I was aware of Lewis' *Adverbs of Quantification*.[30] I was present at its first presentation (at a conference on natural language semantics organized by Ed Keenan in Cambridge in 1974) and must have read the paper repeatedly between then and the time when I started thinking about incremental discourse interpretation. But here too there is an important difference in motivation. Lewis was concerned with the type of expression mentioned in the title of his paper, viz. quantificational adverbs. His paper is just about the semantics and logical forms of sentences that contain such adverbs, not about multi-sentence discourse.

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[30] Lewis, David, 1975, Adverbs of quantification, in: Edward L. Keenan (ed.), ***Formal Semantics of Natural Language***, Cambridge University Press, Cambridge, 3–15.

It was only the work of Craige Roberts about 'modal subordination' in her 1987 dissertation and related work by Peter Sells from around the same time that showed how Lewis' observations can be extended to multi-sentence discourse. But that work makes quite explicit use of DRT.[31] The common ground between Lewis' *Adverbs of Quantification* and my *A Theory of Truth and Semantic Representation* is what the two papers have to say about conditionals in which the quantificational adverb, representing some kind of universal or generic quantifier, is tacit. For such sentences they propose what are in essence the same logical forms, and so are the motivations underlying them. Lewis treats the *if*-clauses of his adverbially quantified sentences as restrictors of his unselective quantifiers, which describe types of 'cases'. In the treatment of TTSR the same *if*-clauses are seen as specifying hypothetical states of affairs and the logical form for the sentence as a whole is obtained by interpreting the main clause in the context provided by this hypothetical state of affairs description. In Lewis' terms: the main clause is interpreted as making a statement about the 'cases' described by the *if*-clause. But the overall motivations were nevertheless quite different.

In TTSR, the emphasis was on the interpretation of donkey pronouns in discourse no less than in sentences. The aim was to show that the mechanism responsible for the behavior of donkey pronouns in donkey sentences was the same as that governing their interpretation in 'donkey discourses' (such as Partee's ball example). The only difference is that in donkey sentences the state of affairs description that serves as interpretation context for the pronoun is hypothetical. Overt quantificational adverbs were not dealt with in the first papers on DRT. And the phenomenon of unselective binding, which stands out as the most salient similarity between Lewis' treatment of adverbs of quantification and early DRT, emerges in the two theories in quite different ways. For Lewis unselective quantification is part of the nature of adverbial quantifiers; in DRT it is a kind of side effect, due to the general architecture of logical forms (the DRSs) and the particular way in which it analyzes indefinite noun phrases. In this respect, by the way, Heim's File Change Semantics is much like DRT; at least this is so for the account presented in Ch. 2 of her dissertation.[32]

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[31] Roberts, Craige, 1990, ***Modal Subordination, Anaphora, and Distributivity.*** Garland, New York.

Sells, Peter, 1984, ***Syntax and Semantics of Resumptive Pronouns***, Ph.D. dissertation, University of Massachusetts, Amherst, MA.

[32] Heim, Irene, 1982, ***The Semantics of Definite and Indefinite Noun Phrases***, Ph.D. dissertation, University of Massachusetts, Amherst, MA. Published with Garland, New York, 1988.

AtM: From what you said earlier about what led you to DRT, it would seem that your motivation and Irene's were quite different. The real issue for her was novelty/familiarity, as the feature that distinguishes definites from indefinites: the referents of definite descriptions are supposed to be known, the referents of indefinites are supposed to be new. That distinction is so very different from MG, and much of her dissertation is concerned with it.

HK: That is true. But it is important to keep in mind in what ways the familiarity-novelty distinction is different from Montague Grammar. It is not just that familiarity and novelty are notions that do not have a natural home in MG (not at any rate as it was understood and practiced at the time). But it was also important because of the way in which it repositioned the category of indefinite noun phrases within the grammar: definites and indefinites are now seen as forming one category of referential noun phrases, distinct from the other main category, the quantifying noun phrases. This repositioning is part of a quite new perspective on questions of reference, quantification and variable binding. It differs from the traditional view, which distinguishes between referential and quantifying phrases, but treats indefinites (and sometimes also all or some definite descriptions) as quantifying, and from Montague's perspective according to which all noun phrases, even proper names, are generalized quantifiers.

KvH: You were saying that your thinking about the Passé Simple and Imparfait in Stuttgart in 1978 set you on the course that led to DRT, that your talk in the Montague Colloquium of 1980 was a crucial next step, but that the decisive step was still missing and that this missing link came to you only during the months you spent a little later at the University of Texas. What was this missing link?

HK: In the form in which DRT was then formulated, it was the insight that the model-theoretic truth definition for the basic DRS-language—the formalism of which the DRSs are the formulas—should be given in terms of partial assignments that are extensions of other assignments. The point here is that, although truth definitions of formalisms like the predicate calculus are usually thought of as 'bottom-up' in that they define the semantic values of complex expressions from the semantic values of their constituents, those definitions are in an important sense, and for the very same reason, also 'top-down'. When you are interested in what the definition has to say about some particular complex expression, you typically start with a clause which states that the expression has a certain value in a given model M at a given index i and under a given variable assignment g and then use the definition to reduce this statement to statements about smaller and smaller constituents, until you have reached the atomic constituents. In the course of such a reduction you move from the initial variable assignment g to different assignments; and when you use partial assignments (which is also possible when you give the truth definition for classical predicate logic, although for some reason this is not usually done), you find that with every reduction that involves a change in the assignments under consideration this change takes the form of *extending* the assignments involved thus far. DRT exploits this feature of growing assignments in a new way. Assignments now play the part of contexts of interpretation in a setting in which the complex expressions are DRT's logical forms, i.e. its DRSs. The assignments that enter into the evaluation of such an expression are made to play the role of contexts for the evaluation of constituents (i.e. sub-DRSs and DRS-conditions) of the given DRS. This means that many of the clauses of the recursive truth definition for DRSs and DRS-conditions have to refer to two assignments rather than one—the assignment that serves as context and the one with respect to which the constituent is said to be true, and which is an extension of the former. To get a truth definition of this form to do just the right thing must appear as something close to a triviality these days. But at the time it took some careful consideration; in any case it took me some time to see how actually to do this.

KvH: When you were at Stanford discussing DRT with the other people in the group, and beyond, how did they react to it?

HK: One of the aims of the group was—I hope I am speaking here also in the spirit of all or most of the other members, and am not just projecting my own self-centered perspective—to bring SS and DRT together, to see what was shared, what was different and how one might make a choice between them or achieve a synthesis of them.

This goal took a more concrete form in the course of that year; we were going to write a book together in which SS and DRT would each offer their own treatment of a certain fragment of English for which two members from the group were designated to provide a syntactic description. Unfortunately, this syntactic description—basically just a necessary preliminary for the semantic parts that would build on this foundation—never made it beyond the pre-final version and eventually that also meant the end of the project as a whole. Perhaps that was just as well, for as I came to see things, partly as the result of my effort to fulfill my side of the commitment, the project was based on what was probably a misconception of the relationship between SS and DRT and of what might be seen as their respective strengths and weaknesses. What I came to perceive as a strength of SS is its emphasis of the fact that, when we think or speak about the way the world is, we typically focus on certain small parts of it, certain 'situations'. It is in relation to the particular situation or situations that we are talking about that the words we are using are to be interpreted and evaluated. SS endeavors to flesh this insight out by developing a detailed theory of the structure of situations and using this structure in analyzing the semantics of language. My own attempts at situation-semantic analyses of certain linguistic constructions left me with a sense that the range of options SS offers for analysis in natural language semantics is almost too rich for its own good. Often the range of possible choices for the theorist is so large that the framework gives no guidance as to how one should proceed—one is faced with a true *embarras du choix*. In this way the strength I mentioned easily turns into what a linguist might feel to be a weakness.

DRT is, both in its aims and in its form, a very different enterprise. It is more directly focused on the question how language represents content. What content is, and in particular, whether situations play any part in it, are less prominent questions than in SS. Central to its conception is the finiteness and partiality of the information provided by linguistic descriptions, below, at and above the sentence boundary (i.e. by sentence constituents, by complete sentences and by multi-sentence discourse). Of course, when the idea is put like this, there is nothing really new to it. DRT makes special use of this partiality; but it is compatible with the adoption of a conventional model-theoretic semantics, which acknowledges no ontological categories that cannot be found in HOIL. This more specifically linguistic perspective might have been combined with SS by formulating a situation-based model-theoretic semantics for some suitable DRS-language. I still think that such combination of SS and DRT might have been illuminating for our understanding about how the two are related and that such a combination might have been fruitful. But the two weren't put together then, and it hasn't been done since. And in any case, the usefulness of such a combination would perhaps be less now than it might have been at the time, since eventualities play a so much more prominent and accepted part in semantics.

AtM: SS was marketed as semantic realism—and Barwise at the time rejected any form of representationalism, as DRT is. You mentioned you had formed the opinion that SS offers far more options than linguistic analysis can use, and that this renders it unhelpful as a tool for linguistic analysis. That is in some ways reminiscent of what Chomsky and other generative syntacticians have seen as a desideratum for grammar frameworks. They should be subject to the constraints built into the human 'language engine', which select for only a small number of the totality of logically possible languages; only the languages belonging to this small subset are humanly possible languages. Your complaint also reminds me of one that has been leveled at certain categorial grammars, which provide for often large numbers of alternative syntactic analyses of intuitively unambiguous sentences, such as e.g. *Mary kisses John*.

HK: There are a number of different points you are raising, too many probably to sort out properly in an answer that isn't going to be excessively long. What I was thinking of in particular just now when I said that SS isn't giving enough guidance to the natural language semanticist was work that Robin Cooper and I embarked on in the early nineties, which explored the different options in Situation Theory for the semantic definition of the logical constants of classical predicate logic: negation, disjunction, conditionals, conjunction and the universal and existential quantifiers. We started with negation, but we never got beyond it. The main reason was that the situation-theoretic framework left us with just too many options to explore without seeming to provide us with any natural handle on how to choose between them and select those that would be relevant to the analysis of negation in natural language (or, for that matter, for any other philosophical or logical applications).

The kind of spurious ambiguity problem, like that of getting multiple syntactic analyses for *Mary kisses John*, is a somewhat different issue. In order for this problem to arise for SS we would need explicit situation-theoretic descriptions of fragments of natural languages, of the sort that has been presented in work Cooper and by Peters and Gawron.[33] But as far as I know the syntactic descriptions proposed there do not lead to the structural ambiguities that are distinctive of Categorial Grammar.

A problem of overgeneration of structural descriptions, by the way, also seems to lurk off stage in Elbourne's proposal to deal with housemate- and bishop-sentences in his non-dynamic situation-based theory of donkey phenomena. What is it about the form of such English sentences that

[33] Barwise, Jon and Robin Cooper, 1993, Extended Kamp Notation: A graphical notation for situation theory, in: Peter Açzel et al. (eds.), ***Situation Theory and its Applications***, CSLI Publications, Stanford, CA, 29–53.

Cooper, Robin, 1993, Generalized quantifiers and resource situations, in: Peter Açzel et al. (eds.), ***Situation Theory and its Applications***, CSLI Publications, Stanford, CA, 191–211.

Gawron, Marc, and Stanley Peters, 1990, ***Quantification and Anaphora in Situation Semantics***, CSLI Publications, Stanford, CA.

determines the particular logical forms Elbourne proposes? Or does the syntactic form of those sentences underdetermine those logical forms, allowing for a great many, with at least one among them that fits Elbourne's proposals?

AtM: You have reminded us of your view of DRT as a more cognitively real way of doing semantics. But applications of DRT are normally formulated as involving algorithms that convert given syntactic structures incrementally into semantic discourse representations. What about the psychological reality of those syntactic structures? What remains of the cognitive claims of DRT, if there is no psychological reality to the syntactic structures which existing versions of the theory take as their point of departure?

HK: That is a serious problem and it has also been a rather frustrating one. All applications of DRT to natural language—and that is of course what DRT is primarily about—have been suboptimal or problematic for this reason. In the most ambitious single attempt of this kind that I have thus far been a party to—the book ***From Discourse to Logic*** (FDL) with Uwe Reyle—we tried to finesse the problem by assuming a syntax of which we said emphatically that it was just a stand-in for more serious syntactic theories which we hoped others would at some point put in its place.[34] We hoped that others would come with the syntactic theory of their choice, motivated by serious syntactic and/or psychological considerations, and define on the basis of it a DRS construction algorithm employing essentially the same construction principles that we were presenting in the book. Of course, from the perspective of actual sentence processing such a replacement would still be a serious idealization in that it describes syntactic parsing and semantic processing as sequential. Actual parsing by human interpreters is clearly a process in which syntactic and semantic structures are identified in tandem and 'on line', as the successive parts of each sentence become available. But that is the kind of idealization that the existing formulations of DRT share with pretty much all other work within theoretical linguistics that concerns the syntax-semantics. So we were well aware, when we wrote the book, that the way we proceeded was far from optimal. But we thought that it would enable us to show enough of the essential properties of the algorithms in question to make it possible for people with more specific syntactic

[34] Kamp, Hans and Uwe Reyle, 1993, ***From Discourse to Logic. Introduction to Modeltheoretic Semantics of Natural Language, Formal Logic and Discourse Representation Theory***. Kluwer, Dordrecht.

commitments or predilections to see how to adapt our proposals to the syntax of their choice. Later on I have often worried whether that had been the right strategy. On the one hand, we did get a certain amount of flack about the naïveté of our syntax; on those detractors our explicit disclaimer—that this was just a make-do syntax and that it did not in any way represent a deeper syntactic commitment on our part—was apparently wasted. On the other hand, the adaptation of our construction algorithm to different syntactic frameworks that we were hoping for never really took off.

But I'd like to think that times have changed and that a new perspective on syntax is gradually emerging, which makes it possible to distinguish between (a) what syntactic structures should be assigned to particular grammatical expressions, and (b) what general syntactic principles should be invoked to explain why it is that those syntactic structures should be assigned to the expressions of a given language and not others. Not that all controversies about the syntactic structure of particular expressions have been resolved. There are many disputes remaining, both within individual syntax theories and between them. But especially the extensive work on syntactic annotation, beginning with the Penn Tree Bank, has led to a considerable degree of convergence on the substance of syntactic structure, even where the debate over underlying principles and the overall architecture of a syntactic theory predicting these structures continues.

Restating DRS construction algorithms as operating on syntactic structures used in large annotation enterprises is now becoming an option that did not exist in this form in the first half of the nineties. On the other hand, a lot has since been learned about the internal structure of words. It is now widely recognized that many words are complex syntactic and semantic structures, built from 'roots' and 'functional elements'. This development also poses a new challenge for DRT: construction of semantic representations should go, compositionally, all the way from the building blocks of the words up to the level of the full sentence (and beyond to that of multi-sentence discourse). Antje Roßdeutscher and I are currently working on such 'root-based' DRS construction methods.

AtM: Returning to what you said about the year at the Stanford Behavioral Sciences Center and the interaction with Situation Semantics: In Situation Semantics and Situation Theory there was an increasing focus on information and information flow. How would you now characterize information structure? What should it do for you or what primitives should it have?

HK: 'Information' is one of those words that are under lots of stress these days. On the one hand, everybody and his grandmother are talking about information and more often than not, one has the impression that few expressions could be less informative. On the other hand, there is also a formally precise and well-understood cluster of information-related notions in the tradition of Shannon.[35] The theory of information in this tradition is of immense importance and the foundation for most of what makes our electronically shaped world tick the way it does. But it is still very difficult to establish convincing and practically useful links with information as it is conveyed by language and captured in thought. In fact, there is some well-known information-related work on natural language that is closer to mathematical information theory. This is the work by Paul Zipf, and further research in the tradition he established. Zipf's Law is a good example of the way in which the information density of individual words of a natural language is distributed over its lexicon.[36] But these results are very far removed from what we normally think of when we speak of the information that one obtains from a verbal message.

Recently, however, these two worlds of information have finally been converging—dangerously so, I am almost tempted to say. In the branch of Computational Linguistics that has come to be known under the name of Distributive Semantics the semantics of a word is captured in terms of a vector that encodes its co-occurrence frequencies with other words in some corpus. This information is useful in many meaning-related computational tasks. But the really hard question is how this kind of information about word meaning can be related to the ways in which the semantic contributions of words to the truth conditions of sentences, or to logical forms such as DRSs. I do not know how far anyone has got with finding answers to this question. But the pressure to build a bridge between these two approaches to meaning is high and growing. So it may not be unreasonable to expect that, if there are any ways of building such a bridge at all, they will be built before long.

But all this only pertains to the notion of information itself and not to the notion of information flow. Here too, the two traditions I have been hinting at are very different. Information theory in the Shannon tradition sees information flow as involving coding, code transmission and decoding. Many of its results concern coding complexity and its optimalizations, transmission noise and channel capacity. The problems about information in linguistics

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35 Shannon, Claude E. and Warren Weaver, 1949, ***The Mathematical Theory of Communication***, University of Illinois Press, Urbana, IL.

36 Zipf, George K., 1949. ***Human Behavior and the Principle of Least Effort.*** Addison-Wesley, Cambridge, MA.

are different because, first, the code is given to begin with—the code is just the natural language as available to its users, with its rules of phonology, morphology, syntax, semantics and use. So the problem of natural language semantics is to a large extent that of finding perspicuous ways of representing or identifying the information carried by different bits of this code. There is, of course, also the question how the information carried by bits of natural language code succeeds in getting transmitted from one agent to another. Questions of noise are already prominent in phonetics and phonology, but it is obvious that the redundancies in linguistic messages make it possible for them to get across as smoothly as they do most of the time. Even when strictly phonetic-phonological recognition is impaired, there is redundancy at the levels of syntax and semantics. Therefore the problem of speech recognition is really a problem of language recognition that involves many different levels of linguistic representation, including also levels of semantic representation which enable the decoder to assess decoding hypotheses for their consistency and coherence with the current context. Translated into the terminology I am using right now, information somehow has to get across—coded, transferred, decoded—at a number of distinct but interacting levels.

Related to this is an issue of general importance for the methodology of natural language semantics. Formal semantics has been operating on an assumption that can also be traced back to some of the deep convictions of Montague, that fits in with what many semanticists with a background in formal logic had come to see as the true and only possible way forward. This is the conviction that languages can be studied as self-contained systems, with a syntax and a semantics, which make certain forms with their associated meanings available for those who use them. On such a view the study of natural language should take the form of (a) studying the syntax and semantics of a language as a self-contained system, in abstraction from what uses are or can be made of it, and (b), building on the results of this part of the investigation, investigations of how speakers use this independently existing system. This is the classical semiotic hierarchy most often connected with the name of Morris, with the study of syntax presupposed by the study of semantics and the study of pragmatics (i.e. of the use of language) presupposing the study of both syntax and semantics. I have become increasingly convinced over the last few years that this methodology has its limits and that there is an urgent need for a new approach to natural language semantics, where the use of an expression by the speaker and its interpretation by the recipient are distinct, but coupled processes. Superficially this is more like the picture of information transmission in the

Shannon-Weaver tradition, though the differences remain more important than the similarities. Such an approach to linguistic meaning is forced to stick its neck out in ways that go well beyond the commitments involved in the currently received way of doing formal semantics, in which semantic values or logical forms are assigned to expressions in abstraction from their role in the coding and decoding of verbal messages. In particular, we must not only be prepared to make assumptions about the form of the *results* of interpretation, but also about the forms in which thoughts are available to the speaker at the point when she encodes them in words. DRT, as a theory that tells us something about the form of the semantic representations that interpreters extract from the verbal inputs they receive, offers some specific proposals about the final results of the communication process, those established at the receiver's end. But we also need concrete proposals for the semantic representations of thoughts that stand at the beginning of the communication chain—those ones the speaker turns into the words that she thinks best serve her communicative purpose. One way to deal with these different aspects of verbal communication is to assume that the thought representations that serve as input to language production are the same as those that result from interpretation. That is no doubt a hazardous assumption, and one that won't be true in general without various qualifications. But for the analysis of a range of problems, especially those having to do with reference—of specific indefinites, definite descriptions (in connection with the referential-attributive distinction), proper names and the various uses of demonstratives—distinguishing between the production and the interpretation of such expressions and then looking at how they are coordinated in actual communication appear to me now as essential to further progress.

There is one aspect to information and information flow in verbal communication that has acquired a status of its own. This is what in current linguistic semantics and pragmatics is usually referred to as 'information structure'. 'Information structure' too is a term with almost as many meanings as users, and its use varies in particular with regard to what it is taken to include. What I take to be the common core to all uses in the current formal semantics and pragmatics literature is the division of information into focus and background. Here the basic agenda was set by Mats Rooth's 1985 dissertation.[37] Our understanding of the semantics and pragmatics of focus,

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[37] Rooth, Mats, 1985, ***Association with Focus***. Ph.D. dissertation, University of Massachusetts, Amherst, MA.

and of its prosodic, syntactic and morphological realizations in different human languages, have made steady progress over the past 25 years, though there still is much that remains to be done. Usually information structure is meant to include more than just the focus-background distinction.

Another notion, or set of notions, that have to be distinguished from the focus-background division, is that between topic and comment; but there are different notions of topic, and with that, corresponding notions of topic-comment division. Topic notions differ from each other along several dimensions. One of these dimensions is the size of the unit that a topic is the topic of. In particular, there is a notion of sentence topic that interacts with focus in important ways, which were first mapped out in the work of Daniel Büring.[38] His work has taught us that systematic investigations of the semantics and pragmatics of focus soon reach a point where the interaction with the topic-comment distinction has to be taken into account as well. But apart from sentence topics there are also more global notions of topic—topics in the sense of 'topics of discussion', where there is a close connection with topics as issues or questions under discussion. The study of this notion of topic is part of the study of the structuring of various types of discourse and texts and has quite different ramifications, which reach into different areas of pragmatics.

Finally, the theory of linguistic information structure is sometimes understood as including the distinction between presupposed and asserted information. Presupposition has played a central part in the development of DRT, largely through the crucial contribution made by Rob van der Sandt and Bart Geurts around 1990.[39] Van der Sandt argued that anaphora (including donkey anaphora) and presupposition are just two sides of the same coin and suggested that DRT was a natural framework to deal in a uniform way with both. But with this proposal comes a fundamental revision of the overall architecture of DRT. DRSs are now constructed in two stages: (i) a first stage, in which a 'preliminary' representation is constructed for the current sentence; this preliminary representation contains explicit representations of all presuppositions triggered by presuppositional elements in

[38] Büring, Daniel, 2007, Semantics, intonation and information structure, in: Gillian Ramchand and Charles Reiss (eds.), ***The Oxford Handbook of Linguistic Interfaces***, Oxford University Press, Oxford, 445–473.

[39] van der Sandt, Rob, 1992, Presupposition projection as anaphora resolution, ***Journal of Semantics***, 9, 333–377.

Geurts, Bart, 1996, Local satisfaction guaranteed: A presupposition theory and its problems. ***Linguistics and Philosophy***, 19, 211–257.

the sentence, including the 'referential' presuppositions triggered by pronouns and other definite noun phrases; and (ii) a second stage, in which the presuppositions of the preliminary representations are resolved, and the representation that results from the resolution is merged with the context representation. Furthermore, when anaphora is treated as a form of presupposition the original motivation in early DRT for constructing DRSs top down—the online processing of anaphoric pronouns—disappears. Largely because of this, the top-down method has in practice been almost universally replaced by bottom-up algorithms. This has been true in particular for me and for those with whom I have cooperated on DRT-related projects during the past twenty-odd years. Within the setting of DRT, the presuppositional/non-presuppositional opposition therefore has a different status when compared with that of other aspects of information structure: it is an integral part of the architecture of DRT as it is now most commonly used. To represent other information structural notions, such as the focus-background opposition or the various notions of topic, additional representational provisions have to be introduced into the DRT formalism. Some of the needed extensions of the formalism are already in place, but there is quite a bit that remains to be done.

AtM: The concern in SS with information flow led eventually to a separate development of Situation Theory, viz. Channel Theory. I myself don't know whether Channel Theory has proven useful in computer science or mathematical logic and model-theory. But I have the impression that it has not had much of an impact on work in linguistics. Linguists like to have handy toolkits that they can apply to new problems. But SS never gave us much of such a practical toolkit, honed for application to linguistic problems. This also relates to more general questions of reception. As far as visibility was concerned, there seems to have been quite a difference between SS and DRT in the early days. Whereas SS was widely hailed, especially within philosophical circles, DRT had less visibility at that point. In fact, there seems to have been considerable resistance to it. Do you have an explanation for this difference?

HK: I have kept up neither with the more recent developments of Channel Theory nor with its reception within the linguistic community. So this is something I do not dare comment on right now. But as far as the early reception of SS and DRT is concerned, I think there are a number of things that I can say. First, there is a difference in the scientific communities that the two approaches address and/or to which they have something useful to offer, or to which they would have liked to have something to offer. The fundamental issues that SS raised have a much broader appeal. At least they were seen as such at the time, and, I think, rightly. SS was concerned with questions about the central ontological categories of thought and language and thus with the content and nature of the propositional attitudes, and it presented these issues in a form that had immediate appeal to the philosophical community. And there were a number of points SS was making that seemed important and right and that could be appreciated without detailed technical knowledge about earlier work such as Montague Grammar.

In my own recollection there was in particular the paper I mentioned already, which Barwise had written before he joined the Stanford faculty and which I thought was marvelous both in its concrete accomplishment and in the way it was pointing ahead. This was his *Scenes and Other Situations*, in which he shows that situations, pieces of the actual world, must be invoked as the semantic values of the infinitival complements of naked-infinitive perception verb constructions, as in *John saw Mary kiss Fred*. This paper made a strong case for a radical revamping of semantics in general, in which situations play a central role, and a double one: on the one hand, as semantic values of such expressions as the infinitival complements of perception verbs and, on the other hand, as indices, taking the place of possible worlds. This was widely seen as nothing short of a revolution, welcomed among other things, because it held the promise of doing away with a notion that many philosophers regarded with much suspicion, viz. possible worlds, and replace it with entities, viz. situations, that for the most part were much more narrowly circumscribed, and therefore much more tangible and tractable. Thus SS promised a healthy realism to take the place of what many saw as the murky metaphysics of possible worlds. I think it is right to say that in the end quite a few of the initial supporters of SS came to see it as failing to fulfill this promise. There are a number of reasons for this disappointment that come to mind. The first is that one of SS's original cornerstones, viz. Barwise's analysis of naked infinitive perception sentences, came to be seen as less uncontroversial than it had appeared initially. This was because of the alternative proposal first made by Jim Higginbotham, according to which the contributions of infinitival complements to the truth conditions of naked-infinitive perception sentences are the *events* that these complements describe.[40] As events became increasingly important in semantics and the need to acknowledge them more widespread, this alternative has gained in popularity, also because it provides a natural

[40] Higginbotham, James, 1983, The logic of perceptual reports: An extensional alternative to Situation Semantics, ***Journal of Philosophy***, 80, 100–127.

explanation of the fact that the infinitival complements of perception sentences must be descriptions of events and cannot be descriptions of states. The second factor was that the use of situations to replace possible worlds as indices of evaluation turned out to be something less than the unequivocal step forward that it had looked like at first. One problem here is a matter of detail. What is needed is a formally explicit theory of situations. Barwise and Perry and some of their collaborators saw this need clearly and took great pains to deal with it. Situation Theory was soon distinguished from Situation Semantics and its first task was to provide a formal axiomatic basis of what situations are. More often than not, when things get more formal and technical, popularity wanes. That is an old story, which has made many scientific theories disappear from general view just because they became too sophisticated to be understood and appreciated by anyone unwilling to take the trouble and time to work through the formal details.

But there was also another problem with the use of situations as indices. This problem has to do with the fact that the space of situations is much richer and more complex than the space of possible worlds. On the one hand, this was something that the situation semanticists wanted—for one thing, because it promised a finer individuation of propositions and therefore a better account of propositional attitudes. On the other hand, too much richness can be a mixed blessing. To repeat what I said earlier, too much richness in a framework for doing semantics means that the formalism may fail to give you any useful guidance to what the linguistically important semantic constructs are and what form the semantic analysis of linguistic forms should take.

AtM: How does all this relate to your infamous, but never published paper *A Scenic Tour Through the Land of Naked Infinitives*? Was that an attempt to get closer to SS?

HK: Yes and no. It was, on the one hand, an attempt to become clear about what was involved in a formal situation-based semantics for some tractable formal language. The paper develops, for a modest extension of what is syntactically speaking just first-order predicate logic, a model-theoretic account of what it is for a formula of the formalism to be supported by a situation. This gives one, for each sentence *j* and each situation-based model *M*, as the *proposition expressed by j in M*, the set of those situations of *M* that support *j*. Since the logical consequence relation of Situation Semantics is weaker than that of classical logic—this was a central point for Barwise and Perry and it is confirmed by the formalization that '*Scenic Tour*' presents—the notion of propositional identity produced by such a semantics is finer than that generated by classical logic, i.e. it is easier for two sentences to express distinct propositions. Barwise and Perry had pointed to this fact as yet another point in favor of SS as compared with the possible worlds semantics of Montague and others: because propositions are more finely individuated, SS might offer a way out of the logical omniscience problem that besets intensional analyses of the propositional attitudes. At this point my paper turns into an argument against SS. The logic generated by the formalization of the support relation proves to be that of the strong Kleene semantics, which is, though weaker than classical logic, still much too strong to give a satisfactory solution to the logical omniscience problem. For instance, take the case of belief sentences with *that*-complements. According to the standard intensional treatment of such sentences the contribution that the *that*-complement to the truth conditions of the sentence as a whole is the classical proposition it (the *that*-complement) expresses. That gives rise to the omniscience problem because there are many cases of sentences S_1 and S_2 that are logically equivalent and hence express the same classical proposition, but whose equivalence is hard to see. It is possible for an agent a to sincerely assent to the one, but not to the other. In such cases the classical treatment entails that either both *a believes that* S_1 and *a believes that* S_2 are true, or else neither of them. That seems unmotivated and the problem is obviously that classical propositions 'collapse' too many distinctions. Since SS offers a finer individuation than classical semantics and logic, it may at one point have seemed natural to expect that this finer individuation could also block the unwanted collapse of the truth conditions of belief sentences. But, alas, it succeeds in this only to a quite limited degree: there remain many pairs of sentences S_1 and S_2 that are not only classically, but also situation-semantically equivalent, but whose equivalence is hard to see or establish. For such pairs the omniscience problem remains.

AtM: We have touched in a few places on connections between your work and cognitive science. On the other hand, you also mentioned computational linguistics more than once. Of course, for the last twenty years you were part of a department of computational linguistics, the IMS (Institut für Maschinelle Sprachverarbeitung) of the University of Stuttgart. The relation between cognitive science and computational linguistics always struck me as quite complex, and also as a topic of highly diverse opinions. Is there anything you can say about this relation?

HK: I am surely not the best person to ask about this. But here are a few things that come to mind. First, it is important to realize that computational linguistics is and has been many things to many people. As is so often the case with a new discipline that gets its inspiration from very different neighbors and tries to cope with their respective expectations, it has gone through different fashions, some of which have become long-lasting methods or perspectives, while others have more or less disappeared from the scene, perhaps lingering in obscurity and waiting for a revival when their time will have come again. One of the disputes that was high on the agenda of many computational linguists in the 70's, 80's and 90's and that also had its run at the IMS, was precisely this: What was, or could be, or should be, the relation between computational linguistics and cognitive science?

That dispute took more than one form: Should computational linguistics —as a science, rather than an engineering enterprise—be the study of how human beings carry out the computational tasks involved in language processing, or should it identify, from a strictly functional perspective, what computations are needed to carry out certain given language-related tasks and then design the most efficient algorithms to implement those tasks? But among those who defended the second position, there was a further point of debate: Would the most efficient algorithms be *de facto* the very same that are also used by human speakers, and would it therefore be useful to study human language-related cognition more closely, as probably the best way to discover what are the most efficient algorithms from a purely functional point of view? Or is the starting position of the computer, with all its resources—online word lists and thesauri, vast amounts of encyclopedic knowledge and so forth—so different from that of us humans that its best language processing algorithms would be unlikely to have much in common with the ones employed by us? In that case the study of human cognition would be useless even as a means of discovering useful clues of what the best language processing algorithms for computers might be like. These disputes never got resolved, even if they helped to clarify where everyone stood. But perhaps the most noteworthy aspect of all this was that at some point the issue simply went dormant. The reason for that was that, during the time when the debate was taken seriously and thought to be important, there had been a broadly shared understanding of computational linguistics, presupposed by all sides participating in it, that computational linguistics was about the computation of linguistic representations of the kind proposed and discussed in computational linguistics. With the rise of statistics in computational linguistics—which is now as prominent at the IMS as it is in all other major centers of computational linguistics—the debate has become marginalized, because the rough consensus about what representations are to be computed exists no longer. The debate between 'statistical CL' and 'logic-based CL' is an even more fundamental one: What counts as a ‘representation’ of linguistically relevant information? Or is even a liberal interpretation of the term ‘representation’ misleading in this context? Is the information about linguistic expressions that is identified and used by statistical CL algorithms, and perhaps even by us, of a fundamentally different character than theoretical linguistics and ‘logic-based CL’ have long been assuming? In other words, the fundamental debate has become even more fundamental than it was. It is now about the nature of information in general, in a setting where information in the form of the representations traditionally assumed in ‘logic-based CL’ is only one among several options.

AtM: Has the IMS ever been involved in natural language systems for artificial agents, I mean robots?

HK: That is an interesting and most relevant question. Actually at the IMS not much of that kind of work is going on. There have been some speech recognition projects, dealing with the problem of filtering out background noise, crucial to applications of automated language processing in speech-based communication between drivers and their vehicles. Actually, we do not usually think of cars as robots. But many of the issues that make robotics so important for theoretical linguistics are already manifest in the exchanges between drivers and their vehicles. What distinguishes the problems that arise in connection with verbal communication with robots from problems in other areas of computational linguistics is that robots have other information channels, besides the ones used for the transmission of language. The point of being able to talk to your car as you are driving is, on the one hand, that the car has non-linguistic means of acquiring information—the car may be equipped with sensors that take in information about the road, and the conditions of travel, including what is delivered by its navigation system that at a minimum can identify its current location. The car’s language facility should understand verbal requests for such information and answer on the basis of the information it has gathered, or look for that information first, in case it finds that it doesn’t have it. On the other hand, the car will also have some of the features of an agent. It can perform certain actions, such as switch off the air conditioner or open the sunroof, and it should be possible to tell it in words that one wants it to do such things. Finally, in really sophisticated systems the car should be able to decide to initiate a verbal exchange, perhaps just by telling the driver something that it thinks he ought to know. But such an initiating utterance by the car could start an ensuing dialogue and the car should be prepared for the reactions such initiations are likely to provoke, and ideally also be able to deal with less expected reactions.

What such a robot must be able to do with language as an integral part of its cognitive system confronts us with issues that are very different from those that have so far been addressed within formal semantics. Let me give an example from a project, led by Alois Knoll from the Technical University in Munich, the task of which was to develop a natural language facility for a robot, who is to put together a wooden toy plane from various building pieces, including slats, screws and bolts. Such a robot must be able to connect verbal predications, as we find them in, for instance, a sentence such as *A slat with three holes is lying behind the green bolt*, with information that the robot takes in through its built-in camera. That is, image recognition, in the sense of converting what is registered by optical sensors into a verbal description of it, is essential to the language component of such a system. The same is true of other modules that process sensory information and that must make this information accessible to verbal realization. The converse of this, being able to picture a scene on the basis of verbal input involving such information, or updating such a picture on the basis of new verbal information of this sort, is equally important. This problem, of the interface between visual and verbal representations, is one that, if I am right, people have been aware of for a very long time. But it wasn't until the eighties that the first formally and computationally precise proposals were made about how the two forms of interpretation might differ and how they could nevertheless interact. I am thinking here of the work by Barwise and John Etchemendy, which they presented as an interactive program that could be used as a kind of logic tutor. Their system *Tarski's World* is one in which reasoning can make use of both geometrical and propositional representations, where propositional representations are basically just logical formulae built from elementary clauses of the form $red(a)$, $behind(s, b)$ and so forth.[41] A lexicon that enables the language processing system to link its atomic predicates to information accessible via other channels is just one of the many demands placed on the language modules of robots with sensors. It is paradigmatic for the kinds of problems that have to be solved, if we want to take the notion of equipping robots with language seriously.

AtM: Some people have stated the ambition to build a robot that people can interact with without recognizing that it is a robot—a robot that can pass the Turing Test. What can be said about that ambition in the light of the difficulties of which you have just spoken?

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41 Barwise, Jon and John Etchemendy, ***Tarski's World.*** http://ggweb.stanford.edu/tarskisworld

HK: It is important to keep in mind that Turing's test is supposed to apply to computers, and not to robots: The person who is to decide whether he is interacting with a robot or with another person is communicating with his interlocutor only by typing in messages and seeing messages displayed on his console. There is no other interaction between the two—nothing remotely like when, say, a human and a robot are in the process of jointly performing a complex task and talking to each other in order to be able to perform the task better. Tricking a human into believing that he is dealing with a human interlocutor is at least a possibility. There have been decades of debate over how hard or easy it is for a computer to fool the one communicating with it via a keyboard and console and over what can be concluded, when the computer succeeds. But the point is that the only way in which the human partner and the computer interact in the classical Turing Test are the messages that the partners send to each other; they do not interact in any other way.

To conduct a meaningful conversation about anything in natural language, in which the partner shows the capacity of reacting adequately to things that we put to him and that could not have been predicted at that point of the conversation, so that the reactions cannot be simply canned, that is already a vastly more complicated task than Alan Turing seems to have thought. It is vastly more difficult in one part for the reasons mentioned in connection with robots. Producing and interpreting speech as relating to information that one has from some other source is a hard problem, even when that source is not sensory. Even building a natural language dialogue system that can sustain conversations about some topic, about which all the relevant information can be encoded without too much effort in digital form, such as chess, is much more than we can chew off at present, even now, close to sixty years after Turing conceived his test. As far as the applicability of Turing's test to robots is concerned, here we have the obvious problems that—so far at least—robots look so different from human beings: You know that you are dealing with a robot before the poor thing has had a chance to say or do anything. Trying to build a robot that can fool the judge to the point of there being nothing that distinguishes his partner in action and conversation from a human seems as pointless, as it feels creepy. For the kinds of robots we are likely to interact with in the future it would seem that the closest one can get to a positive Turing Test result is that the judge develops a personal attitude to it, treating the robot as another person and perhaps being unable to do otherwise. But that is a long shot from being convinced that one is dealing with a human person.

AtM: Information from linguistic input is used in reasoning by combining it with information from other sources. As you were implying, a robot must be able to reason with the information it gets from what you are saying to it in combination with information it has or can acquire through other channels. But isn't this ability to reason with information obtained from linguistic input something we expect from automated language processing systems generally?

HK: When your system uses formulas of predicate logic as logical forms for natural language sentences and discourses, of course it can use any kind of theorem prover for predicate logic: you convert your inputs into predicate logic formulas, let the theorem prover carry out its deduction and reconvert the conclusion into natural language. This is a very old recipe, as already in the eighties there were projects that tried to combine this idea with DRT in that natural language would be turned first into DRSs, these would then be translated into formulas of predicate logic, the theorem prover of one's choice then let loose on those formulas and the conclusion turned back into natural language with or without the use of DRT. A propos, you can also develop deduction systems that operate directly on DRSs. Uwe Reyle and I did this in the early nineties for the DRS language of Chapters 1 and 2 of FDTL. But, of course, since that DRS language is equivalent to first-order predicate logic, and there are very simple translation algorithms for turning DRSs of this language into first-order formulas and conversely, formulating such a DRS-based deduction algorithm can't be hailed as a particularly telling result.

But how useful can such deduction modules be, when the task is to provide a robot with reasoning facilities that are able to interact in the right way with its language processing modules? Much of the reasoning on which we humans depend is not deductively valid. It is non-monotonic in one way or another and often hard to distinguish from what might be described as our learning new information, expressed in the conclusion that we adopt. For the robots we have been talking about, the matter would be no different. One of the important and exciting lessons of Learning Theory that seems pertinent here is that it may well be that there is no principled distinction between learning and reasoning. Within the general setting of information acquisition and use, instances of traditional deductively valid reasoning reappear as marginal and in a sense degenerate special cases of something much more general. This is a development about which I know far too little to speak about it with confidence. If the perspective on reasoning it suggests is right, the importance of deduction systems for AI and Cognitive Science is probably much smaller than some logicians, myself included, have long liked to believe.

There is also another point that should be mentioned in this context. The DRS languages we need for natural languages transcend the limits of first-order logic. The need for such higher-order DRS languages arises in particular, when plurals are taken into account as well as singulars, as argued in detail in Chapter 4 of FDTL and more recently in our DRT survey in the 2nd edition of the ***Handbook of Logic and Language***.[42] Since complete proof systems are impossible for second- and higher-order formalisms, the best we can hope for from a proof system for such a formalism is that it cover all that human reasoning will ever use. But how will we, as theoreticians, know when enough is enough? The classical picture of the role of deductive logic in information processing is hence being eroded from two sides. On the one hand, we cannot hope for systems capable of proving all that is deductively valid and, on the other, the reasoning systems that robots, like we, really need must be in a position to draw inferences that are not deductively valid, hence transcending the bounds of purely deductive logic.

AtM: DRT as I know it shares with other systems of Dynamic Semantics that it is strictly incremental: as discourse processing goes on, DRSs only grow and nothing ever gets erased. That seems unrealistic in general and especially so when we think of dialogue. When people talk to each other, they often argue, they make conflicting statements, and, hopefully, eventually one will persuade the other and the other will withdraw the claim he had made. Is there a way to model such conversational processes in DRT? And how much does that affect the central assumptions of the theory?

HK: There is a whole range of interesting questions here that this touches on and that haven't yet been discussed. To start with, yes, of course updating isn't always monotonic in dialogue, and the same is true, by the way, if less prominently, in spoken monologues and written texts. But it is important to distinguish between different ways in which this can happen. Probably the most common type of case, and the one you seem to be thinking of, is that of argumentative dialogues, in which the participants argue over the truth or tenability of a certain claim or position. The aim of this kind of verbal exchange is that the discussants come to an agreement on the point

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[42] van Eijck, Jan and Hans Kamp, 2010, Discourse Representation in Context, in: Johan van Benthem and Alice ter Meulen (eds.), ***Handbook of Logic and Language***, 2nd rev. edn., Elsevier, Amsterdam, 181–252.

at issue, either because one convinces the other of his position or because the debate ends inconclusively, with each of the participants sticking to his position, or because a kind of Hegelian compromise or 'synthesis' is reached, to the effect that both are aware that what they were arguing over meant something different from what they thought, or that it really didn't mean anything at all, when you come down to it. Let us focus on the case where either participant A convinces participant B of his position, or vice versa, and assume—quite implausibly, but we will come back to this—that the dialogue consists entirely of assertions. How do we describe such a dialogue? A kind of minimal proposal is to restrict attention to that information which is publicly accessible from the overt execution of the dialogue—that is, from the utterances the participants actually perform. We can think of such information as appearing on a kind of Lewisian scoreboard, consisting of three parts: (i) a component containing the statements that have been made by A, (ii) a component containing the statements that have been made by B and (iii) a component containing the publicly accessible common ground, as it gets established in the course of the conversation. The task then is to describe how a new utterance by either A or B modifies the information on the scoreboard. The new scoreboard will have a representation of the last speaker's utterance added to his scoreboard component and, depending on how the other speaker reacts to the utterance, possibly also to the common ground component. One of the things a speaker can do in this dialogue model is to retract a claim previously made. For the sake of simplicity, let's assume that such retractions take the form of the speaker asserting the negation of the claim *S* that is withdrawn. The effect of this on the scoreboard will then be that the representation of *S* is removed from the speaker's component on the scoreboard and replaced by a representation of neg-*S*. And in case a representation of neg-*S* is also part of the other speaker's component, neg-*S* will also be added to the common ground component. Retractions of this kind need not involve removing material from the common ground component. But that kind of retraction is possible as well. For instance, a participant may retract a claim he made earlier on, that was accepted by his interlocutor at that point, so it was added to the common ground. The speaker's present disclaimer will have the effect that the earlier claim is now removed. Besides these two types of non-monotonic scoreboard evolution, there may be yet others that could also be described in this simple dialogue model. But the more important point is that the model oversimplifies the reality of actual dialogues in a number of ways. First, it is rare for conversations to consist exclusively of assertions. In fact, such dialogues hardly ever occur in real life. Conversations are almost always made up of utterances of different speech act types and a proper record what the participants in a dialogue say to each other should record the speech act type of each utterance as well as its propositional content. This already makes the components of the score board more complex; but in addition, in a coherent dialogue there will be certain relations between the utterances of A and B, such as, for example, when A asks a question and B reacts to that by giving an answer to it. These relations are as a rule also part of the overt, accessible information pertaining to the discourse and should be recorded too. This entails that the order in which the participants make their respective contributions will have to be recorded as well.

A more ambitious project of dialogue description is one which includes in the description of the successive stages of a dialogue not only a record of the publicly accessible information, but also partial descriptions of the attitudinal states of the participants. These richer descriptions provide a basis for an analysis of how dialogue partners can draw conclusions about each other's beliefs, desires and intentions on the basis of what the other is saying. Much of what is involved in drawing such conclusions is or involves Gricean reasoning with implicatures, but there is more. For instance, the reasoning involved in the recognition of indirect speech acts or the interpretation of novel metaphors. Such inferences are for the most part defeasible, which may give rise to further cases of non-monotonicity. I mention this just to give some indication of how much is arguably involved in describing what goes on in the course of a conversation and what a description of it must include if it is to enable us to capture the Gricean forms of pragmatic reasoning that are widely seen as a central part of linguistic interpretation and *sine qua non* for the theory of meaning. In Stuttgart we have been working for many years on extending DRT so that it can supply the descriptions of the public and non-public components of dialogue stage descriptions I indicated above. Since the nineties we have been making use of an extension that permits detailed descriptions of complex mental states, consisting of propositional attitudes and entity representations. An extension in which one can represent the speech act types of utterances together with their content has come a long way too. This work has not yet been published in any form. But these are only some of the formal tools needed for the rich descriptions of dialogue and conversation stages we have just been talking about. A general theory of conversation—or, for a start, a theory of dialogue—which articulates how utterances determine the transitions between successive stages that result from them is yet another matter. If such a theory is to be formulated in a DRT-based framework, then the notational extensions I mentioned are indispensable. But that is only the beginning. Much more will be needed in addition and by no means all of that is in place. You may feel that I have strayed quite far from the question you asked. And you wouldn't be wrong. But my reasons for saying all this is to convey my conviction that, if we want to take the kind of non-monotonicity you are talking about seriously, all these different aspects of the activity of conducting a conversation will have to be brought into the picture. At least in the context of natural language semantics non-monotonicity is, I think, always a matter of belief revision (or, more generally, a matter of changing attitude). That is why the problem of developing a satisfactory theory of conversation that can deal with its non-monotonic aspects is so very hard. In fact, it isn't just a problem that is hard to solve. The first hurdle is to find the right way of stating it.

AtM: These are all extensions of DRT that could be carried out by looking only at one natural language. But what about linguistic variability in semantics?

HK: So far the explicit use of DRT in cross-linguistic semantics has been quite limited. But there appears to be a growing number of linguists among those working on semantics in a cross-linguistic setting, who are becoming convinced that the dynamic phenomena DRT was designed to make sense of are quite prominent in many non-western languages, including, in particular, those in which sentence boundaries are less well-defined and therefore much less easily identified than is the case for languages like English, French or German. Some kind of dynamic framework is, for all we now know, absolutely indispensable to describe how form and context determine content in such languages. Arguably the most prominent representative of this new dynamic trend in the semantic description of broad spectra of different human languages is Maria Bittner. Bittner has developed a mode of semantic representation—involving her 'infotention states', which is importantly different from DRT, but which has nevertheless incorporated some of its central ideas.[43] She has also launched the idea of bringing out a collection of papers representing different forms of and perspectives on dynamic semantics, that should clarify how the different systems differ from each other and what options they offer the working semanticists who need a framework to describe the dynamic phenomena that they have encountered in the language or languages they are working on.

[43] Bittner, Maria, 2008, Aspectual universals of temporal anaphora, in: Susan Rothstein, (ed.), ***Theoretical and Crosslinguistic Approaches to the Semantics of Aspect***, Benjamins, Amsterdam, 349–385.

AtM: Quite a few of our colleagues think of you as the one who invented DRT and then did much to develop it further. As this selection of your papers shows, that is a rather limited view. But it does seem that over the last thirty years DRT has played a very large part in your work. Can you say something about how you see the future of DRT?

HK: Yes, it is definitely true that DRT has been of central importance to me over these last three decades. I guess the main reason why it has been so important is that for me DRT is the crystallisation of a quite radical change in my way of thinking about language, about its relation to human thought and about the ways in which it functions in human communication. And the changes that DRT has seen over the years are largely a reflection of my thinking about these questions.

The version of DRT that I believe is most widely known is no doubt the one presented in our book ***From Discourse to Logic***. But much has changed since the time, almost twenty years ago now, when FDTL was published; the versions of DRT that we at the University of Stuttgart have been using for at least a decade differ from the FDTL version in several quite fundamental respects. Not all changes have been motivated by the ideas I just alluded to. For instance, one important change is that in the current versions DRSs are constructed bottom up and that their construction involves two stages—first, construction of a preliminary DRS with explicit representations of all presuppositions and then resolution of those presuppositions in the context. These changes were made following van der Sandt and Geurts, who in the early nineties modified DRT to fit van der Sandt's treatment of presupposition justification as a form of anaphora resolution.[44] And another important difference of one of the DRT versions in current use is one in which DRS construction doesn't start with semantic entries for lexical items, but 'further down'—with the semantic specifications of lexical roots and the semantic contributions of sub-lexical functional heads. This change is motivated by work on the syntactic and semantic structure of the lexicon that has been undertaken by Roßdeutscher and the people in Stuttgart who are working with her.

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44 van der Sandt, Rob, 1992, Presupposition projection as anaphora resolution. ***Journal of Semantics***, 9, 333–377.

van der Sandt, Rob and Bart Geurts, 1991, Presupposition, anaphora, and lexical content, in: Otthein Herzog and Claus-Rainer Rollinger (eds.) ***Text Understanding in LILOG***. Springer, Berlin, 259–296.

But other important changes *are* reflections of the evolving ideas about the relation between language and thought and the ways in which language functions as an instrument in communication. One of these changes has been in place for quite some time now and descriptions of it can be found in the existing literature.[45] This change involves two steps: first, using DRSs as building blocks in a formalism for the description of complex mental states, consisting of a collection of propositional attitudes, as well as a collection of 'entity representations', which can serve as constituents in the propositional attitudes; and, second, an extension of the DRS formalism itself with conditions that attribute complex mental states described in this way to some agent at some time.

In more recent work, starting as joint research with Agnes Bende-Farkas on specific indefinites around 2000, the assumption that the DRSs constructed from linguistic input capture important features of the mental representations constructed during interpretation is extended to a similar assumption about the representations of thoughts that people turn into words when they express them in speech or writing.[46] I am as aware now, as we were then, of the dangers of such a step. The assumption that DRSs, or structures that contain DRSs as building blocks capture important aspects of mental representation is already one that can be met, and has been met, with considerable scepticism. But in assuming that these same structures also serve as inputs to language production we are surely sticking our necks a good deal further out. But hazardous as these assumptions may be, they have, I think, proved quite fruitful in dealing with a number of linguistic and philosophical issues, and we may as well hang on to them so long as no alternative, based on more solid evidence, is available to us.

Hypotheses about the form in which information is present to the mind both as input to language production and as the result of language interpretation are important insofar as they enable us to describe verbal communication as a process in which the recipient tries to build a representation from what he hears or reads which matches the intention of the speaker.

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[45] Kamp, Hans, 2003, Einstellungszustände und -zuschreibungen in der DRT, in: Ulrike Haas-Spohn (ed.), ***Intentionalität zwischen Subjektivität und Weltbezug***, Mentis, Paderborn, 209–289.

Kamp, Hans, Josef van Genabith and Uwe Reyle, 2011, An updated survey of DRT, in: Dov M. Gabbay and Franz Guenthner (eds.), ***Handbook of Philosophical Logic***, Second Edition, Vol. 15, Springer, Berlin, 123–349.

[46] Kamp Hans and Agnes Bende-Farkas, 2001, ***Indefinites and Binding: From Specificity to Incorporation.*** Lecture Notes for the 13th European Summer School for Logic, Language and Information (ESSLLI 2001) at the University of Helsinki, Finland, 13–24 August, 2001.

This way of describing the function of language is particularly useful in connection with a range of issues about reference, about how the different types of definite noun phrases of a language such as English succeed in transmitting reference in thought from speaker to addressee, and about what guides speakers in choosing one such reference device as opposed to another.

One would expect that such a 'bi-polar' approach to meaning transfer should not just be important in connection with reference. We can see it as a new setting for making formal sense of the Gricean perspective of linguistic meaning as 'non-natural meaning' and of the idea that success in communication is achieved when the addressee captures the communicative intentions of the speaker. It is a general feature of verbal communication that the speaker has to be careful when choosing the words she uses to get her message across and that she is guided in her choice by what she thinks is most likely to work given what she can expect her audience to know. Questions of this sort—whether the speaker's words will do what they are supposed to do and how she goes about choosing words that she thinks will do the job—arise for instance quite prominently in discussions of 'tropes' such as irony and metaphor; but so far these discussions have been necessarily informal, since there has been no satisfactory formal framework within which they could be made explicit.

Thinking about meaning and communication along these lines leads inevitably to the question what it is for two or more persons to share information, and how verbal communication can lead to information sharing. Accounting for this central aspect of communication—the *intersubjectivity* that it is capable of creating and on which it also relies—seems to me to be one of the great challenges for any theory of meaning and communication. Information can be shared without having to be objectively true. And this can happen in more than one way: in cases where those who share the information are subject to a shared illusion—each wrongly takes shared information to be true—and in cases where it is clear to all involved that what is shared isn't true objectively, as when we are engaged in fictional discourse.

AtM: Formal semantics originated in the sixties and became firmly established as a well-defined discipline in the seventies. What differences are most salient to you between natural language semantics as it was done then and how it is pursued today?

HK: The particular section of the philosophical and logical community by which I was formed and socialised in the second half of the sixties was what has since often been referred to as 'Southern California Semantics', and as I indicated earlier in this interview, the two most important strands of that socialisation were (i) the model-theoretic approach to natural language semantics initiated by Montague, and (ii) the operator-oriented approach to semantic questions, which in my case was represented primarily by Prior's work on Tense Logic, but of which there were also many other instances (such as e.g. Hintikka's modal-logical approach to knowledge and belief). In both kinds of work we can, from the present perspective that has been shaped by developments over more than four decades, clearly discern the influence of ideas that came from formal logic and that nowadays are widely seen as alien to the way in which meaning is realised in the languages we speak. Montague's work contains ways of dealing with English syntax which could be seen as implementations of his dictum that 'syntax is the handmaiden of semantics', in the more provocative and more problematic interpretation of that phrase. And the attempts to capture aspects of natural language meaning in terms of Tense Logic and other variants of Modal Logic were inspired by the idea that many parts of speech in natural languages function as modal operators—that natural languages were much more like modal logics than we have come to think they are since then.

As I see things now, a significant part of the developments in natural language semantics during the seventies had to do with freeing ourselves from the sway of these alien conceptions. A more balanced and refined understanding of the relation between syntax and semantics was due largely and crucially to the work and personality of Barbara Partee. It is thanks to her that Montague's model-theoretic approach could have become the dominant method in formal natural language semantics that it is today. And she made that possible because her insight that the model-theoretic method which Montague had succeeded in applying to natural language was crucially important to semantics went hand in hand with that knowledge of generative syntax that enables us to see the syntax of natural languages as having a certain autonomy. Its structures are not only motivated by the meanings that they serve to support, but are partly determined by principles that are authentically syntactic, principles that have to do with questions of form and questions of form only. And then of course it was also essential that Barbara had already established for herself a reputation as an authority in English syntax.

In rethinking and redoing Montague's own work in ways that do justice to these insights about syntactic autonomy, and by gradually extending those results to cover an ever larger range of semantic phenomena in an ever larger number of languages, Montague Grammar has become what it is today. It constitutes a theory of the relationship between form and meaning that acknowledges both form and meaning as realities in their own right, each subject to its own laws and principles which reveal themselves through mostly complex and partly independent webs of empirical evidence, but nevertheless are bound together in the tight and systematic manner without which human languages could be neither learned nor used. That connection is usually described as the 'compositionality' of natural language.

I cannot resist adding at this point an observation that has been with me ever since it was made clear to me that syntax is significantly autonomous. If syntax is the handmaiden of semantics, she comes to this task with properties of her own, of which it isn't self-evident that they are optimally suited to this task. The bulk of work in natural language semantics over the past decades has consisted in dealing with questions that have to do with the syntax-semantics interface; questions of the sort: 'How is it possible for this syntactic construction to carry this kind of meaning?' In fact, questions of this sort have been the main preoccupation of natural language semanticists, and they will undoubtedly continue to be a central concern for the foreseeable future.

But why should that be so? Why don't the syntactic forms of natural languages show the kind of semantic and logical transparency with which we are familiar from the languages of formal logic, such as the predicate calculus or the lambda-calculus? From this perspective—and thus from the vantage point of the formal logician—it easily looks as if the human race has made things unnecessarily difficult for itself, by developing languages in which meanings are expressed by syntactic means that seem to lack the kind of transparency that formal languages so clearly and admirably display. Why is it that humans have made things so difficult for themselves?

As a matter of fact I do not really endorse this last question. I am somehow convinced that the way of looking at language that prompts the asking of it is wrong. But why is it wrong and in precisely what ways? I suspect that coming up with true and persuasive answers to that question is one of the hardest challenges for a general theory of language; but I also expect that good answers may teach us more than almost anything else about the nature and likely origins of human language as a tool for communication and for the clarification of our own thoughts, and about the ways in which language is interwoven with non-linguistic aspects of human cognition.

My own emancipation struggle in the seventies was primarily against the assumption that so much of meaning in natural language is expressed in the form of modal operators. As I mentioned earlier, my doctoral dissertation had been on an extension of Priorean Tense Logic. The aim of the dissertation was to design a Prior-type tense logic that was optimally expressive. Given that aim, the project of the dissertation could be described as a success. But in a way the very success of the project boded its own downfall. The main result of the dissertation was that the tense logic proposed in the dissertation is capable of expressing arbitrary temporal relations that are definable in the first-order theory of the relation of earlier and later. But when one looks more closely at *how* various such relations can be expressed in a language like English and compares those modes of expression with how the ways in which those relations can be expressed in the tense logic of the dissertation, then one cannot help being struck by the overwhelming discrepancies: the tense-logical paraphrases of the constructions that are used to express temporal relations in a natural language such as English bear virtually no resemblance to the constructions they paraphrase.

But the inadequacy of tense logic as a framework for dealing with the temporal dimension of natural languages goes further: A closer exploration of the various ways in which temporal relations are expressed in English and other human languages quickly reveals that there is virtually no linguistic justification for the idea that there is any role to play for tense operators in the semantic analysis of temporality in natural language. Many of the expressions we use in expressing temporal relations behave, by any reasonable criteria, as referring terms—terms that refer to times in the same way in which, say, as the name 'Barack Obama' refers to the man Obama. The most telling examples of such terms are dates—expressions such as *Wednesday*, *the first of October*, (*at*) *five to ten* and so on. But the referential function of temporal expressions is by no means restricted to them. If we take their referential appearance at face value, as I soon became convinced one should, then the semantics of temporal reference takes on a completely different character. Times are now part of the ontology of natural language, just as physical objects, persons and various other familiar and uncontroversial sorts of individuals.

Admission of times among the first class citizens of ontology wasn't the end of the story. The next step along the road of emancipation from the modal operator paradigm was the conviction that verbs and their various syntactic projections should be construed as describing and thereby introducing into the discourse events, processes and states. The arguments that led me to this second conclusion aren't exactly the same as those that led to the adoption of times as part of the ontology. But in spirit the arguments were quite similar; and the conclusions were similar too, in that they both led to a richer ontology than one would have had to acknowledge without them. For a model-theoretic approach of the kind that has become a kind of standard since Montague, these ontological extensions come with the challenge to say precisely what models which reflect such a conception of ontology must be like: What are the different ontological sorts that make up the universes of such models, what is the internal structure of each of those sorts, and how are different sorts structurally related to each other?

This task still is a major problem for formal semantics as we have it today. In fact it is nothing but the model-theoretic guise of the problem that, in the apt phrase of Emmon Bach, is often referred to as the question of 'natural language metaphysics'.[47] So far, precise articulations of natural language ontology have been fragmentary at best whether as part of model-theoretic natural language semantics or in some other setting. Some parts of it are in reasonably good shape. In particular, it seems to me that our understanding of the structure of time and its formal articulation as part of model-theoretic semantics is now about as deep as it needs to be. But already when we turn to events and states the issues are much less clear, and explicit articulations of ontological structure much more tentative. On the whole, most of the work in this domain, to the extent that it exists at all, has been limited to such ontological domains as time, space and mereology. But there is a vast range of ontological questions that will have to be clarified and articulated formally as we proceed to more ambitious systems of model-theoretic semantics, which cover larger natural language fragments, and which contain better approximations of the semantics of ever larger parts of the lexicon. A notorious problem present the many different kinds of abstracta that we find among the denotations of words: properties and kinds, laws, obligations and contracts, virtues and vices and so forth.

There are various resources that anyone who wants to address these aspects of natural language ontology can and should look at—from world knowledge data banks such as Cyc to more linguistically motivated banks such as WordNet or FrameNet. But in my own limited experience what can be found in those sources almost invariably needs to be carefully rethought and recast before it can be incorporated into the particular model-theoretic semantics that one is trying to develop. I expect that in the years ahead of us work on the ontology of natural language will reassert itself as one of central preoccupations of our discipline.

This answer is a very long one already—but then, this is a very big question!—and so far the only issue it has addressed is the relation between semantics and ontology. But that, of course, is only one of the issues that have been prominent in formal semantics over these past forty years and

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[47] Bach, Emmon, 1989, ***Informal Lectures on Formal Semantics***. SUNY Press, New York.

for which there nevertheless remains a great deal to be done. Some of the other big themes, both during these past decades and for the future, we have touched upon in this exchange as we went along: dynamics, hyper-intensionality, generation as an (imperfect) mirror of interpretation, the internal syntactic and semantic structure of expressions that dictionaries treat as words. But other topics haven't even been mentioned. Among them are a number of difficult general problems at what is often described as the 'semantics-pragmatics interface'. The precise form of different patterns of Gricean reasoning that interpreters use to compute implicatures is one such issue. A second one is if and how utterances can be factored into a content component and component which captures the speech act type of the utterance: which elements of an utterance should be treated as constituents of the content it expresses and which as indicators of the type of speech act that is being performed? Another big and difficult problem is vagueness, to which linguists and philosophers keep returning, but which has proved remarkably recalcitrant.

Perhaps the biggest challenge of all is the still poorly charted field of non-literal meaning, including metonymy and metaphor. I think existing work on metaphor has made it quite clear that there are considerable differences between phenomena all of which have been described as metaphoric. What we need for a start is a finer, well-motivated classification of the phenomena—that is an indispensable foundation for any account of metaphor that is to be both comprehensive and formally precise. But to my knowledge such a classification is still outstanding, not to speak of a theory that it could serve as foundation.

A particularly irksome feature of metaphorical uses of language and of the creation and non-literal extension of lexical meaning is what might be called their 'semi-productivity': Sometimes it is possible to predict on the basis of fully general principles how the semantics of a word can be extended from its literal to its non-literal meaning, or how new words can be formed with a particular form and meaning. But in general such precise predictions are not possible. On the other hand, even where exact prediction is not possible, it is nevertheless as a rule quite clear that not everything goes. Sense extension, introduction of new words and other aspects of metaphoric language use are constrained by general principles. But these principles only determine outer bounds within which sense extension, creation of new words etc. are possible. These principles do not define, they only confine meaning. How the meanings of certain words are extended and what new words get introduced into a language, and when, are for the most part idiosyncratic.

For formal semantics, accounting for this kind of semi-productivity is a particular challenge. It is a challenge that in some ways is comparable to the challenge posed by vagueness: How can we be formally precise about what is, by its very nature, the very opposite of precise? For vagueness quite a bit of progress has been made with this challenge in the course of the past forty years or so. Here some creditable methods have been developed for describing the imprecise in precise terms. But no comparable frameworks have thus far been worked out to deal with the problems of sense extension, creation of new words and non-literal meaning more generally. If we had such a framework we would probably have a much better and deeper understanding of linguistic meaning, of its fluidity, flexibility and dynamics, than we do at present. But we don't; and developing such a framework may well prove to be the greatest challenge of all that semantics is facing today.