JON BARWISE AND JOHN PERRY

# SHIFTING SITUATIONS AND SHAKEN ATTITUDES

*An Interview with Barwise and Perry*

## INTRODUCTION

Charged with the task of getting a response to the commentaries on their work, I met with Jon Barwise and John Perry on one of those lovely California May afternoons. We first met in the Stanford Philosophy Department. Housed in a dark, cramped, building that has seen better days, the department reminded me of nothing so much as the set of *Barney Miller*, with various conversations going on in the cramped offices and spilling out into the hallways, on assorted odd topics like whether fetuses have sensations, whether an interview is one event or hundreds, whether Lisp expressions can simultaneously cause activity, describe procedures, and denote mathematical objects, whether love is a feeling or a relation, whether it's a category error to say that the laws of physics are true, and whether proper names are more like indexicals or demonstratives. Nancy Steege, the quietly efficient administrator, walked around reminding folks to sign a form, teach a class, advise a student, and somehow kept the operation limping along. Even traditional philosophy departments make me dizzy, so I suggested that we take advantage of the weather and walk over to the new Center for the Study of Language and Information, about half a mile away. We talked on the way over. In fact, CSLI was the first topic of conversation.

**Interviewer:** *Rumor has it that you have gotten millions of dollars to pursue situation semantics, enlisting the help of dozens of computer scientists, linguists and philosophers. Can that be so?*

**Barwise:** Hardly, though you are not the first person to think so. What happened was that a number of research groups in the Palo Alto area, all working on aspects of language and information, and all sharing a certain perspective about the importance of semantics, pooled their energies to establish the Center and submit a proposal to the System Development Foundation. We like to think that our ideas played a positive role in the favorable reception the proposal received from the Foundation and will contribute to the work of the Center, but the work on situation semantics

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is in fact one of the smaller research projects at CSLI.

**I:** *But you two are the P.I.s and Barwise is the Director as well.*

**Perry:** Well, there was a power struggle and we lost. But let's not get sidetracked on the Center, interesting as it is. Ask us about situation semantics, or the book.

## *Overview of* Situations and Attitudes

**I:** *I've known you two for several years and watched situation semantics develop in your earlier published and unpublished papers, but I must say I was quite surprised by* Situations and Attitudes.

**P:** Oh, dear. In how many ways?

**I:** *In at least three, now that you ask. In the first place, the dust jacket of* Situations and Attitudes *proclaims it "witty" and "highly readable," and the two of you as having "a gift for clear and interesting exposition." I know that the latter is true of at least one of you. I'm afraid, though, that in spite of its lighthearted tone and homespun examples, I found the book pretty rough going, almost tortured in placed. Some of your commentators (**PARTEE, VAN BENTHEM, DRETSKE**) seem to have had a similar experience. How do you explain this?*

**P:** The "witty" and "readable" were the doing of the PR people at the press. I would have preferred "turgid but profound." Yes, the book is difficult. We were grappling with very hard problems, trying to find a new way to approach them.

**B.** You used the word "tortured." I couldn't have chosen a better one myself. Imagine inheriting a beloved, baroque old castle, one that had been in your family for generations, but one that did not have the conveniences like drains and bathrooms that we find essential to modern life. Imagine further that you had no choice but to rebuild the castle, room by room, stone by stone, without any sort of plan, and that you had to live in it during the process. It would be a pretty tortured existence, for a while.

There have been times when that has been the way working on situation semantics has felt, to me, with traditional logic being the castle we have inherited that has to be rebuilt. Writing this book was like trying to write a book about the building you were going to end up with while you were in the middle of construction. It's not too surprising if some of what we were going through comes out in the book.

**P:** To complete the analogy, add a lot of good-hearted and helpful friends who want to see how the project is coming and keep making penetrating criticisms and suggestions, and then, when we take their criticisms to heart, wonder why things keep changing. For a while, we were keeping up,

but I fear that with publication of the book we have slipped to operating at about .6 perwise.

**I:** "*Perwise*?"

**B:** That is a term introduced in our honor by Emmon Bach. One perwise is the slowest rate at which a theory can change while staying ahead of criticism.

**I:** *In the second place, I expected a more formal book with at least some fragments of English worked out, something with the rigor and formality of Montague Grammar, but with a different set of primitives. Again, I don't think I was alone in these expectations* (**PARTEE**).

**B:** Geoff Nunberg, a linguist here at CSLI, recently described the book in my hearing as one of the most rigorous but least formal books on model-theoretic semantics. I took that as a terrific compliment, however he meant it. Rigor and formality are two quite different things. I aspire only to the former, though I know we did not achieve either in this book.

**P:** Partee was perfectly justified in her expectations – at one time, they were our expectations too. Let me try to explain what happened.

Originally, our thinking went something like this. Model theoretic semantics, as we find it, say, in Montague or David Lewis, was basically OK; but it was burdened with a couple of inherited features that made it much more complicated – both mathematically and philosophically – than it needed to be. First, possible worlds, we thought, could be replaced with situations, which seem more plausible philosophically and more tractable mathematically, once one set aside the semantic holism logic inherited from Frege and exploited the tools of the theory of partial functions. That was Jon's research program when we got together; by using models that were just traditional functions from language to sets – except they were partial – he had worked out a semantics and a logic, in the perfectly straightforward received sense of logic, for naked-infinitive perception reports.

**I:** *Like "Sam saw Mary run." Why are those so interesting*?

**B:** Well, we have what has been taken to be a sentence embedding construction, used to report mental activity, and to that extent like other attitude reports. But two factors make them especially interesting. It's very natural to take the embedded sentence, *Mary run*, to describe something limited and perceivable, a scene, rather than something abstract and huge, like a complete model, or a set of them, or a set of possible worlds. This seemed a natural place to use partial models. Second, the usual complications of "opacity" don't intrude. If Sam saw Mary run, and Mary is the tallest CIA agent in Kansas, then Sam saw the tallest CIA agent in Kansas run. There is no reluctance to substitute, as we like to put it. So

naked-infinitives seemed like a natural beginning point for rethinking the received wisdom on the attitudes and logic, in that it seemed more straightforward than the others and wasn't well handled with traditional tools.

**I:** *Did you think your approach could be extended to the other attitudes?*

**B:** Well, I certainly hoped it would lead to some new ideas about the semantics of the other attitudes. This is where John's work came in. A certain picture of the attitudes, inherited partly from Frege and partly from cognitive psychology, was making things more difficult than they needed to be. In this picture, an attitude, say belief, is a relation to a representation, which is also the meaning of a sentence with which one would express the belief. An attitude report embeds a sentence that has that meaning, and that's where the opacity comes from. Substitution of one term for another with the same reference won't necessarily preserve the meaning. So this wouldn't fit with the picture of attitudes as relations to something in the world, like a scene, at all.

John was arguing that this picture has to be abandoned when one takes context sensitivity seriously. In its place one gets a picture in which an attitude involves being in an efficient cognitive state, which itself has a meaning, and an interpretation, relative to the agent's context. One is *in* the state, rather than having an attitude towards it; the interpretation is what one has the attitude towards. So one makes a sharp distinction between ways of believing and what is believed. Then one has to look and see whether attitude reports focus on the one or the other. The answer seems to be that attitude reports do not, in the general case, embed a sentence that has the same meaning as the agent's cognitive state, but one that has the same interpretation – as we were to put it later – relative to the reporter's context. This point of view doesn't eliminate the difference between the cognitive attitudes and naked-infinitive perception, but it makes it look like they have a lot more in common than it seemed with the old picture.

**P:** I thought this picture squared with insights Kaplan and others had about the semantics of context sensitive sentences and with the insights Hector-Neri Castaneda had about the epistemological importance of what we now call "efficiency." But where Kaplan and Castaneda, each in their own ways, had developed their ideas "on top of" a basically Fregean picture, I thought the picture as I developed it undercut the motivations for the Fregean picture. Fregean senses and the intensions of possible worlds semantics both attempt to provide an entity to serve as the object of the attitudes, a single entity that could both be true and false, yet fine-grained enough to serve to individuate mental states. From my

perspective, this seemed like a mistake. The objects of the attitudes could be coarser-grained, made up somehow of objects and properties, like Wittgenstein's facts and states of affairs, while the entities used to individuate mental states shouldn't map onto them because of efficiency. They had to be "cross-grained" rather than fine-grained. Kaplan's characters had the right feel, but within his system, at the time, they were what Joseph Almog calls a "penthouse" on top of a possible-worlds version of Frege's picture. So I called what I wanted roles but had no model theoretic treatment of either the objects of the attitudes or the roles.

**B:** So, when we first got together, it seemed these ideas complemented each other in a very simple and exciting way. John's perspective on the cognitive attitudes, to the extent it underminded the traditional approach to the "opacity" of the cognitive attitudes, made it seem possible to extend a scene type semantical analysis beyond naked-infinitive perception statements to attitudes generally. The partial models, designed for the one type of attitude where the Fregean picture doesn't even *seem* plausible, seemed to be very close to what he needed for the objects of the attitudes. And we quickly found that by taking "meanings" to be relations between such partial entities, one for the context and one for what we begin calling the "interpretation," we had a non-penthouse approach to Kaplan's characters.

**P:** It seemed that we had a straightforward project. First, re-examine some of the crucial issues, particularly the attitudes that had been so crucial in the development of the philosophical perspective that underlies modern logic and semantics, from this new point of view. Then develop a Montague-like fragment. We announced our intention to carry it out, finding, somewhat to my surprise, that a lot of people were very interested.

**B:** When we got into things, our theory seemed to develop a life of its own, though. Prior questions about the nature of what we were doing, and the nature of meaning, kept surfacing in the most tantalizing way possible. That is, the answers weren't obvious, but they seemed clear enough that we were not able to ignore the problems as hopelessly complicated and keep our eyes on the project we had in mind. I think it would have been intellectually dishonest to do so. As a result, though, the book evolved into one on meaning. It's as if we intended to write a constitutional amendment but drifted into drafting a new constitution – or part of one, since we don't get around to discussing a number of topics, like modality, that the old system had firm rules about.

**P:** So, while the book isn't what many readers, including Partee, expected, it wasn't what we expected either. But if you don't have prior expectations, I think it is a pretty straightforward book.

*The Structure of Reality*

**I:** *The third thing that I found surprising, and I guess this was the biggest surprise because it snuck up on me, was the extent to which your version of realism has changed between this book and the earlier papers. I really don't think you were straightforward with your readers, even those without prior expectations, about this.*

**B** and **P:** In what way?

**I:** *Well, look. In your earlier papers and in the early chapters of the book, you sketch a certain program. There is a perspective, two ideas, and a strategy for dealing with the most serious challenge to the perspective. The perspective is that of hard-headed realism, the idea that human language is a natural phenomenon and that we should be able to understand it in realistic terms without appeal to things like mental representations or other possible worlds. One idea is that understanding what you dub the "efficiency" of language is crucial to understanding meaning. The other is to understand meaning side by side with information by viewing utterances in terms of their ability to communicate partial information about the world. The conjunction of these two ideas led to what you call the "relation theory of meaning" for sentences: meaning as a relation between situations or events in the real world, the event where an assertion is made, on the one hand, and the situation described by the assertion, on the other.*

*The rationale for writing a book about the attitudes, as you said in the Preface, was that the attitudes have always seemed an insurmountable problem for any realistic account. Your announced strategy for dealing with the attitudes was to classify perceptions, beliefs, sayings, and the like, in terms of their external significance, using real objects, properties, relations, situations, and so forth. You were infamous as opponents of any sort of mental representation. This is the avowed purpose of the book and the announced line. Am I right?*

**B** and **P:** Yes.

**I:** *But look what you end up with as part of your reality: types of events, constraints, roles, frames of mind, ideas, concepts, and images, for example. You have to admit this sounds pretty far from the hard-headed realism of the early papers. I would call that promiscuous realism. Anyway, it's what I mean when I accuse you of not being straightforward with us.*

**B:** I suppose there certainly were – and continue to be – questions about exactly what our realism amounts to. But we should try to be careful to keep track of what has shifted from the original perspective and what remains.

**P:** When we talk about realism, we are talking about where the structure of reality comes from. There seem to be five main views one can take

about the source of this structure. To give ourselves a way to refer to them, even at the risk of revealing myself a totally amateur historian, I'll attach some names to them.

(1) There is the view that the natural world, the one we perceive, move about in, causally interact with, and are generally part of, is the source of all the structure that there is. Let's call that the Aristotelian view. Our view was, and continues to be, a version of that.

(2) Then there is the Platonic view that there is another realm, one that we can grasp with the mind, and that objects in the natural world somehow owe their structure to some non-causal relation to objects in this eternal realm. I think that is also Frege's view, at least if one takes some of his remarks about sense literally, and also one that is implicit in the idea one finds in possible worlds semantics, that properties are intensions. Intensions aren't in our world, but only in the whole "possibluum."

(3) If one takes this view and simply removes the mysterious part of it, one has a form of nominalism. This is a way of looking at Quine and perhaps even Davidson. Take Frege and throw away the realm of sense. What one is left with is all one has available for semantics: objects and sets of them.

(4) Take this view and add a structure-giving mind, or a structure-giving Language, with a capital *l*. That's conceptualism; structure gets into the world through mind or language.

(5) Finally, take that view and throw out what little of the world it has left, and you have idealism and its logical extension, solipsism.

**B:** I think John listed these in order of decreasing (or at least nonincreasing) plausibility. It is a form of madness to really believe that the world is only a projection of one's mind, so we reject the fifth view out of hand. The world has structure above and beyond set membership; that rules out the third view. Mind and language would not have evolved in a structureless world, so that rules out the fourth. That leaves Platonic and Aristotelian realisms.

Platonic realism is fantastic, but one might have to accept it if something like the first approach can't be made to work. In fact, my own interest in the semantics of natural language stemmed from being a realist about mathematical objects, but one that was dissatisfied with Platonic realism there. Anyway, for these reasons, we wanted to make the first approach work, and still do.

**P:** This perspective, I am convinced, has stayed with us from the beginning. We saw early on that traditional model theory needs to be supplemented. One can't get by with just words and extensions. We took possible worlds semantics as providing a Plato-Frege type intermediary. No doubt that probably was unfair to some possible worlders; maybe we can talk about it later. Our level of interpretation was provided with situations, objects, and properties, conceived as real parts of the causal order – that is, the natural world. All the other things you mention, we see as uniformities across the natural world. And we attach great importance to the distinction between representations conceived of as intrinsically meaningful entities and meaningful cognitive states, evolved states that, because of their causal role, carry information.

**I:** *So you deny that you changed your minds?*

**P:** No, you are certainly correct in saying that our realism went from the relatively austere to the relatively rich. Here's how that happened.

At the outset, we realized that we would need more than real situations. Otherwise, how could we provide an interpretation for false statements? Or an object for false beliefs? In a way, we tried to finesse the problem by relying on set theory. We introduced abstract situations, not as alternative realities but as sets that could be used to model situations. Then we allowed ourselves to introduce into our semantical theory any constructions from abstract situations and the other devices of set theory that we needed to make the semantics work. This seemed to allow us a lot of freedom at the level of our theory, while investing the world only with situations, objects, locations, relations, and the like.

Somewhere along the way, though, we realized that this was an illusion. After all, our theory was intended to be a theory about the world. To the extent that it is correct, the sets we constructed did get at real uniformities in the world, so we are committed to all sorts of things. This dawned on us as we worked out the book, but it should have been emphasized more at the beginning and worked into the theory in a more wholehearted way. After all, if we are going to get by without some Platonic realm like senses or alternative possible worlds and find everything one needs in the reality we inhabit, we had better be prepared to recognize all the structure that's really there.

**I:** **JACKENDOFF**, *in his commentary, is very critical of your realism, since he thinks that representationalism is the way to go. He gives evidence from language that people recognize places, directions, actions, events, manners, and amounts and then asks, "But do we therefore have to ascribe such entities to reality? I should hope not." Would you say that in your view of realism there are such things?*

**B:** One of the things a commitment to realism means is finding out just how much there is in the causal order besides the brute physical. Not only is there the physical world, there are invariants across it that organisms, like people, recognize, invariants across those invariants, and so on. As van Benthem points out, this is not all that dissimilar from ideas of Helmholtz that fed into ecological realism. *Of course*, there are such things as those Jackendoff mentioned. How could he seriously doubt it? These things are much less abstract than the national debt, the collapse of detente, the IRS rules, let alone some of the things we discuss in the book. The fact that all of these things depend on the interaction of cognitive agents with their environment for their existence should not be held against them, any more than the fact that we had to write our book should be held against it.

**I:** *How's that?*

**B:** Well, the book is the product of the interaction of reasonably intelligent and definitely finite agents with an external world. The book isn't a basic particle, or even a single physical object, but a very abstract uniformity, one that plays a role only in the life of a very select subgroup of a certain species. But it still exists. We get royalties. Or at least we would have, if we had not made so many corrections in proof.

**I:** *It seems to me that you are now breaking with tradition here in a way that is not even hinted at in the book that I want to get back to, but first let's return for a minute to the five approaches to structure you sketched. There is clearly something true that motivated the fourth approach. A lot of the structure that is most important to us is, in some sense, the products of minds, language, and culture quite generally. It would be just as mad to deny that, surely.*

**B:** But should one think of it as a matter of projection, from mind, language, and culture onto the world? Or as a matter of selection, interaction, and creation of new structure, involving a structured world and some of its most structured parts, people? This is basically the problem of secondary qualities, the mismatch between the properties found most crucial in physical theory and the properties that play the most central role in cognition and action. No one thinks that actions or manners are real in the sense of being fundamental physical properties, but then neither are depressions or elections. They're real, nonetheless, and the objects of study of various scientific disciplines.

**P:** You used the term "promiscuous realism." I once suggested that term to John Dupre for his view (at least one he held at one time) that the properties and relations used by various sciences like botany and biology are perfectly real, whether or not they can be identified with or reduced in any simple-minded way to the properties and relations used by physics.

That's like the view we came to in the book.

However, promiscuous realism, so conceived, still isn't unprincipled realism. Before admitting something as real, one has to say what it is an invariant across. And one doesn't rely on relations to alternative realities to give structure to the world or on intrinsically meaningful entities.

**I:** *You presumably think there are no unicorns, but there is a concept of a unicorn. I think the tough question is how to draw the line between reality and nonsense.*

**B:** I think we had an important idea about that. I don't think any of our commentators fully appreciated it, though, or they would have pointed out to us all of the people who had it before us. There must be some.

There are two relations that an abstract situation can have to a real one: it can classify it, in which case the abstract situation is factual, or it can interpret it relative to some constraint. (Actually, we would now see the former as a special case of the latter, where the constraint is the correspondence between our model of the world and the world itself.) It is the latter possibility that makes it possible for nonfactual situations to play a role in building up factual ones: it may be a fact that you believe that Caesar crossed the Rubicon, even if in fact he never did. And your believing this can have a role in the causal order. This is the basic idea that will have to be used to give an account of fictional objects, such as Santa Claus, and unrealized properties, like being a unicorn. While no unicorns are part of the causal order, a certain type of situation does interpret many texts, fantasies, even beliefs. If we take the concept of a unicorn to be a uniformity across these, it is a part of the causal order and so, real.

**P:** We don't work all of this out in the book, to be sure, but the point is that promiscuous realism, in this sense, isn't the same as Meinongianism. All the things mentioned by Jackendoff are certainty part of the causal order, but unicorns just aren't.

**I:** *I think we are getting ahead of ourselves. Maybe we could take the four points I mentioned earlier, in order, and discuss the commentators' worries as they come up.*

**B:** Which four points?

**I:** *Realism, efficiency, partial information, and your account of the attitudes.*

**B** and **P:** Sounds reasonable, though I suspect it will be hard to keep the topics separate.

## REALISM

**I:** *Let's try. First the realism. As your distinction between Aristotelian and Platonic realism shows, there are several things to be discussed. Let's start with Platonic realism as embodied in possible worlds semantics.*

*Realism and Possible Worlds*

**I:** *Your brand of realism has real properties, relations, and situations, at least. I suppose it's fair to say that as far as semantics for natural language goes, your main competitor has to be Montague Grammar. Montague's framework has individuals and possible worlds. From these he could construct properties, relations, and situations, or passable substitutes.*

*Now it seems that there are two (compatible) methods you could employ to make people give up Montague's approach for yours. One would be to show that there is something so terrible with his approach that it must be abandoned. The other is to show that yours handles everything his does, and better, or that it can handle more.*

*You don't do either of these in the book. You explain your own view and argue that the attitudes, which might have been thought to provide an insurmountable problem for your approach, do not. And you provide some reason for us to believe that, with situations, we have a tool for getting around problems like logical equivalence. But you neither mount a sustained attack on Montague's theory nor provide a worked out fragment as extensive as his. So why should we prefer your theory to his? Sounds like buying a semi-pig in a poke.*

**P:** It's true that in the book we concentrate on developing our own theory. Perhaps we could claim that this was all we meant to do – introduce an interesting new competitor or "paradigm" (**VAN BENTHEM**) into the field, that people would find intriguing to develop and that might or might not turn out to be better than what was already available. However, that isn't what we were doing, so we won't claim it was. It would be more accurate to say that we felt that the possible worlds point of view is dead wrong, deeply unsatisfactory, both philosophically and mathematically. We assume that right-minded people will be relieved to see an alternative based on more plausible notions outlined, even if it isn't as fully developed.

**I:** *But surely possible worlds are not all that implausible. I mean, the world might have been other than the way it is. You yourselves admit the distinction between necessary and contingent constraints in your discussion of Humean structures. Surely the notion of possible worlds is nothing more than a working out of the distinction between necessary and contingent fact.*

**B:** We need to distinguish between intuitive ideas about possible states of affairs and the actual assumptions about possible worlds that are built into the mathematical theory called possible worlds semantics, as embodied in Montague Grammar. It is with the latter that we have our quarrel, not the former.

**I:** *I must confess that, not being a logician, I have never bothered much with the formal development of the theory. Could you be a little more explicit*

*about the formal side? Isn't a possible world basically just a model of the kind used in first-order logic, with some bells and whistles?*

**B:** No, possible worlds are not just models. That is one of the biggest misconceptions around. Carnap's original idea was to replace truth in one model by truth in a set of models, to capture the intensionality missing in first-order model theory. That just didn't work, as various people discovered. It's a little hard to explain, but the problem is that distinct models for some language L, when restricted to a sublanguage of L, may restrict down to the same model. Or, to turn it around, a single model, when considered in the context of a richer vocabulary (say, one where you are talking about someone's beliefs that could be formulated in the original model), may need to split into different models. It is not entirely obvious, but because of this it was realized that sets of models, in and of themselves, are not anything like fine-grained enough to make the kind of distinctions that the theory – for example, in the case of iterated attitudes. I am not sure who first realized this, but Montague certainly recognized it. In his paper on philosophical entities, he discusses this and says, for example, that "the identification of possible worlds with models was the main obstacle to a successful treatment of iterated belief contexts within the intensional logic of [Kaplan's thesis]."

The assumption you need, and the one built into Montague Grammar, is that each world provides total information about the extension of every piece of language in that world, for all of time. When you recall that the interpretation of phrases in MG are total higher-type functions, of arbitrary finite type, defined on the set of all possible worlds, this becomes an incredibly strong assumption, and the thought that talk of Jackie biting Molly is really talk about this collection of worlds is simply beyond belief.

**P:** We have to remember here the same principle we discussed earlier. A semantical theory is commited to the reality of the entities it uses in its explanations. Situation semantics is committed to the reality of the uniformities we use to explain language use. The Montague Grammarian, or other possible worlds theorist, is committed to possible worlds and needs to tell us what they are if we are to take their theory seriously (**PARTEE**). Saying that they are "just indices" is not a responsible response.

I think there are really two basic accounts one can give. David Lewis suggests both in his book *Counterfactuals*, but he sometimes talks as though they are the same. One account takes possible worlds to be alternative realities. Actual is a relative notion; each reality or world is actual relative to itself, merely possible relative to others. What favors our world is just that we are in it. A lot of people sneer at Lewis's point of view, but I think he is one of the few people to understand the commitments of

the various formal accounts. He thinks possible worlds are needed by the best theory we have, so he believes in them. Still, I think most people feel about David Lewis the way they feel about Bishop Berkeley. Perhaps his view admits of no refutation, but it does not produce conviction.

Lewis also says that we can think of possible worlds as other ways the world might be. This seems a very different conception; not other realities, but other "total properties" reality might have. I think this is more or less the conception Stalnaker has and develops. (E.g., in his paper "Antiessentialism," Stalnaker, developing an idea of van Frasen, defines possible worlds to be total functions from a set $D$, which he takes to be the set of all possible individuals, to a set $H$, which he takes to be the set of all points in "logical space." If we think of points in logical space in terms of properties and relations, it might be possible to see early situation semantics as replacing Stalnaker's total functions by partial functions.) It seems to me a much more plausible view, though I don't see that it is compatible with the formal theory in Montague Grammar. But in any case, if one is going to have objects and properties – the world and all the possible ways it could be – why not start off on a modest and common-sensical scale, with the properties that we actually recognize? That's what we are trying to do. While Stalnaker's approach to possible worlds seems the most plausible, once one has gotten that far, the move to studying limited situations and their properties, rather than the total world and its properties, seems natural and inevitable.

**I:** *But a couple of your commentators (**PARTEE**, **VAN BENTHEM**) comment about interpretations of possible worlds as partial worlds.*

**B:** It is true that some writers have augmented the theory of possible worlds to add partial possible worlds. However, no one, as far as I know, and even as far as van Benthem knows, has worked out the higher-order Montague-like analogue of this theory. I thought about it once. The idea would be to have a part-of relation between partial worlds and look at those higher-type functions that were hereditarily consistent with respect to this part-of relation. However, I found that it became terribly complex once you went beyond the first couple of levels in the type hierarchy, much more complicated than the analogous problem in the theory of partial functions of higher type recursion theory. But then maybe my heart wasn't really in it. Certainly my intuitions weren't. For those that really believe in other possible worlds, it is surely a worthwhile task, though.

**P:** It is sometimes just easier, and certainly more fun, to start over than to try to figure out how to modify great ideas from the past to get the result one wants. And even more often, one really doesn't see what others were getting at until one has worked out one's own approach. Situation

semantics could probably have been developed by successive modifications of a number of different theories. Perhaps it would have happened that way, had we been better scholars – but I don't really believe it. I do know that a lot of ideas that seemed off the wall when I first encountered them years ago now seem pretty sensible. One example that our commentators don't mention is relevance logic; there are a lot of ideas in that literature that bear on the themes we mention. Another example – someone we did mention in our "scanty" acknowledgements – is Jaakko Hintikka. Hintikka thinks of his possible worlds in terms of partial situations in some of his writings. But in his later writings, say on perception, he identifies limited situations in the real world with the set of all possible worlds compatible with the limited situation.

**I:** *That sounds like a reasonable thing for a person who believes in other possible worlds to do.*

**B:** Even then it has its dangers. In the first place, if a situation corresponds to a set of possible worlds, then a set of situations (the situation semantics analogue of a proposition, at least in the old days) corresponds to a set of sets of possible worlds. That means the notion of proposition used in possible worlds semantics (a "set" of possible worlds) is at odds with thinking of situations in terms of all the worlds that contain them. Worse, though, is the ease with which one falls into the trap of identifying the properties of the partial situation with the properties common to all of its total extensions. Even we have fallen into that trap at least once.

**I:** *What's wrong with that?*

**P:** Look, the situation right here is quite limited, right? But being limited is not a property shared by any world containing it. That's just where the problem of "logical equivalence" comes from. Consider this situation here and now, with the three of us talking. Reagan is not here, right? He isn't part of the situation. But in every total world, at least under the assumptions built into the formal theory, Reagan is either sleeping or he isn't. That is a property each world has, but this situation doesn't have that property.

**I:** *So the point is that the theory, as it is developed in Montague Grammar, at least, commits you to each possible world being a primitive object that carries complete information about all of language. You don't think there really are such things; and, even if there were, it is a bad idea to confuse a limited situation with the set of all total situations that contain it. Is that it?*

**B:** That's part of it, but there's another point. Look, why did Carnap have to move to sets of models, in the first place? It was to find a way to recapture properties and relations so that, e.g., nouns with different meanings that happen to have the same extension can have different properties (for Carnap, functions from models to sets) associated with

them. When you move to possible world semantics, you reconstruct properties as functions from primitive possible worlds to sets. But if you need properties to understand language and the world, and if, as it seems from a scientific point of view, there really are properties and relations between things in this world, independent of language, why not just admit that and stop trying to define them in terms of some other less plausible primitives? The whole movement that resulted in possible world semantics seems like nothing so much as an historical accident, stemming from Frege's neglect of properties since he thought he needed senses, and then through Carnap's decision.

**I:** *I want to understand that last remark. But, first, I thought I heard shudder quotes when you mentioned that, in possible worlds semantics, propositions are modelled by sets, or "sets" of possible worlds. Was that my imagination?*

**B:** No, it wasn't. Not only does the formal theory take other realities as primitive, it assumes that there is a *set* of all such. It must, in order to have arbitrary subcollections (propositions) count as legitimate objects. But the assumption is at odds with the view of sets built into the set-theoretic metatheory of possible worlds semantics.

The standard wisdom in set theory, the one used to justify set theory in the first place, is that sets are *limited* collections, collections that can be comprehended as a completed totality and so be elements of other sets. How in the world are you going to take all these separate but distinct total realities and comprehend them as a completed totality? Is that totality outside of reality, or what?

Suppose you believe that there are possible worlds and that contingent facts are facts that hold in this world but fail in some other world. I think it was Kaplan who noticed that since there are more propositions (sets of possible worlds) than worlds, by Cantor's theorem, there are not enough worlds to differentiate contingent attitudes toward propositions. The same point can be made without the attitudes. If there is only a *set* of possible worlds, then there is only a set of contingent facts and hence only a set of objects of which contingent facts hold. But, in the set theory used by the theory, there is no set of all objects, so there are many objects of which no contingent fact holds. Take some such object, say the least such that is an ordinal number. Call it $c$. How can it be that there are no contingent facts about $c$? What about the fact that we are talking about it? Is that necessary? How about the fact that I called it "$c$" rather than "$d$"? Is that necessary too?

**I:** *Is that really a problem? It sounds more like a problem in set theory than a problem about possible worlds.*

**B:** Maybe so, but it is a problem in the set theoretic basis of Montague Grammar. It is the kind of problem that we took seriously in our own theory and which gave us fits, but which is simply sluffed over in possible worlds semantics. I can't help feeling that the whole thing is philosophically incoherent, when push comes to shove.

**I:** *Can't linguists think of it all in terms of using a mathematical model, and leave the connection of the model to reality up to mathematicians and philosophers*?

**P:** Well, that's what we are, and we can't see what the connection is supposed to amount to. The relation between a model and what it is a model *of* is something we have been forced to think about this year by some of our colleagues here at CSLI. Put it this way: if the model-theoretic structures of possible worlds semantics, the ones that include a set of all possible worlds, are supposed to be a model of something, say super-reality, under some correspondence or other, then there ought to be one that is an intended or standard model, the one that really corresponds to super-reality. But when you put it that way, the whole enterprise seems fatally flawed, for all the reasons we have indicated.

**I:** *We should come back and talk about the relation between model theory and reality because I suspect you are making a new move here, one that is not in the book. But, first, what was that point about Frege's senses making us think we don't need properties*?

**P:** Well, here's the picture I have of it, for what it's worth. In Frege's account, senses are primarily *what we grasp*. While they aren't psychological, as he emphasizes, they are postulated as necessary for dealing with the structure of thought and meaning, not the structure of the natural world. So they are not properties, not constituents of facts. Frege actually has properties, or something like them, in his system; but, because senses provide a more fine-grained level in his theory, he really doesn't put them to much use and seems to think of them as extensional. Because the structure of thought is finer-grained, the structure of the natural world can be coarser-grained. This is the back and forth one sees throughout the history of philosophy.

The phenomenon we call efficiency undercuts this whole picture. The entities we need to get at the structure of *thought*, if thought is to be nomologically related to perception and action, will not map straight onto properties and objects. They will be cross-grained, not fine-grained.

There are really two steps in our thinking here. Step one is that, if meanings are efficient, one needs a level of interpretation, of properties, objects, and situations, and can't get by with just meanings, objects, and sets of objects. That is, a Platonist who understands efficiency has to be an

Aristotelian too, for his realm of meanings cannot map onto the structures thinkers use the meanings to think about, but relations between the thinkers and those structures. Second, one can see those meanings as relations; one can be an Aristotelian, without being a Platonist. Meanings are higher order uniformities, across thinker situations and situations thought about. This last point is our interpretation of the Gibsonian dictum, that the unit of study has to be the organism in its environment.

### *Aristotelian Realism and Ecological Realism*

**I:** *It seems clear that you think of some such form of ecological realism as the guiding spirit behind situation semantics. Do you consider yourselves Gibsonians?*

**B:** Sticks and stones. We have found the naturalist perspective of Gibson a liberating antidote to the idealism and solipsism that is so rampant in the study of language and the mind. It certainly seems to be a red flag in some quarters, though.

**P:** Gibson was, of course, a psychologist, and for him the relevant information has to be grounded in physical invariants, things he could measure. But he emphasized that given the complexity of organisms, and the fact that their perceptual systems evolve in quite specific ecological niches to meet certain needs, there is no reason to expect the physical invariants to be easy to find. His disciples seem to be making progress in this research project, with respect to perception and locomotion. No one has come near that with language, yet. Indeed, language has always been one of the phenomena that seemed, to representationalists at least, as almost an *a priori* case where Gibson's program was doomed to fail. One of our hopes, in writing the book, was to cast doubt on the intractability of a naturalistic perspective toward language. No one would have found the physical invariants/perceptual invariants discovered by Gibson and his followers, either, without the perspective that suggested looking for them.

**B:** We have our points of divergence with the hard-line Gibsonian, too, as the comments by **TURVEY AND CARELLO** show. We try to drive a wedge between meaning and information by saying that information depends on the relevant constraints being actual, and the conditions right, while meaning also requires attunement to the constraints. That is, meaning depends on the presence of minds, or information processors more generally. This is a narrow wedge, and it seems to Turvey and Carello like the beginning of the slippery slide toward representationalism. However, their worry seems to be as much about the sociology of the matter as about the fact, since what concerns them is the "danger that attunement *might be*

*read* in a psychologically contributory sense, viz., the organism is able to interpret the information, that is, ascribe meaning to the information. [Barwise's stress] From a realist viewpoint, meanings are discovered by animals, not invented or created by them."

Surely we cannot let ourselves be pushed into denying that the cognitive abilities of an organism contribute in essential ways to the animal/environment duality, just for fear that this will be read as the animal interpreting some representation of the environment.

**P:** I think this point is related to **DRETSKE**'s worry about our notion of conventional constraint, but from the other direction. One of the problems with the standard line in ecological realism, from the perspective of a philosopher of mind and language, is that it often seems to leave no room for error. You often hear Gibsonians say that animals just cannot make mistakes because they do just what is appropriate given the information available to them. Our emphasis on conditional constraints, constraints that hold only under certain conditions, is our way of trying to get at the intuition behind this idea, while at the same time saying what is wrong with it.

In one sense, our work since the book has pushed us away from Gibson's emphasis on perception. In the book, we distinguish between belief as failed knowledge and belief as a positive strategy. Unfortunately, we didn't say much about the latter. Think of a blind squirrel in a nut-rich environment, that goes around lunging and gobbling. We might think of it as conjecturing that there are nuts in front; even if it is wrong most of the time, its a good strategy in the right environment. The unsuccessful conjectures are not failed knowledge. There may not be any simple organisms that work this way, but such a squirrel might be a good model for some distinctively human cognitive attitudes, like conjecturing and believing.

**I:** *Well, Gibsonians or not, there is an almost missionary zeal that comes through in your book, especially in Chapter 11. Why does it seem so important to you to give a realistic account of the way language carries information?*

**P:** I think we imagined ourselves as arguing against a powerful consensus of theorists, an unholy alliance of possible worlds semantics, Fregeanism, and representationalism. I don't think we were totally wrong, but the theorists who weren't part of the consensus are naturally miffed, and those that are part of what seems to us like a consensus don't see it as one, in any case.

**B:** I have a suspicion, which I am not enough of a scholar in the philosophy of science to be able to prove, but which undoubtedly infected

my own writing in the book. It is that the basic ideas, about logic and its relation to the structure of reality, that we are attacking have been swallowed, hook, line and sinker, by influential thinkers in the philosophy of science, and that they have spread from there to some people actually doing science. These ideas make them skeptical about the notion that their theories are either right or wrong. Instead, they think of them as just being different ways of looking at things. I think that is very dangerous, especially when social scientists, whose domain is so complex and important and whose ideas can turn into public policy, start working in that frame of mind.

### *Real-world Semantics versus Set-theoretic Model Theory*

**I:** *Let's get back to realism. I said a while back that I thought you were now breaking with tradition in a way that is not hinted at in the book. It seems to me that what you are doing is, in fact, breaking with the basic method of set-theoretic model theory in semantics.*

*In your book, you start by assuming there is a fixed collection of primitives, individuals, properties and relations, and space-time locations, and you see what you can do with those in terms of classifying real events, using abstract mathematical objects constructed from your primitives. This is the standard model-theoretic method. You even give us replacements for standard models with your structures of situations. It seems to me, though, that by the end of the book you have dug yourselves into a hole, and that is really what you acknowledged earlier.*

**B** and **P**: Maybe. Say more.

**I:** *Well, as we have already said, by the end of the book you recognize that you need a host of more abstract real things, like types of events, types of objects, roles that things can play. That seems to be where a lot of the meat in your theory is, but you don't have those things as part of the world. Rather, I would say, what you have given us is a mathematical model of them. It seems that you are now suggesting having a theory that is about the world itself, with all it contains, rather than having a theory that is mainly about set theoretic models of the world.*

**P:** Yes, you're right. In the long run, the theory should be about the relation between language and the world, not about the relation between language and set-theoretic models.

**B:** For the moment, let's call *all* the set-theoretic objects we construct "models." For example, what we referred to as "event-types" I will now call "models of types of event," or, more briefly, "event-type models." What we called roles, I will call models of roles, or role-models. Now, it

seems to me that what we did in the book was take various intuitions about real things like types of events and the roles things play in them and try to capture those intuitions by constructing models, like event-type models and role models.

**I:** *Why didn't you, instead, express those intuitions about types of things and roles directly? Why not develop a theory of situations, types, roles, and the like?*

**B:** You mean something like the difference between stating the basic properties of the real numbers with some axioms, versus some particular set-theoretic construction of a model of the axioms, say as Dedekind cuts or Cauchy sequences?

**I:** *If you say so. Sounds like the same sort of thing, though my calculus days are far behind me. The idea, I guess, is to break away from the hegemony of set theory in logic, to get back to talking about real things, not just models of them?*

**B** and **P:** Exactly. That is another way of saying what John said earlier about the illusion we were under when we thought we could get by with set theoretic objects to classify invariants, rather than admit the invariants as first-class citizens of reality. Part II of our book was called "A Theory of Situations," but really all it is is a model of a theory of situations. It is a real theory of situations that we are working on, now. We have found it to be a very liberating idea.

**I:** *Isn't there a danger that you will end up having to axiomatize everything there is in reality, since we can talk about everything in language?*

**B:** No, I don't think so. The main subject of the theory is really information and action. We need to get at the structure that is most relevant to understanding the flow of information and its relation to action. We feel we have enough experience now to see that there are certain key notions, like type of situation, constraint, condition, anchor, role, and the like. We are beginning to get at the significant generalizations about their structure and relationships.

**I:** ***VAN BENTHEM*** *wants to know how logic is going to fit into your approach. After all, you are a logician, or used to be.*

**B:** Yes, I've wondered too, over the past few years. I know that a lot of my friends think I have turned from logic to linguistics, which was never my aim, or the way it felt to me. It now seems to me that what we logicians have done in set-theoretic model theory is to restrict ourselves to the logic that can be defined in terms of the structure captured by set membership. Once we admit more structure into semantics, there will be a richer domain for logic. For example, by studying the structure of the things I just mentioned, we are finally getting to the position where we can do what van Benthem wants us to do, prove something.

**I:** *I see several parts of your theory, or model, that will not appear to succumb easily to this approach. For example, what in the world (no joke intended) are indeterminates models of?*

**B** and **P:** That bothered us for a long time. But, really, it seems almost obvious, now. They are the "argument places" of properties and relations. Or, to use the earlier terminology, "argument-place models."

**I:** *Come again? It is hard to both think and listen to people speaking in unison.*

**B:** Look, why do logicians always think of relations as coming with their argument places given by some order, so that in logic we write relation symbols with their arguments listed in a fixed order? And why do we, in our own book, use ordered sequences $\langle a, b, c, \ldots \rangle$ of things, say in defining event-types? Certainly if you watch a dog fight, there is no ordered sequence of things involved. Maybe it is just because that is the way the languages spoken by the fathers of Western logic do it. English, for example, codes the arguments of a relation syntactically: *Jackie bit Molly* means something different from *Jackie was bitten by Molly*. Other languages don't do it that way. Some languages have free word order and mark the things that fill different argument "places" with different case markers.

**I:** *That's true, but so what?*

**P:** So – what we needed indeterminates for was to model argument places, their interaction in types and constraints, and the way they get filled.

**I:** *Hmm . . . Wouldn't that let you think of things like the activity of biting as having an additional argument, namely one for the space-time location? That might simplify things for you.*

**B:** Yes, Carl Pollard made a similar suggestion once. It looks more attractive to me phrased this way.

**I:** *But aren't you in danger of ending up with an extra argument place for all properties and relations? Is there a missing argument place in the less-than relation for the location where 3 is less than 5? That sounds very odd.*

**P:** Ivar Tönnison once suggested that we made a mistake to have all our facts located in space-time. We have come to agree, so there would be no need for an extra location argument for relations that take place outside of space and time.

**I:** *Do you see a role for something similar to a property or relation, except that it would have no argument places at all?*

**B:** Yes. Notice that it would be independent of location in space and time, but it would have a truth value.

**P:** Propositions, whatever they are, have just those properties.

**I:** *I thought you guys didn't believe in propositions.*

**B:** What ever makes you think that? In our papers, we tried to model

propositions with collections of abstract situations, but this got us into set-theoretic hot water. But, once you get away from trying to build everything up from a fixed stock of primitives in set theory, and see the task as that of understanding the relations between all that is, it frees you from having to construct propositions.

**I:** *You mean take propositions as primitive*?

**P:** It depends what you mean by "primitive." If you mean take them as things in their own right, rather than construct them out of other things in set theory, yes. But if you mean that they would not have a structure, then no. They clearly have structure, but not set-theoretic structure. There is nothing in our theory that is primitive in the sense of not having structure.

**I:** *It sounds to me like you are going the route Soames urges on you in his comments.*

**B** and **P:** Right. Soames' comments have played a very important role in our thinking recently. They undoubtedly softened us up for the move from model theory to real world semantics, as you put it.

**I:** *Well lets not discuss Soames' comments until we get to the attitudes. Does this new attitude mean that you are giving up your restricted set theory* KPU *as a metatheory? A lot of people think that was responsible for your foundational problems.*

**B:** The use of KPU does seem to have confused people. The foundational difficulties we ran into in the book were not caused by the restricted nature of that theory. They would have been just as bad in Zermelo-Fraenkel set theory, ZFC, since they were problems that come from the existence of collections that are not sets, and from the foundation axiom. But, yes, we are giving up set theory as a meta-theory, in favor of a much richer, more inclusive theory. Set theory only recognizes one kind of structure, that given by set membership. That is just too impoverished to mirror the structure of reality. Besides, we now want to talk about the structure of reality directly.

**I:** *Why did you use* KPU *instead of a stronger, more familiar theory like* ZFC?

**B:** For a number of reasons, some reasonable, some mistaken. As I said earlier, I am also a realist about mathematical objects like sets. I happen to think that there are real invariants across situations that we call sets. Consequently, I want the set theory we are using to be true, not just consistent. I happen to have qualms about whether the unrestricted replacement axiom of ZFC is really true. I didn't mention this in the book because most of my logician friends think it is a bizarre view, and it was not really very relevant to what we were up to. Another reason we used KPU, that we mentioned, was the interest in seeing just where one is forced out of such a restricted theory into a theory that posits infinite objects.

The mistaken reason, we now feel, was that we take minds to be finite and so wanted our set-theoretic objects used in doing semantics to be finite. But that is really misguided, as our own theory of indirect classification shows. There is no reason to suppose that the best theory of any particular finite organism, device, or process will be one that classifies it using only finite objects. It is only if you confuse the theory of the device with the means used by the organism, or whatever, that you will make that mistake.

**I:** *What about the axiom of foundation, which is part of* KPU *as well as* ZFC, *and which caused some of your problems? Will that be part of your richer theory of situations?*

**B:** I'm not sure, to be frank. There is no doubt that the traditional justification for it is based on a vew of sets as comprehensible totalities built up from atoms, or from nothing at all. If you give up this atomistic view, it is not clear why you should accept foundation as true, and I can see possible arguments for its being simply false. For example, it seems related to problems in understanding the nature of public information. But I am still of two minds.

**I:** *Let's get back to real world semantics, as you describe it. Another problem I foresee with the project is the one that vexed Russell for years. What is one to do about false statements? Your abstract situations, or situation-models, as you would now call them, are fine for classifying false statements. But if you make this move toward promiscuous real world semantics, what is going to take their place?*

**P:** Types of situations.

**B:** One of our problems was that we wanted to classify statements and beliefs with realistic propositions, construed as collections of situations. However, that didn't work, for set-theoretic reasons, since they were not eligible for membership in other abstract situations. There is no such problem with types of situations.

**I:** *I am getting lost. Are you distinguishing between types of situations and situation-types, a term used in the book?*

**P:** Oh, sorry. Yes, a type of situation is a real thing, the kind of thing our event-types are set theoretic models of. Situation-types, as they are used in the book, are set-theoretic models of static situations. You'll notice that we now use the term "situation" more often than "event" or "course of events."

**B:** We also have a notation for types of things that might be useful later on. Just as one uses "$\{x \mid \ldots x \ldots\}$" to denote the set of things that satisfy the condition $\ldots x \ldots$, we will use the notations "$[x \mid \ldots x \ldots]$" to denote the type of thing that satisfies the condition. This works out neatly with a new notation for roles: write "$x \mid \ldots x \ldots$" to denote the role of being an $x$

such that . . . $x$ . . . . Then for any role $x$ we can form the type $[x]$ of thing that can play that role.

**I:** *I don't quite follow all that, but never mind, it's only an interview. Now it looks like you have two candidates for propositions: the 0-ary unlocated properties mentioned above and types of situations. Isn't this an embarrassment of riches?*

**P:** No, propositions are true or false, but a type of situation is not the sort of thing that is true or false. What will be true or false is whether or not some real situation is of that type.

**I:** *You have developed a whole theory based on the idea that declarative statements describe types of situations and events. If you now admit propositions, aren't you going to have to give an account of the relation between the type of situation a statement describes and the proposition the statement expresses.*

**B:** Yes, that's true, but that is just a general case of the problem of understanding the relevant relationships among all that is, that is bound to be a part of real world semantics. Thinking about Soames' problem makes me suspect that one very important way to arrive at propositions is, in fact, from types of situations. I am sure that, given any type $S$ situation and any real situation $s$, one can form a proposition $P$ that is true just in case $s$ is of type $S$ and another $P'$, that is true just in case $S$ is realized by some real situation.

**I:** *Would propositions have constituents, like Russell's propositions?*

**P:** Take the proposition $P$ that $s$ is of type $S$. The structure of $P$ would be systematically related to the structures of $s$ and $S$, so if, say, $s$ has constituents, I see no reason not to call them constituents of $P$ as well.

**I:** ***WINOGRAD*** *is quite sceptical about the workability of realism of any form in doing semantics. Take his example of the word* on. *It can be used in lots of different ways. What makes you think that they all fall under any single relation of being on?*

**B:** Well, anyone who calls us the Newton of semantics is clearly insightful, but we never did think such a thing.

**P:** Or at least not for months. There are lots of different relations that humans recognize that hold between two different things, relations that we happen to use *on* to refer to in English. Other languages give us different words for some of these.

**B:** It's really not all that different from the case of names. Just as the name *Jackie* can, under appropriate circumstances, be used to refer to anyone or anything named Jackie, so too *on* can be used, under appropriate circumstances, to designate any of the appropriate relations.

**I:** *Winograd anticipated this response. He claims that you are going to end up with one on relation for each situation in which the word is uttered.*

**B:** I don't see any more reason to suppose that to be true than that there are separate Jackies for every use of the word *Jackie*. Of course, there are philosophers who think that's the case too, different Jackie-stages, or something, that get referred to.

**I:** *It sounds like you are just saying that* on *is ambiguous in the way that, say, bank is ambiguous.*

**P:** Not necessarily. In the book, we pointed out that while there is not too much in common among the various things named *Jackie*, there is something in common; and, historically, names often carried more information about their bearers than they do now. It seems entirely reasonable that the same is true of other kinds of words. If you look at only the various two-place relations that can serve as the interpretation of *on*, it may be hard to see what they have in common. On the other hand, if you recall that the meaning of an expression also involves a component for its context of use, then you realize that the meaning (as opposed to interpretation) of *on* is really a three-place relation, one that gives rise to the various two place relations by fixing other aspects of the discourse context. It is at that level that the interesting generalizations about them should emerge, if they are there, as you seem to expect. With *bank*, however, I can't imagine what such a thing would be.

**B:** Part of Winograd's qualms about realism stem from the fact that we have to use language to talk about language, and this seems a vicious circle. "There is," he says, "no reason to suppose that the use of language in general rests on anything else [like reality]." As a computer scientist, I would think he would recognize the difference between vicious circles and boot-strapping, well-founded recursion. Something like the latter has to be provided as a grounding for language use.

He gives himself away when he says that a difference between computer languages and the languages of mathematics and logic is that the former are "designed to be *about* something" [Winograd's stress]. If one doesn't think that the language of mathematics is about anything, but sees it instead as language just resting on more language, then you are not going to see much hope for a mathematical theory of meaning. But then I don't see how you could hope for a mathematical theory about gravitation, or *about* anything else, either. It is certainly an odd view of mathematics. If ever there was a case of boot-strapping, of using language about things to introduce new concepts, properties, and the like, that one could proceed to talk about coherently, it is mathematics. Just because you use language to introduce some property or relation does not mean that the property or relation itself is a piece of language, or that talk about it is talk about language.

**P:** Let's not let Jon get started on mathematics and language again.

I think we are in danger of missing an important positive point that Winograd makes. He makes a certain claim about how he thinks language should be studied. We have been discussing the reasons he gives for thinking that it will be forced on one by a failure of realism. What he says is that we should move from the view of "language as conveyor of information" to a view of "language as a mode of action." I must say that, over this past year, I have come to feel that he is absolutely right in stressing language as action. In fact, we say so somewhere in the book, but then we say a lot of things.

**B:** Oh, I agree. It's just that what we need is a realist theory of action, one that relates action to information about the environment in which the action takes place.

**P:** We stressed the fact that properties and relations arise out of action, that is, out of the activity of living things. It is not that the properties and relations are just there, but that it is the regularities in the environment that are relevant to the active agent, that are there for it, in that they are individuated by it. But it is true that we didn't present a theory of action.

**I:** *Do you have a theory of action now?*

**B** and **P:** Not yet, but that is where the action is.

### *Realism and Representations*

**I:** *You clearly intend your brand of realism to be an alternative to representationalism, so perhaps we should turn to the issues raised by Fodor and Jackendoff.* **FODOR** *says that your view has undergone interesting developments and is now closer to representationalist semantics than possible worlds semantics, with the main difference being in the "metaphysical banners" involved. Do you agree?*

**P:** No, I think she is wrong on both counts. As to the basic task of semantics, I think we, the possible worlds theorists, and even those who try to get by with only extensional model theory are on one side, with the representational semanticists on the other. David Lewis, in a justly famous essay, put the point perfectly. In discussing the Katz-Postal idea that semantics was translation into what Lewis calls *Semantic Markerese* he says, "... [W]e can know the Markerese translation of an English sentence without knowing the first thing about the meaning of the English sentence: namely, the conditions under which it would be true. Semantics with no treatment of truth conditions is not semantics. Translation ... is at best a substitute for real semantics, relying either on our tacit competence ... as speakers of Markerese or on our ability to do real semantics at least for the one language Markerese." (From the introduction to Lewis's essay

"General Semantics.") Exactly the same points go for translation into Mentalese. It doesn't provide a semantics for English, unless we have a semantics for Mentalese, and vice-versa.

**I:** *Isn't that what Fodor is implicitly admitting?*

**B:** I wish I thought so, but I don't. Fodor plans to divide and conquer the semantical problem; *first* get the English to Mentalese relation right, and *then* do a "real" semantics for Mentalese. But surely the relation one is trying to establish, in carrying out the first step, is itself semantical. The English and Mentalese expressions won't have the same spelling, or pronunciation, or number of letters. Lewis's point is that what it means to get any such relation right is sameness of meaning, and that presupposes an account of meaning for Mentalese. So one needs a theory of meaning, one that allows that expressions of natural language and internal states of organisms can have the same meaning, one conventionally, the other by virtue of its role in the language user's perception, thought and action. Of course, that is just the kind of theory we try to provide.

**I:** *But in Chapter 10 you introduce efficient cognitive states and assign them meanings. Doesn't this just amount to a language of thought?*

**P:** Throughout the book, we emphasize that all sorts of events can be assigned meanings, relative to various constraints. We don't therefore conclude that all of reality is language. In Chapter 10, we suggest how the cognitive activity of adapted organisms might be assigned meaning relative to various constraints and suggest that is the way of looking at cognitive activity that underlies the attitudes. This doesn't require seeing these cognitive states as instances of language use any more than noting that smoke means fire requires thinking that smoke is a word for fire.

Actually, that reminds me of something. Underlying the representationalist idea seems to be a feeling that where there is meaning there must be language. Perhaps a similar idea led Berkeley to his view, which was really that smoke is God's way of telling us to expect to see fire if we get closer. This moved our hero, J. L. Austin, to comment "There won't be books in the babbling brooks until the dawn of hydrosemantics." Well, even Austin was wrong. Hydrosemantics, or at least pyrosemantics, has dawned, at least according to our book. But there still aren't books in the running brooks or in the head either, although there are things going on in some heads of certain ill-adapted organisms that lead them to write books.

Our review is just the reverse: *where there is language, there must be meaning*. That's why, whatever our confused advance publicist may have said, we could hardly believe that language can have its semantical properties without the presence of minds.

**B:** Yes, remember the point I was trying to make in response to Turvey

and Carello, about the relation between mind and meaning. For example, utterances carry information about the world they describe only because they are the products of people with minds that carry that information. It doesn't follow from that that the way to do semantics for language is to try to relate it to a language of thought. It's just the fear of that mistake that makes Turvey and Carello seem to want to disassociate meaning from minds.

**P:** Look. Suppose a time-traveller visiting us from the nineteenth century wants to write a manual that will allow those that follow him to drive automobiles. He has to figure out the meaning of turn-signals, say. The first step is to associate the signals with the movement of the automobiles – turn left, turn right, stop. That, plus the realization that these signals are the product of intentional activity of individuals who also control the cars, should allow his to figure out what the signals mean about the mental states of the driver. This is what we meant, in the book, by the priority of external significance.

**I:** *Do you deny that we use words and symbols to think with*?

**B:** How could you ask a mathematician that? Using a language, whether to communicate or think or prove a theorem, is a complex intentional activity. We don't see how such activity could underlie all cognition and so be a necessary pre-condition of itself, or why postulating such a language of thought is the least bit attractive. But we don't need to deny that we can do computations in our head or lay awake at night planning what to say at the board meeting the next day, or use images in trying to figure out what a new building might look like.

**I:** *Well it sounds like you think that representationalists should regard situation semantics as an enemy to be fended off, after all.*

**P:** No. We think, or conjecture, that meaningful cognitive states should serve all the legitimate purposes that Mentalese is supposed to serve, that the semantical tools we provide and the sketchy account of how to use them to represent mental states should be very helpful to those who want to work out theories of cognition and language. So we don't think of ourselves as an enemy to be fended off. But we are not just putting forward a notational variant of representationalism.

**I:** *Jerry Fodor argues for Mentalese and for the fact that thought should be syntactic computations over sentences of Mentalese. Part of his argument for talking of them as syntactic comes from the obvious structure that beliefs have. Do you think that meaningful cognitive states are syntactic*?

**P:** We certainly grant that meaningful states are structured, as our account of the attitudes makes clear, but we don't think they are like syntax.

**B:** But, then, one of the results of getting philosophers, logicians, computer scientists, and linguists together here at CSLI has been to discover what radically different ideas we all have about just what counts as syntax. Talk about shifting situations! Calling mental states syntactic and likening cognition to manipulation of syntactic objects based on form turns out to be a giant pun unless one says something substantive about what one takes syntax and syntactic operations to come to. There is a certain intuition that drives Fodor that is quite understandable, but I just don't think it has to drive you to think of meaningful mental states in terms of an intrinsically meaningful internal language.

**I:** *You said Janet Fodor was also wrong to think that you have adopted a more representationalist theory than your "advance publicity" suggested. But you also admitted that you have adopted promiscuous realism. Isn't it a bit hard to tell the difference?*

**P:** We have certainly changed our minds on many issues, but I am certainly no more a representationalist than I ever was. The whole point of all the stuff I did that fed into situation semantics was to emphasize the importance of what I called "ways of believing," the cognitive states one was in, by virtue of which, together with contextual facts, determined what one believed, etc. This same point of view was emphasized in our *Journal of Philosophy* paper. Even in Chapter 9, where we try to do the semantics of attitudes as relations to situations, we say that these relations are determined by the agent's states of mind at the time. It sounds to me like our advance publicist, whoever it was, may have been pretty confused.

**I:** *I suspect you will have even less enthusiasm for* **JACKENDOFF**'s *version of representationalism than for Fodor's.*

**B:** Yes. It's hard to know exactly what to say about Jackendoff's representationalism. Jackendoff is a smart man and he has made a lot of siginficant contributions to linguistics. Also, when you squint at his recent work, it looks sort of like ours, especially if we had come up with a better notation for event-types. He is just the sort of linguist we want to appeal to because he sees so much of what is wrong with the traditional approach through first-order logic. But if we try to follow his ideas about representationalism, we just get lost. We've gone slowly through his explanation several times, trying to get clear about what he could possibly mean, how the parts are supposed to fit together, but we always came out with something like our view, not his.

**P:** His basic notion seems to be that of a construal of the external world. This is the result of an interaction between external input and the "means available to internally represent it." This suggests to us an efficient cognitive state or frame of mind. The construal is both the result of the

interaction and *of* the external world. This makes it sound like it is a meaningful cognitive state, in our sense. So far, so good.

Such a construal will contain information, in our sense, about both the external world and the structure of the mental apparatus. Here, as always, an event contains information relative to a constraint. Again, so far, so good.

But then Jackendoff says something we just don't understand. He says that "since one's construal of the world is heavily mediated by complex computational processes which have nothing whatever to do with language . . . the semantics of natural language is more revealing of the internal representation of the world than of the external world *per se*." This argument seems like a total non-sequitur.

**B:** To see why, think of a photograph that comes out of a polaroid camera. This photograph is a product of an interaction between the input from the external world and the internal workings of the camera, which are quite complex. Does it follow that the photograph is more revealing of the internal workings of the camera than of the external world? The photograph will be quite revealing of both, depending on what other factors we keep constant. To most of us, the photograph will be a good indication of what the world was like in front of the camera. But, to the photographer at the time, who sees both the scene and the photograph, the photograph may indicate that the settings on the camera are maladjusted, or that there is a light leak, or that the film is old.

Similarly, we can use people's testimony about what is going on in front of them as an indication of how accurately they can see or as an indication of what was going on in front of them, depending on what else we hold constant. The whole idea of the testimony and the internal states it expresses, as the joint product of the brain and the external world, suggests this relational picture.

But if we just remove one of the elements in the relation, the world, the whole picture collapses. Without a world to compare the photograph against, it is useless for giving us information about the internal states of the camera. This seems so obvious that we think we must have missed out on something Jackendoff is getting at.

**P:** For example, Jackendoff talks about rules of inference as a part of his semantic theory. But how can one have a sound rule of inference, without the sort of thing Lewis called a real semantics? That's a lesson that was learned decades ago.

**I:** *But Jackendoff gives a number of specific criticisms of* Situations and Attitudes, *as well as sketching his own view.*

**P:** His first argument is that the continuous flow of matter in the physical

world does not come neatly segmented into events, or manners segregated from actions, and that these concepts are a product of our means of perception and cognition. Therefore, he says, there are no events, actions, manners, etc. This is another non-sequitur. It doesn't follow from the fact that things don't come physically segmented by nature in some neat way that they don't exist, and it doesn't follow from the fact that a concept K is the product of an organism's means of cognition and perception that K's don't exist.

His next argument is that since we admit to using the term "event" in a technical sense, we have conceded the former argument, and of course that's just plain silly.

**B:** His next argument has as a premise that humans have a widespread abstract system of organization that cuts across semantic domains, or "a priori unrelated semantic fields." We agree, if by "a priori unrelated semantic fields" one means "things that are in many ways quite different."

**P:** I think that taking this to be evidence against our view, betrays a fundamental misunderstanding. Consider Hoover Tower and Ron Rebholz.

**B:** Ron Rebholz?

**P:** What do they have in common? Not a hell of a lot. Yet, as I sit here, they are both to my left. Am I saying, or is my language implying, or would a realistic semantics for it imply, that there is some *simple property* that Ron Rebholz and Hoover Tower have in common, or perhaps that comes *neatly pre-segmented* by nature? Of course not. Are we then saying that being to the left of me is a matter of my internal representations, not a matter of what the world is like, but a psychological issue, something constructed by me, which, for all semantics should care, is a *pure figment of imagination*? Of course not. There is no reason to flit from one alternative to the other. Being to the left of me is a property things have, not a basic physical property, but one that plays an important part in my life that I can recognize and use to categorize things. My language evolved to fit the needs of ordinary Englishmen, not physicists, and there is no reason in the world why it shouldn't have simple words for the complex relational properties that play an important part in our lives. And there is no reason it shouldn't exhibit abstract patterns of grouping that pull together things that, when we abstract from the nature of human life and thought, don't have much in common.

**I:** *Well, I must admit I couldn't quite imagine what he was driving at there. But mustn't you grant him his next point, thay you were wrong in saying that the word "cookie" was a uniformity across situations*?

**B:** No, I don't think we do. It is really the same point. If you remove the

language users from the picture, it will be impossible to imagine what the uniformity could be. It's undoubtedly both complex and abstract. But we do, by and large, do a good job of recognizing situations in which the word "cookie" appears, in spite of all the different ways a situation can be. What point is there in denying that?

**I:** *But what value is there about talking about "invariants" and "uniformities", if one can't say what they are?*

**B:** Well, isn't that really sort of a naive question? Can we, even today, really say what colors are? It's a very complicated story, that involves not only properties of light and reflective surfaces, but also the structure of the visual apparatus and even cognition. What help would it be, in trying to come up with a theory of color and color perception, to deny that there are colors? One couldn't even set up one's experiments.

Look at what Jackendoff says. Situations are alike for us because we are constructed to be able to construe them as alike. Now imagine trying to construct something that will react the same way to different situations – construe them as alike. How would you do it? You would have to find some way in which the situations were the same and build something, however complicated, that responded to that sameness, however abstract. That is what you would have to do, to have things exactly as Jackendoff says they are. Now try to imagine doing it the way he seems to think it was done. Try to design something that will systematically respond in some way to different situations, without identifying any way in which the situations are the same. It just boggles the mind. That's why we find it hard to make sense of Jackendoff.

**P:** In a way, one can regard the whole import of efficiency, as the point that the uniformities in cognitive situations are not uniformities across the cognized objects, but across the whole situation, including the agent. Jackendoff pauses to note that this really is our position, says he is going to treat us more charitably, and then notes that, given this view, which of course is the one we held all along, his earlier objections don't have any bite. To save the position, he then argues that our use of the truth values, or 1 and 0, or yes and no, in our courses of events, presupposes that reality is broken up "categorically." I am not sure what he means by this; but, as far as I can tell, this notion of a categorical reality, where everything is settled, is exactly contrary to the partiality that we emphasize.

**B:** Jackendoff's final argument is against our treatment of the attitudes. He notes that if we say "John put Mary in his picture. She looked terrible." the second sentence can be taken in two ways. I guess one way says that Mary really looked terrible; the other says that Mary is represented as looking terrible in the picture. I just can't get straight why this is supposed

to be a problem for us or semantic innocence. And the later examples all seem ones to which our account of complex singular terms would bring much illumination. So I can't see how this was supposed to cut any ice at all.

**I:** *Do you disagree with Jackendoff's view, that the study of the semantics of natural language is above all the study of human cognition?*

**B:** I don't know about "above all." Language is part of the flow of information and the harnessing of information to guide action. But sure, semantics is part of the study of human perception, thought, and action. But Jackendoff goes on to say that semantics ought to be grounded in a satisfactory psychological theory. I guess what we really think is exactly the opposite. The basic notions that cognitive psychology, at any rate, uses are semantical. Go back to Locke. His ideas are *of* things, and it is by the things they are of that they get classified. It seems to us that the psychologist needs a theory of meaning, a semantics such as we try to provide, in the way that a statistician needs arithmetic. So, in that sense, cognitive psychology needs to be grounded in semantics.

## EFFICIENCY, CONSTRAINTS, AND THE RELATION THEORY OF MEANING

**I:** *Well, I guess that brings us to efficiency. You make it sound like a pretty profound concept. But isn't it just your funny word for what everyone else calls "context sensitivity."*

### *Context Sensitivity of Language*

**P:** No, that's not right – though it obviously came across that way to some of the commentators (**PARTEE, VAN BENTHEM**). We are using the term much more broadly than that. On our view, "efficiency is not just a feature of linguistic meaning, but is critical to all meaning." (*Situations and Attitudes*, page 14.) We talk about efficiency of perceptual conditions, types of events of all kinds, beliefs, and so on. But the phenomenon of efficiency, which shows up in language in a host of ways, including indexicality and context dependence, is at the core of our relation theory of meaning. If that doesn't come across to the reader, then we failed pretty miserably in our exposition.

**B:** For us, natural language is interesting as a fascinating example of action and information about the world interacting. That's what we want a theory of, though, not just linguistic meaning.

**I:** *This seems to suggest a shift of interests. Wasn't the subtitle of the second*

*book supposed to be "A mathematical theory of linguistic meaning?"*
**B** and **P:** Yes. We chose that title back before the bigger issues started attracting us. We are now after bigger fish.

### *Conventional Constraints*

**I:** *I stand corrected on your use of "efficiency," then. As I see it, one of the big changes between this book and your paper of the same title is the move from thinking of meanings as a relation between events, so thinking of meanings as constraints, which are relations between types of events. Would you say that is correct?*
**B** and **P:** Absolutely.
**I:** *What do you think of* **DRETSKE***'s first point, that the relation between types of events that captures linguistic meaning is really rather different from the other types of constraints you talk about and that it is probably a mistake to talk about them as constraints at all?*
**B:** We called them conventional constraints to emphasize their similarity to other constraints, relations between types of situations such that a situation of one type can carry information about a situation of another type. But Dretske's point is well taken. It may be misleading to call them constraints, though they do constrain communication that conveys genuine information.
**I:** *Dretske's main complaint is that you have not gone far enough, that the interesting question is not to identify the constraint, but rather to figure out what does the constraining.*
**P:** I certainly agree that it is an important question that we did not try to answer. My hunch is that it will come from understanding the relation between the information, intention, the mind, and action, and that Grice had a missing piece, intention to convey information.

Think of the meaning of a sentence, say, *Here's a cookie*, as a relation between types of events, and think about me here, with this cookie, wanting to give it to you. What is it about me and my situation, above and beyond what we isolate in the book, that plays a role in my being able to convey the information to you that there's a cookie? That is, beside the fact that I have the information that there's a cookie, what typically is it about me that makes my utterance mean that there is a cookie? I think it is that I intend to convey the information that I have a cookie.
**I:** *You are saying that besides having the information, you also have the intentions to convey that information. Grice takes this as the hallmark of non-natural meaning. But then aren't these intentions to communicate also part of the meaning?*

**B:** You have to be very careful here. Grice is, but it is easy to miss an important point. We can put it in our terms. Recall that we distinguish between the information a statement can contain, its meaning, and its interpretation. These are three different, but related things. It is important to realize that we can account for the ability of a statement to convey the complex sorts of information about intentions that Grice notices without having to build iterated intentions into the meaning of the sentence.To think that all the information conveyed by an utterance has to be part of its meaning, thought of as a relation between types of situations, is another form of the fallacy of misplaced information.

**I:** *Can you give any hint as to how these complex intentions are conveyed? It seems rather different from the examples of misplaced information you describe in the book.*

**B:** I've talked with Stephen Neale about this. We have come to think that the ability to convey complex intentions in the way Grice points out arises from the fact that the conventional constraints of a language are *public* information within a linguistic community. This seems to me to be a very important property of linguistic constraints, and some other conventional constraints, but not of necessary or nomic constraints, one that we did not mention in the book.

**I:** *How could you have a conventional constraint whose existence wasn't public information?*

**B:** It's hard to give an example, since that requires that I come up with some conventional constraint that is not common knowledge, and I know little that is not common knowledge. But it is just the sort of thing a linguist would notice. Lauri Karttunen, who grew up in Finland, tells me that the way one sneezes is conventional. In America we "achoo" while in Finland they "achee." This means that the way we sneeze actually carries information about where we are from. Of course, most people are unaware of this relation between the way they sneeze and where they are from. It is one of the things that might give away a Finn trying to pass himself off as an American, I suppose.

**I:** *So this is an example of a conventional constraint that can carry information, but not the kind of information that Grice is building into his notion of non-natural meaning?*

**B:** That's right. If I had gone to Finland and said "achoo," before Lauri told me about this relation, I would have conveyed to some people, those like Lauri that understand this relation, that I am not from Finland. However, I wouldn't have sneezed this way with the intention of communicating this information, hence, I wouldn't have conveyed the information that I intend for them to pick up this information.

But, what's more, even now that I know this relation, so that I may intend to sneeze in the American way, and so intend to convey information about by foreignness, I cannot intend to convey my intentions to convey my foreignness, the way I could by saying "I'm from America," or its Finnish equivalent.

**I:** *What is not so clear is how the situation would change if the convention became public information.*

**P:** Well, since sneezing is involuntary, we would probably need to assume that there was also another option, a neutral way to sneeze, analogous to remaining silent rather than talking. Lets call that "achugh." So if we are Americans we can "achoo" or "achugh." If we are Finns, we can achee or achugh. We do the former when we intend to convey our nationality, the latter when we intend to remain silent on the matter.

**B:** I see. If we had these options, and if I chose to "achoo" when I sneezed, then I would be intending to convey my nationality, and those very intentions could be inferred from knowledge of the relevant constraint, even though intentions are not part of what I said or what I meant to say.

**P:** Something like that. And you could even lie, by saying "achee."

**I:** *You are saying that an important part of linguistic meaning constraints, one that you did not mention in the book, is the fact that the relevant "constraints" are public information within a linguistic community. With this, you hope to keep iterated intentions out of the meaning relation. It might work, but even so, just what is public information?*

**B:** I was afraid you would ask that. It seems clear that there is such a thing and that we take advantage of it all the time, but just what makes information public is hard to understand. To me, it has long been a fascinating question. That's why I put an instance of the Conway Paradox in my paper "Scenes and Other Situations," with the dirty children puzzle. Unfortunately, I don't have anything interesting to say about it. There are several papers around on public information and its relation to the Conway Paradox, but none of them seem to me to get at just what public information is and how it works.

**I:** *As long as we have slipped into talking about constraints, perhaps we should discuss* **LANDMAN***'s comments on constraints at this point. While Dretske fails to see how constraints can be anywhere but in the world, Landman says that they "just ain't in the world." He has a rather technical argument that I failed to follow in all its details, but it looks pretty impressive. He seems to show that your notion of constraints gets the logic of conditionals completely wrong.*

**B:** Landman's arguments are based on two assumptions. In one he misses the point you observed above, that the real move in the book, as opposed to the earlier article, is to think of constraints as relations on *types* of events, with parameters in common, not on events themselves. He deliberately trivializes constraints by considering only those that do not involve parameters at all!

The main problem with conditionals has always been to get some kind of a link between the state of affairs described by the antecedent and that described by the consequent. All the work on relevance logic, entailment, strict implications, has been after that. What we have found is that the move to types of situations, with roles common to the antecedent and consequent types, is just what seems to be needed. How far they will go in a theory of the conditional is another matter, but it is the types that are the crucial move. Even if the rest of his argument were sound it would be like arguing that the standard definition of the factorial function is wrong because it is counter-intuitive to have $0! = 1$. No one really cares about the value of 0!, and we don't think the case of constraints between events is interesting.

**P:** His next move is to argue that to get modus ponens to work, we need to admit that the world is Humean, that is, that we have to give up the distinction between real constraints and accidental patterns. But his argument is based on his desired conclusion.

He takes it that modus ponens should be the rule that if the world $M$ respects a constraint $P \Rightarrow Q$ and $M$ respects $P$ then $M$ respects $Q$. But that is not right. Modus ponens is the rule that if a constraint $P \Rightarrow Q$ is actual and if $P$ is actual then $Q$ is actual. It is only if you think that respecting a constraint is the same as the constraint holding (which is what he is in the process of arguing for) that you would take his definition of modus ponens.

Here is an example of the kind of thing Landman seems to think is a counterexample to modus ponens. Let $P$ be the constraint that being over 40 involves being wise. Imagine that in creating the world, God did not make $P$ a real constraint, but that, for lack of imagination about a more useful gift, he just happened to bestow wisdom on each person as a 40th birthday present. Now let $Q$ be the proposition that God, in creating the world, didn't want to be just an accident that those over 40 were wise. Then the world will respect the constraint $P \Rightarrow Q$, by failure of the antecedent. It will respect the constraint $P$, by accidental generalization. $Q$ won't be true, but so what? $P$ was not an actual constraint.

**B:** So, anyway, Landman makes these two assumptions that are completely out of the spirit of the book and then shows that the resulting logic

of constraints is odd. It seems a very odd sort of argument and does not sway me at all.

**P:** Nor me. We say in the book that the world *might* be Humean, but that that is not the common sense view of the world. You see, if the world is Humean, then possible and necessary constraints coincide. In fact, as Jan van Eijck recently pointed out to us, in that case every situation that is compatible with the actual situations would also be factual, so that every possible situation would also be factual. All modal distinctions disappear. Can that be what Landman wants? It seems unlikely.

**I:** *I suppose you would make the same sort of reply to* **THOMASON**, *since he explicitly ignores types of events in his discussion of the proper reference of gerundive nominals.*

**B** and **P:** Say more.

**I:** *Well, he argues that events are not fine grained enough for interpreting gerundive nominals. He thinks they probably denote propositions. But the whole point of your section on nominals was to show that something like types of events would be needed.*

**P:** I think that's right. All of his examples seem to point to just what we were saying, that gerundive nominals get at the anchoring of event-types to events, sometimes multiply, which is something we didn't notice.

**B:** Speaking of propositions, though, I recall now that in commenting in Philadelphia on our early paper "Situations and Attitudes," Thomason urged us to admit 0-ary properties as propositions. At the time, that didn't seem right, because we had all properties and relations located in space-time. In retrospect, though, in view of the discussion above, I see what he was getting at. Too bad we didn't listen then. Maybe we shouldn't be too hasty now.

**P:** Yes, that's true. The anchoring of an event-type to an event is just the sort of thing that gives rise to a proposition, in this new way of thinking, so maybe we are not that far apart. He needs to take types more seriously, and we need to take propositions more seriously.

## INFORMATION

**I:** *On that happy note, maybe we should leave efficiency, if we are still there, and move on to information, per se.*

### *Partial Information*

I guess we have already discussed part of it, the emphasis on *partial* information in the discussion of possible worlds. And then, in your view, it

is constraints that allow one situation to contain information about another, so we were implicitly discussing it here too. Is there any more to be said about the emphasis on partial information?

**P:** The whole treatment of event-types and constraints presupposes the use of partial situations, too, so it has been under the table all the time.

**B:** There is one thing we haven't discussed, along those lines, that would have come up, I think, if more computational linguists had written comments. It has to do with the advantage that partial information has over sets of total ways of filling out the information. It is just much more manageable.

**I:** *For example*?

**P:** Contrast the effect of learning a sequence $f_1$, $f_2$, $f_3$ of new facts in situation semantics terms, versus possible worlds terms. In the former we can see this as successively building up a factual situation. In the latter, each one is seen as cutting down on an infinite set of possible worlds. If you are interested in actual mechanisms for representing such information, which is what computational linguists are forced to consider, then the latter is obviously much less straightforward.

**B:** Robin Cooper and Stanley Peters have both pointed out that a similar strategy could apply to problems of ambiguity, where you might think of an ambiguous expression as giving one a set of possible interpretations, to be narrowed down later. The above suggests thinking of ambiguity and its resolution differently, as partial information, to be increased later. This is an idea that has already been fruitful in various treatments of syntactic ambiguity that use what is called "unification." Some of the tools we have been developing lately seem very appropriate for such a semantic analogue of this approach. Take the case of the word *on*, mentioned above. Rather than thinking of *on* as giving you a set of possible relations, which has to be narrowed down later, we want to think of it as providing partial information about which relation is relevant, information that can be fleshed out by context, both linguistic and extra-linguistic.

**I:** *Your mention of factual situations reminded me of something. Don't you confuse two things, or at least not make the distinction very clearly, namely partial information about the whole, with total information about a part. Isn't there a difference between situations as real parts of all there is and situations as partial information about all there is*?

**B:** I see why the reference to factual situations brought that to mind. I fear you are right. You recall that we distinguish between those situation-models that were actual, i.e., those that were models of real situations, and those that were factual, i.e., parts of actual ones. Factual situations correspond to partial information about real situations, whereas actual

ones correspond to real situations which are part of all there is. So I think your distinction is there, even if it is not very clear. I agree, though, that it is an important distinction.

**P:** That, in turn, reminds me of something we should mention. In the book, our definition of event-type and what it was for an event to be of a given type had the property that if an event was of a given type, then any event of which it was a part was also of that type. This is what gave us such problems with persistence of information. We now feel that our notion of event-type was much too impoverished and that it should not have the property I just mentioned.

**I:** *I wondered about that. It seemed to me that you had a mechanism for putting conditions on the internal structure of an event, but no mechanism for putting conditions on the global properties of events.*

**B:** That is not quite right, but close enough. We do allow indeterminates over events in event-types, so they constrain the external structure of the events to which they get anchored. What we cannot do with the machinery in the book is to simultaneously put internal and external constraint on the same situation. Sometimes what one needs to be able to do is to describe conditions that are explicitly about the internal structure of some type **s** of situation

in **s**: at **l**: sitting, **x**; yes

so that we can use that type in some other condition, say

in **s**′: at **h**: sees, **i**, **s**; yes,

We think of these as describing real conditions on anchors, which are themselves situations.

You can think of our oversight here as an instance of what we keep warning others against, thinking of the properties of a situation as only those that are common to all its extensions. We built that into the weak notion of event-type in the book, believe it or not.

## *The Flow of Information*

**I:** *We have strayed away from our topic: information. Barwise, in his commentary on Dretske's book Knowledge and the Flow of Information took* **DRETSKE** *to task on two points. One was not recognizing explicitly enough that information is always relative to constraints and so can only be picked up by agents who are appropriately attuned to the constraints. I can see where this complaint comes from and how that gets handled in situation semantics, since you have explicitly posited the constraints as the repository of the ability*

*for one situation to contain information about another. But Barwise also complained about Dretske's definition of information on the basis that it precluded there ever being information about some of his favorite facts, mathematical truths. I suppose you feel that your approach gets around this, but it is striking that you didn't say anything about it.*

**B:** Well, since we don't recognize even the laws of logic as true in every situation, it seemed pretty obvious that we were not going to get other sorts of mathematical laws coming out as true in every situation. Mathematical laws themselves are certain kinds of necessary constraints, arising out of the very process of individuating the mathematical objects, properties, and relations. Relative to some of those constraints, others may follow, but the fact that they follow will be another fact, the kind of fact that giving a proof contains information about.

**I:** *So you would say that a proof of some theorem contains information about the mathematical objects it is about? Relevant to what constraints?*

**B:** That would depend on the kind of proof, wouldn't it? The axioms and rules of logic are nothing but certain necessary constraints. Constructive proofs are those that are more constrained, and so contain more information.

**I:** *Another very glaring omission in your book is that while you talk a lot about information and how it flows along constraints, you don't really have information per se in the world.*

**P:** That worried us, too. Again, I think that we were misled by doing model theory instead of real semantics. Of course, information is in the world; it is just a matter of understanding its structure and relation to things like constraints, situations, and the like. We feel like we understand a lot more about its structure than we did when we started.

**I:** *Do you think that information is propositional? That is, does having propositions solve the problem of having information in the world?*

**B:** That is an attractive suggestion. One of the annoying things one comes up with in talking about information is that English has this word for the uniformity we call information and one for the uniformity we call misinformation, but it seemed to us that there was no word for what was common to both, something neutral between information and misinformation. But maybe that is just what propositions are, an invariant across information and misinformation.

**I:** *So true propositions are information false ones are misinformation?*

**P:** Something like that sounds pretty plausible to me. Notice that it fits neatly with our theory of constraints and what we said earlier about propositions. Take some constraint like that kissing involves touching: $S_1 \Rightarrow S_2$, where $S_1 = [\mathbf{s}_1 \mid C_1]$, and similarly for $S_2$, where $C_1$ and $C_2$ are the following conditions:

in $\mathbf{s}_1$: at **l**: kissing, **a**, **b**; yes
in $\mathbf{s}_2$: at **l**: touching, **a**, **b**; yes

Now suppose you have a mushy family reunion $s$, which is of type $S_1$ many times over, and you want to know what information this constraint provides about who touched whom. Clearly what matters is not just that $s$ is of type $S_1$ but rather what anchors $f$ meet the condition. The propositions that $a$ kissed $b$ would imply (relative to our constraint) the proposition that $a$ touched $b$, but only if the former were true would you have information about a touching.

**B:** There is a problem with this, or with making it square with one aspect of our treatment of constraints in the book. In the book, we allowed constraints of the form $S_1 \Rightarrow S_2$ where there were roles in the defining conditions for $S_2$ that were not in $S_1$. I think that was a mistake and was really just what our computer scientist friends call a "hack," to sneak in existential quantification. Certainly if you think of indeterminates as we suggested earlier, as ways of modelling relations between the argument places of relations, this makes a lot of sense. The way we had it before, we allowed one situation $s$ to contain information about things that were not tied to $s$ in any way at all.

**I:** *Then how would you bring in quantification?*

**P:** Explicitly, as relations between types of things. Take our favorite example. A lion is eating. The lion is being watched by a hungry vulture. By seeing that the lion is eating, the vulture learns that there is some animal there for it to eat, too. In the book, we represented this constraint with two event types, the antecedent type $S_1$ having an indeterminate **a** for the lion and one **l** for the location, the conclusion type $S_2$ having three indeterminates, **a**, **l**, and one **b** for the animal that the lion is eating:

$S_1 = [\mathbf{s}_1 \mid \text{in } \mathbf{s}_1\text{: at } \mathbf{l}\text{: eating}_p, \mathbf{a}; \text{ yes}]$
$S_2 = [\mathbf{s}_2 \mid \text{in } \mathbf{s}_2\text{: at } \mathbf{l}\text{: eating}_r, \mathbf{a}, \mathbf{b}; \text{ yes}$
$\text{in } \mathbf{s}_2\text{: animal, } \mathbf{b}; \text{ yes}]$

Here is a different way to do it that does not violate the argument structure. Think of the relation of co-instantiation that holds between two types **x** and **y** of things if there is something of both types. Replace $S_2$ by the type $S_3$ of situation where this relation holds between the type of thing [**x**] that is an animal and the type [**y**] of thing that of being eaten by **a** at **l**

$S_3 = [\mathbf{s}_3 \mid \text{in } \mathbf{s}_3\text{: co-instantiated, } [\mathbf{x}], [\mathbf{y}]; \text{ yes}]$

where the latter two types are given by

in $\mathbf{s}_3$: animal, **x**; yes
in $\mathbf{s}_3$: at **l**: eating$_r$, **a**, **y**; yes

**B:** We are being a little sloppy in the notation in the first line. What we should really have, rather than [**x**], is the type

[**x** | in $\mathbf{s}_3$: animal, **x**; yes]

of thing that is an animal, and similarly for [**y**].

We knew we were going to have to have this sort of relation between types when we treated general noun phrases, treating the semantic value of *every* as the inclusion relation on types, the value of *no* as the disjointness relation on types, and so on. But we thought we could sneak by with the above hack in this book. Like most shortcuts in intellectual work, though, it has come back to haunt us. You really want the structure of indeterminates to carry some weight, in talking about information flow, and you lose that with the hack. For example, if you don't do this, then you could have a constraint $S_1 \Rightarrow S_2$, and a proposition corresponding to some anchoring that makes $s$ of type $S_1$ without getting a proposition out as a conclusion, since there would be unanchored roles.

**P:** Also, it left us open to Soames's problem, by having our situations have less structure than is really there, but that is getting ahead of ourselves, I guess. But the main point is that there are relations between types of things, as well as between things, and these "higher order" relations are crucial to an account of the attitudes.

## THE ATTITUDES

**I:** *Then let's turn to the attitudes. I suspect that many of your readers will not make it as far as Chapters* 9 *and* 10, *where you give your account of the attitudes, so let me remind them that you give two quite different accounts. In Chapter* 9 *you give an account of the attitudes as relations to situations, building on the ideas about seeing. Let's call this your* RTS *semantics for attitude reports. You end Chapter* 9 *with a list of problems for* RTS *semantics. Then, in Chapter* 10, *you give an account in terms of what you call "frames of mind." I'll call this your* FOM *semantics for the attitudes. I would like to understand the reasons for your move from* RTS *semantics to* FOM *semantics better.*

**P:** Well, there were several reasons, but they fall into two kinds of problems that we ran into. One set of problems arises from our own high expectations of what a semantic theory should be. The other set of problems arises from set-theoretic difficulties. The latter now seem to us to

arise from our doing set-theoretic model theory rather than real semantics. The former, though, are real problems that any decent theory is going to have to confront. You might say that our problems come from our own demands for a rich theory without the ontological apparatus necessary to obtain such a theory.

**I:** *Just what do you expect of a semantic theory of the attitudes that Montague Grammar doesn't give you, if anything, besides a plausible form of realism?*

**B:** There are really two ideas about what a semantics for attitude reports, or any other part of natural language, for that matter, should do. One conception, the one I had before working with John, goes like this. We have intuitions about the logical behavior of a certain class of sentences. With attitudes reports these are largely intuitions about the phenomenon of "opacity:" reluctance to substitute co-referential terms and the like. We codify these intuitions in a set of logical principles, and then semantics consists of finding a collection of plausible set-theoretic models that makes the logical principles come out correct.

I think this is the traditional conception in semantics, and it is the setting for Montague Grammar, but it is what I would now call the thin conception of semantics. Opposed to this is the rich conception, where a semantics is a theory of meaning, which has a lot more evidence to account for than "logical intuitions," and thus has to provide more than a collection of models that makes the inferences come out right.

**P:** The rich conception, applied to the attitudes, must confront the fact that the attitudes *work*; that is, we successfully use the information in attitude reports, along with principles of common sense psychology, to explain the predict all sorts of things. In particular, it has to assign meanings to statements of folk psychology, as well as simple attitude reports, that are rich enough to account for the information they actually carry. Thus, a semantics for the attitudes has to be compatible with common sense psychology and the nature of the mind. It doesn't have to solve all of those posed by these topics, but it does have to be compatible with a reasonable approach.

**B:** The semantics of the attitudes isn't a matter of designing a machine that makes a canon of inferences work, but of constructing a theory that fits together a whole bunch of difficult topics: general facts about communication and language and special facts about the uses we make of the attitudes and the particular parts of reality, the minds and actions of intelligent agents, that we use them to describe. For better or worse, that is what we take to be the task, and this conception makes us feel that the RTS account is inadequate and would be even without the set-theoretic problems.

*Basic Picture of the Attitudes*

**I:** *Maybe you should give us your basic picture of how the attitudes work, so that we can discuss your expectations from a semantic theory more coherently.*

**P:** Our overall picture is this. A person has an attitude (believes, knows, doubts, sees that, sees, imagines, etc.) by being in a certain frame of mind, with various ideas and concepts anchored to the world in various ways. Frames of mind are modelled in our theory by indexed event types, roughly what we are now calling conditions on anchors. The ideas and concepts are modelled by roles, which are anchored by real relations to objects in the world, like holding something, or seeing it, or having information about it through some chain of information, or being told about it by someone else. Thus an attitude situation has an interpretation: the type of situation one gets when one anchors the event type via the anchor.

**B:** Given this general picture of an attitude, we get the following picture of attitude reports. Basically, the way an attitude reports works, is that the embedded sentence in the attitude report has to have the same interpretation, with the reporter's discourse situation and connections doing the anchoring, as does the attitude situation reported. We started with the idea that the interpretation of the embedded sentence is to be a collection of situations. Thus, in the RTS account, we tried to do the semantics directly in terms of relations to the situations that are in the interpretation.

**P:** The trouble is, this just doesn't let one give a rich semantics, in the sense of providing insights about common sense psychology. It is a bit better than Montague Grammar, in that it does not predict that one believes everything logically equivalent to what one believes, but it is nowhere near good enough. For example, if you and I both believe that I am talking too much, then we are related to exactly the same proposition, but I am the one that will try to talk less, you will try to talk more. At the level of propositions, we just don't get all of the uniformities we need. It is to capture such aspects of common sense psychology that we needed to bring the frame of mind in directly.

**I:** *It's all very fine to say that frames of mind can be characterized by event types, but why exactly can they be so characterized? You guys are critical of the idea that the attitudes involve relations to representations. But aren't your "frames of mind" representations?*

**B:** No, there's the beauty of it. The fact that a cognitive situation can be treated as frames of mind (which we model by indexed event types) connected to the world (which we model with anchors) is just a special case of events being meaningful.

**I:** *But a meaningful event has to be meaningful to someone. Presumably my frame of mind is meaningful to me. After all, it is my action that it guides. Right? But then it seems I must perceive and interpret it. Why doesn't that make frames of mind representations in the mind?*

**P:** That is a very seductive way to talk, but it's not the way we look at it. Your frames of mind don't *guide* your action, the way a map or a recipe or a fortune cookie might; that would involve a pretty nasty regress. Rather, they *control* your action. That's what makes them meaningful.

**I:** *It sounds like you think my frame of mind is primarily meaningful for others.*

**P:** Exactly, That's what we take common sense psychology to be all about. One starts with the fact that we make sense out of each other's actions, that we learn about the world from each other, and that we are very good at predicting each other's behavior. If you see that there is snow on the lawn, then you are in a frame of mind that means there is snow on the lawn. If you believe there are cookies in the jar, you are in a state that means that you will behave in a way that will work if there are cookies in the jar.

**I:** *Aren't you "flirting with behaviorism" with this idea of a mental state controlling your action? It's a charge I have heard Jerry Fodor level at you.*

**P:** There is a difference between flirting and having a serious relation. Behaviorism may have been a mistake, but it was not completely crazy, any more than representationalism is. We think what was right about it was just that we understand each other's behavior in terms of meaningful mental states and the system of constraints embedded in common sense psychology, nothing more.

**I:** *I see, I think, how this idea of meaningful frame of mind, or mental state, fits into your general theory of meaning. This, I guess, is part of what you were getting at in the talk about the concept of efficiency. If there are to be lawlike connections between our mental states and other parts of the world – what we see, or our own future behavior – then mental states will have to correspond to types of events. They will be "recyclable." We will need to distinguish between the meaning of the mental state and the interpretation of someone's being in it in a particular setting.*

**B:** That's the idea, exactly.

**I:** *That's fine and good. But there seem to be some pretty large loans on further research in this approach.*

**P:** That's quite right, in two senses. First, the basic picture is our version of *functionalism*, a philosophy of mind developed by Armstrong, Lewis, Putnam, Jerry Fodor, Davidson and others – a point of view that we think is a truly exciting development in the history of philosophy.

**I:** *I thought those guys all disagreed – that functionalism, for example, was Fodor and Putnam's alternative to Armstrong and Lewis's identity theory, that Fodor had given it up, and that Davidson had his own version.*
**P:** Well, when you are standing on the shoulders of giants they sometimes look more alike than they do to each other. Central to the perspective is that our concept of mental state is of states apt to cause certain behavior (Armstrong), or states that play certain causal roles (Lewis). The disagreements pale in significance to the power of this idea, which is pretty much shared. We use "functionalism" for this broad shared idea, not for whatever particular version of it also goes by that name right now.
**I:** *So your theory is a combination of functionalism, in this broad sense, plus your general theory of meaning, which allows you to assign interpretations to types of events in general, and so to mental states, which are just types of mental situations, in particular. And the efficiency of mental states is really just a corollary of all of this.*
**P:** Right.
**I:** *But you said there was another loan on research, too.*
**P:** Yes, what I said above about belief brought in the idea of interpreting a state, partly in terms of what it means about the future behavior of the agent, in terms of that behavior being successful. It is this sort of meaningfulness that separates mental states of organisms from all sorts of other meaningful states. If and when we understand action better, there will be more to say about this.

### *Soames' Puzzle*

**I:** *OK, I'll grant that trying to think through the attitudes within some such general conception of mind has merit, and even that your picture is kind of neat. But still, there are details that have to turn out right, and I see a problem. Or, rather,* **SOAMES** *saw it. Let's see if I understand the basic ways things work, though. An agent is in a frame of mind at a location, with the roles in the indexed event-type that represents the frame of mind corresponding to concepts or ideas. These are anchored to things in the world. The frame of mind plus the anchor gives us an interpretation. Then the basic idea of an attitude report is that the embedded sentence has to have the same interpretation as the attitude itself. Right?*
**B** and **P:** Right. Or at least strongly imply it.
**I:** *But this means that the way the attitude reporter identifies the objects shouldn't really matter, so that we should be able to substitute coreferential terms within attitude reports. It's all very fine to say that you aren't doing thin semantics, but look. Our feeling that one cannot substitute, say,*

*"Tully" for "Cicero" in "Sam believes that Cicero was an orator." even if Tully was Cicero, isn't just some illusion held by logicians who haven't thought about the mind. Quite the opposite. It seems to tie right into a central fact about the mind, that we believe and perceive and know things under a description, or relative to some mode of presentation, or representation, or something. This is a central problem for theorists like Kripke, Kaplan, Montague, and yourselves who adopt some sort of theory of "direct reference." You can't just wish away these troublesome facts.*

**P:** We can't wish them away, but we can distinguish between a host of different facts and a certain explanation for them. Your remarks really have the following form. There is a certain phenomenon, what we call "reluctance to substitute" in the book. Call it the opacity of the attitudes, if that makes you happier. Then there is another phenomenon, which we'll call the intentionality of mind and action. You seem to take it that the latter explains the former in a straightforward way. The job of the embedded sentence in an attitude report is to capture the way the agent holds the attitude. This is a very pretty picture, built right into Frege's theory of sense and reference, but it just doesn't work.

**I:** *Because of the difference in perspective, I suppose.*

**B:** Yes, Consider this example. You say to me, pointing at John, "That fellow needs a shave." I say to John, "The interviewer said that you need a shave." That's perfectly OK, even though "you" is not the way you referred to John.

**I:** *But everyone admits that cases involving indexicals need special attention. Why should this special case threaten the soundness of the basic picture?*

**B:** Well, we don't admit what you say everyone admits. That's the difference suggested by the change from the term "context sensitivity" to "efficiency." We think of efficiency as an absolutely ubiquitous phenomenon. Indexicals bring out blatantly something that is always present. It is eternal sentences that are the special case.

**I:** *So do you deny a connection between intensionality and opacity?*

**B:** No, no, not at all. But we regard opacity – the reluctance to substitute – as a complicated phenomenon, with various explanations in various cases. Sometimes there is a straightforward semantical explanation. In our theory of descriptions, "The youngest man in the room needs a shave" and "The shortest man in the room needs a shave" described different situations, if the descriptions are taken attributively, so we cannot substitute one for the other within an attitude report.

**I:** *I have read the book, and your theory of descriptions seems like one of the more successful, or at least promising, parts of the theory. But when it comes*

*to the attitudes, the hardest cases involve co-referential names, like "Tully" and "Cicero" or "Hesperus" and "Phosphorus." With names you have a much more complicated story to tell, one that involves the purposes of the report, brings in the idea of describing parts of situations, applied beliefs, and such. Well, I can't say that this is the wrong approach. It always has struck me as extremely odd that since Frege discussion of the semantics of attitudes reports has been carried on with so little attention to a theory of what the attitudes are. In fact, I was pretty convinced by your account until I read Soames' essay.*

**B** and **P:** Well, you're not alone. Soames' piece has caused us to do a lot of serious thinking. That, plus a general questioning of the relation between real world semantics and model theory from Brian Smith, have been largely responsible for a lot of the shifting attitudes you've already uncovered.

**I:** *You agree that there is a problem, then?*

**P:** I don't think there is anything wrong with the basic picture, but there certainly is a problem with the semantic account in the book. I think Soames has come up with a genuinely new puzzle here, which is rare in philosophy. I also think it is much more serious than many readers are likely to realize. Linguists tend to ignore problems involving beliefs about the reference of "Hesperus" and "Phosphorus" as a hangup of philosophers, but the basic trick that Soames is using here can be pulled in all kinds of contexts, so that versions of it would infect the semantics of not just attitude verbs, but also verbs like *means*, *causes*, *implies*, and the like.

**B:** John is right, that there is a genuinely new puzzle here. I think that is important to keep in mind, because our account, where we emphasize the importance of conventional constraints on the way an attitude report works, where one is supposed to ignore irrelevant anchors and unapplied concepts, does quite well for the traditional puzzles, like Kripke's Pierre. It does fall short, though, with Soames' puzzle.

**I:** *I'm relieved to hear that because I found both your account of names and his argument pretty persuasive. I think the discussion will be helped if we distinguish between two things, the general argument that Soames is making and a certain derivation that he carries out in the course of that argument. Let's call them "Soames' argument" and "Soames' derivation," respectively.*

*Soames' argument is that any approach to the attitudes that takes the interpretation of the embedded sentence as the circumstances that support its truth (and here he is neutral between possible worlds and situations as circumstances) is committed to certain logical principles. Let's call them "circumstance logic." Using circumstance logic, Soames derives something*

*false from something true. Here is the version of his argument that is aimed most squarely at something you claim to give an adequate semantics for*:

(1) The ancients believed that (a) "Hesperus" referred to Hesperus and "Phosphorus" referred to Phosphorus.

(2) The ancients believed that (b) "Hesperus" referred to Hesperus and "Phosphorus" referred to Hesperus.

(3) The ancients believed that (c) "Hesperus" referred to a heavenly body$_i$ and "Phosphorus" referred to it$_i$.

(4) The ancients believed that (d) "Hesperus" and "Phosphorus" were co-referential.

*In* (3) *take the reading where the indefinite description is given an inner attributive reading, i.e., has narrow scope with respect to* believed *and with the subscript "i" indicating that the pronoun takes the indefinite description as antecedent.*

*Do you agree that both your* RTS *and* FOM *semantics satisfies his circumstance logic, and that with this logic, the above is valid*?

**P:** We certainly don't have to buy the step from (3) to (4), since there is a shift of subject matter, with a new property of sets of names being talked about. And we would argue that (1) isn't a good way to report the facts, since the ancients really had two beliefs, which is getting reported as one, but these are quibbles. Getting from (1) to (3) is bad enough. So, yes, both of our semantic accounts of the attitudes do satisfy Soames' logical principles, and yes, they do get one from (1) to (3).

**B:** But really, from our point of view, the situation is even worse than that, if possible.

**I:** *How's that*?

**B:** It would be relatively easy to modify the FOM semantics so that it would not be closed under strong consequence, only strong equivalence, which would get us out of one of Soames' logical principles. The idea would be to require not just that the interpretation of the agent's frame of mind strongly imply the interpretation of the reporter's embedded sentence, as we did in the book, but to require that they be the same. Call this FOM′ semantics. This was our first inclination, on hearing the puzzle from Soames, since the definition is really more in keeping with the philosophical picture we had anyway.

**I:** *So, that sounds like an easy way out. What is wrong*?

**P:** Well, even though Soames' *argument* (that is, his argument for why his derivation will be seen as valid for any semantics that uses circumstances for the interpretation of the embedded sentence) breaks down, the damned derivation of (3) from (1) comes out valid in FOM′ anyway.

**I:** *Oh. That must have been a shock. I know you accept the move from* (1) *to* (2)*, and so does Soames. I am sure that some of your readers will find that the problem, though.*

**P:** In our account, the move from (1) to (2) is fine if what one is after is evidence about what the world is like, that is, about what names referred to what, but misleading if what one is after is evidence about what the ancients would have admitted to be the case, since it uses an unapplied concept. But being misleading does not make it false. And, anyway, Soames has additional good arguments for why that step cannot be the source of the problem.

**I:** *So, I guess you would agree with Soames that it is the step from* (2) *to* (3) *that is wrong. If it is not too painful for you, I think it would be worth while if you would show us why this step is valid in* FOM *and* FOM′ *semantics.*

**P:** OK. Let us represent the ancients' state of belief with a frame of mind $S(\mathbf{s}, \mathbf{h}, \mathbf{v}_1, \mathbf{v}_2)$, and anchor $f$, where $S$ has two concepts $\mathbf{v}_1$ and $\mathbf{v}_2$ of Venus, with the following beliefs:

in **s**: at **h**: refers to, *Hesperus*, $\mathbf{v}_1$; yes
in **s**: at **h**: refers to, *Phosphorus*, $\mathbf{v}_2$; yes
in **s**; heavenly body, $\mathbf{v}_1$; yes
in **s**: heavenly body, $\mathbf{v}_2$; yes.

Let $f$ anchor **h** to the ancients' here and now, ancient Mesopotamia, say, and both concepts of Venus to Venus. This is a way of believing that supports the attributions made by Soames with (1) and (2).

**B:** Unfortunately, with the treatment we gave in the book, it also supports his attribution (3), since every situation $s$ of type $[\mathbf{s} \mid S(\mathbf{s})[f]]$, where the objects have replaced the indeterminates, is also one where the embedded sentence (c) in (3), with Soames' connections, is true.

**P:** Common sense says that what *should* be required to support (3) is that a frame of mind $S'(\mathbf{s}, \mathbf{h}, \mathbf{v})$ with just one concept of Venus, something roughly like

in **s**, at **h**: refers to, *Hesperus*, **v**; yes
in **s**, at **h**: refers to, *Phosphorus*, **v**; yes
in **s**, at **h**: heavenly body, **v**; yes

and an anchor $f'$ that anchors **h** to ancient Mesopotamia and **v** to Venus.

**B:** Notice that $S[f] = S'[f']$, which is what gets us in trouble in FOM and FOM′ semantics. Of course things are no better with RTS semantics.

**I:** *I know the answer, but let me ask, anyway, just to be a good interviewer. Why don't you require that the frame of mind have the same meaning as the embedded sentence in the attitude report? S and S′ obviously have different meanings.*

**P:** The meaning of Soames' embedded sentence (2b) is classified with a type $E$:

in **s**: at **p**: refers to, "Hesperus", **v**'; yes
in **s**: at **P**: refers to, "Phosphorus", **v**''; yes

where **v**', **v**'' are complex indeterminates for the roles of being the speaker's referent of the two different uses of the word name *Hesperus*, and **p** is a complex location indeterminate, i.e., a role, that can only be anchored to so that $\mathbf{p} < \mathbf{h}$, i.e., to locations that precede that of the utterance.

The interpretation of the embedded sentence comes from anchoring $E$ with the anchor $g$ defined by

$g(\mathbf{h})$ = here and now,
$g(\mathbf{p})$ = ancient Mesopotamia,
$g(\mathbf{v}') = g(\mathbf{v}'')$ = Venus.

$E$ has the same interpretation, relative to $g$, that $S$ has relative to $f$, but not the same meaning, because of the difference of tense.

**B:** While we are at it, we might just as well do the same for the embedded sentence (3c). The main difference between (2b) and (3c) is the use of the indefinite description and its dependent pronoun, which forces it, at the level of meaning, to denote whatever the indefinite description denotes. Thus, the meaning of (3c) is classified with a type $E'$:

in **s**: at **p**: co-instantiated, [**x**], [**v**]; yes

where [**x**] is the type determined by the condition:

in **s**: at **p**: heavenly body, **x**; yes

and [**v**] is the type determined by the condition:

in **s**: at **p**: refers to, "Hesperus", **v**; yes
in **s**: at **p**: refers to, "Phosphorus", **v**; yes

with **p** as before. The interpretation of the embedded sentence comes from an anchor $g'$ not defined on the types **x** and **v**, since they are within the scope of the quantifier, but with $g'(\mathbf{h})$ = here and now, $g'(\mathbf{p})$ = ancient Mesopotamia.

**I:** *Can you pinpoint the problem and how you plan to escape it? I gather, from all you have said, that you are going to take his suggestion and go back to propositions.*

**B:** Not so fast. We aren't willing to give up that easily. I think that in the book we were (and that Soames is still) misled by intuitions garnered from first-order logic. Once we make the move to promiscuous realism and take a realistic attitude toward things like propositions, but also toward

relations between types of things, there is a straightforward solution staring us in the face.

**I:** *Are you saying, then, that you can, after all, block the move from* (2) *to* (3) *while staying with an account where the interpretation of an utterance is taken to be the type of situation that supports its truth*?

**B:** Yes, that's why I snuck in that description in the meaning (3c) just now. Look, the interpretation of that, $E'[g']$, isn't $S[f]$ at all. It's got some real stuff about there being something that is simultaneously of two different types of things.

**I:** *Wait just one minute, here. Your're the ones who said that* $S'$ *with* $f'$ *should support the truth of* (3), *and that* $S[f] = S'[f']$, *not me. Now you are saying that* $S'$ *and* $f'$ *won't do*?

**P:** I fear we were stringing you along, to show what is wrong with our account as given in the book. Besides, we said something *roughly* like $S'$, $f'$ would support (3). What one really needs is $S''$, with $f''$, where $S''$ is given by:

in **s**: at **h**: co-instantiates, [**x**], [**v**]; yes

where [**x**] is the type of object given by the condition:

in **s**: heavenly body, **x**; yes,

and [**v**] is the type of object given by the condition:

in **s**: at **p**: refers to, "Hesperus", **v**; yes
in **s**: at **p**: refers to, "Phosphorus", **v**; yes,

and $f''$ is given by the obvious anchor.

**B:** In talking about the attitudes, you just have to keep both internal and external significance in mind, to see what is happening with the puzzles. The beauty of our approach, at least in our eyes, is that it allows you to do this. Look at what we have, here, three frames of mind, with three anchors: $S$, $f$; $S'$, $f'$; and $S''$; $f''$. As you move along, you can keep exact track of what is going on in the puzzle. The frames of mind characterized by $S$ and $S'$ do not have at all the same internal significance: one has two concepts of Venus; one has one. But they have exactly the same external significance, given the anchors $f$ and $f'$. On the other hand, the second pair $S'$ and $S''$ give us almost the same frame of mind, but not at all the same external significance. There is clearly no way to get from $S'$, $S''$ and the anchor $f''$ for $S''$ to the anchor $f'$ for $S'$.

**I:** *Let me see if I have this straight. You are saying that while the frames of mind characterized by* $S'$ *and* $S''$ *are more or less internally equivalent, it is* $S''$, *with an anchor* $f''$, *that is needed to support* (3). *Does that mean that the*

*interpretation* (3*c*) *is not a strong consequence of the interpretation of* (2*b*)?
**P:** That's also true. Neither strongly implies the other.
**I:** *Why do you prefer this to Soames suggestion of using propositions? I take it from what you have said earlier, that you are not opposed to propositions on principle. You admitted that they are clearly part of the way people understand the world.*
**B:** Yes, but there are two problems with Soames' suggestion. First, admitting that there are such things as propositions doesn't seem to help without knowing a lot more about the structure of propositions, and about how that structure is related to the statements that express them. After all, since a single statement can both describe a type of situation and express a proposition, then there had better be a close relationship between the two.

## TRUTH AND PROPOSITIONS

**I:** *Well, even if you have gotten away without propositions in this case, I think there is another argument for them and that you have laid the seeds for its growth in your treatment of truth for non-persistent statements.*
**P:** Well, Soames may well be right, that propositions are the right way to treat the semantic value of embedded sentences in attitude reports. I sort of suspect he is. In a different paper, Landman had pretty convincing arguments that the complementizer *that* makes a real semantic contribution to such a report. The contribution may well be that of getting one from a type of situation to a proposition. If so, perhaps our analysis will help us get a handle on what the structure of those propositions is. But you think you have an argument?
**I:** *Yes, but it involves more complex reports. It seems to me that it is going to be hard to tease out the difference between having propositions and having types of situations for the semantic value of embedded sentences where there is an easy way to get from one to the other, as there is when the interpretation is persistent. You can always get from a type S situation to the proposition P that the type is realized by some real situation or other.*

*But look at a case where you needed to bring in reference to a real situation to define truth, in the book. Embed that in an attitude and it should give a case where you cannot get directly from the type to the proposition.*
**B:** Oh, gosh. I see. Take your belief that no one, here in the office, is sleeping (John, wake up!). Suppose I report this with:

"The interviewer believes that no one is sleeping."

The point we made in the book was that the truth of a non-persistent statement requires the statement must be made about some situation. The

point you are making is that this belief also involves, in a crucial way, the real situation we are in and that it is not true just in case there is some real situation where no one is sleeping. Very pretty.

**I:** *Thanks. You know, I was surprised that none of the commentators even commented on this very radical departure in your theory of truth for non-persistent statements. I expected a hue and cry. Do you think they bought it, or that they didn't quite notice?*

**B:** No, I'm sure they understood it and realized it was right. After all, it is not new. We got it from Austin.

**P:** You know, now that you mention this problem, I see that one thing we are missing in our analysis of Soames' derivation is that the type of situation that we take to characterize the ancients' belief is one they take the actual situation they are in to be. That is, there was an actual state of affairs *s* regarding what names referred to, what heavenly bodies back there in ancient Mesopotamia, and really what they believed was that *s* was of a certain type, and, as you point out, that is just the sort of thing we have seen would give us a proposition.

**I:** *If you end up going that route, in the long run, is it semantically innocent? It looks to me like you would have a different interpretation for embedded sentences than for unembedded? It almost smacks of sense and reference.*

**P:** The point of the slogan "semantic innocence," which we borrowed from Davidson, is supposed to be to argue for respecting common sense intuitions about language. It does not seem to violate that to admit that a statement can simultaneously describe a type of situation and express a proposition, as long as we can see the relationship between the type of situation and the proposition. But I think the slogan has outlived its usefulness and begin to regret we ever used it.

**B:** The relation between the situation a statement describes and the proposition the statement expresses does not seem to be at all like the relation between sense and reference. Types and propositions, in this view, are both real, and not in different realms.

**I:** *It seems to me if you went to propositions, you could give a much simpler treatment of the attitudes, even simpler than the Chapter* 9 *account, and that you could use the relation between propositions and types of situations to get at the insights in Chapter* 10, *as well.*

**P:** No, not really. We could give a version of the Chapter 9 semantics, using either types of situations or propositions, that was as good as the one we gave, and much simpler. But remember, Jon said there were two problems with the move to propositions. The other is that it only gives you

a thin semantics, since propositions are definitely not efficient. To get what we consider an adequate account, one where you can attach semantics to statements of folk-psychology principles, not just to simple attitude reports, you would still have to tie up the structure of propositions with efficient mental states, that is, with what we are calling frames of mind. That's why we still resist calling the attitudes just propositional attitudes. There's a whole lot more to them.

## CONCLUSION

**I:** *Well, you two seem to have shifted ground quite a bit from when the book was written, let alone from the early days of the theory, but you don't seem to have recanted. In fact, you seem just as firmly convinced as ever that you are on the track of something important. Are you sure you are not just being stubborn?*

**B:** No, I don't think so. We have learned a lot during the year and a half since the book was written. What we've learned, both from our colleagues here at CSLI and elsewhere and certainly from thinking about what Soames and our other commentators had to say, has shaken some of our attitudes and caused us to shift ground in various ways, so I don't think you can call us stubborn. But, yes, we do feel situation semantics is on the track of something important. All the signs are there.

**P:** As we said in the book, situation semantics is not this or that particular idea, but the search for a naturalistic theory of meaning that can include language use, using the ideas of efficiency and information. It seems terribly important to have a sensible view about meaning and the structure of reality, and no one has convinced us that we are on a wild goose chase with our approach.

**B:** We agree with Dretske that we have left the most interesting questions open, but at least we have helped set the stage for them to be asked, and perhaps answered, by someone.

**I:** *One last question. How is the new book,* Situation Semantics, *coming along?*

*Wait.*

*What are you doing?*

*Get back. Hey . . . HELP?*

Hearing shouts of 'help,' a number of people rushed into Barwise's office, only to find Barwise and Perry at each other's throats.

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