

Chapter 1

Reflections of a Formal Semanticist

1.1 A Personal History within the Development of Formal Semantics

What follows is a personal view, as much about my own history in the field and my perceptions as the development of the field itself. I have written on the history of the field elsewhere (Partee 1988, 1996, 1997); here I focus more on the human side of the story. Within this section, the earlier periods (being more “historical”) get more than their share of the space. Section 1.2 contains general reflections. An Appendix contains some of my best-known example sentences; they form a mini-history of their own.

1.1.1 “Preparation”

My early history is a mixture of lucky timing and of following my interests without any particular plan, ending up ideally prepared for a field that didn’t exist as I was “preparing.”

As an undergraduate at Swarthmore College from 1957 to 1961, I went into Honors, where you were supposed to have a major and two related minors. I wanted to do three apparently unrelated subjects – math, Russian, and philosophy – they were just the subjects I loved best. Mr Brinkmann, head of the Math Department, thought it might be possible to invent a connection. He’d heard of a new field called “mathematical linguistics” or “machine translation.” He noted that computers use logic (part of my philosophy minor) and math; the Cold War was increasing the demand for Russian translation. So if I could write up some such story, my program would probably be approved.¹ I did, and it was.

¹ I think I benefited in a way from the unconscious sexism of the times. No one considered it important that I plan for any particular future; I was free to study the things I loved most.

I then began to notice the word “linguistics” more frequently and to wonder what it really was. By serendipity,² I found my way into a linguistics seminar offered at Penn in the summer before my senior year, for students in mathematics, philosophy, or psychology – just perfect. David Lewis, Gil Harman and I all got our introduction to linguistics in that seminar. It was taught by Henry Hiz with guest lectures by Zellig Harris. It was an introduction to Harris’s transformational grammar, and it was fun. We learned that Harris’s student Chomsky was about to start a graduate program in linguistics at MIT. So since that sort of linguistics seemed to be great fun, I applied to those two schools.

I chose MIT over Penn for a rather timid reason. I learned from Carlota Smith, who was a graduate student at Penn visiting MIT in 1960–1, that the MIT program would be rather structured, whereas Penn left students free to design their own program. Since I knew nothing about linguistics, I thought I’d better go into a program where they would tell me what to do. So I went to MIT.

At MIT, syntax was the center of the universe, and I loved it. Students were immediately part of the team discovering and inventing all kinds of new stuff. We felt like pioneers, we wrote much more than we read, and it was very heady. Chomsky was inspiring, Halle was a great mentor, and Chomsky and Halle were both devoted teachers. I loved both Chomsky’s and Klima’s syntax courses, and noticed an interesting difference between them: Chomsky was interested in English syntax only as evidence for theory, and Klima was interested in theory for its help with puzzles in English syntax. I think I felt more Klima-like, but it was implicit that one wanted Chomsky as dissertation director, since it was theory that was interesting with a capital “I.”

What little semantics there was around MIT then was peripheral, mainly the developing work of Katz, Fodor, and Postal. Katz and Fodor, junior faculty at MIT, made the first proposals for semantic theory in generative grammar. They were clearly concerned with compositionality – what they called the Projection Problem. Because of negation and questions, Katz and Fodor (1963) computed meaning on the basis of “T-markers” representing the transformational history of an expression, analogous to Montague’s derivation trees (Bach 1979). But their semantics was primitive, just bundles of “semantic features” and no attention to things like quantifiers. They aimed to capture ambiguity, synonymy, and semantic anomaly, which could all be expressed in terms of “readings” – how many, and same or different. There was no mention of truth conditions or entailment relations. There were thoughtful ideas about compositionality in their work, but the representation of meanings as bundles of features was clearly inadequate. But on my own I had no idea what a semantic theory should look like.

² My uncle, an MIT alumnus, heard of my interest in “mathematical linguistics,” and sent me the program of the MIT conference (published as AMS 1961). I wrote to almost everyone on the program, asking what I could do in the summer to help me find out what linguistics was. A surprising number wrote back. Several suggested I contact Penn, which worked out wonderfully. The kindness of established linguists in replying to a naive young student made a big difference to me.

During my third year came the Katz Postal hypothesis (Katz and Postal 1964), that transformations are meaning-preserving and meaning can be determined from Deep Structure. They put Neg and Q morphemes into Deep Structure to trigger the Negation and Question transformations, arguing that there was independent syntactic motivation. Their claim led to important debates. The architecture of the theory, with syntax mediating between semantics on one end and phonology on the other, was elegant (and laid some of the groundwork for Generative Semantics).

Chomsky's thinking was evolving from *Syntactic Structures* (Chomsky 1957) to *Aspects* (Chomsky 1965) while I was there, and despite his deep skepticism about semantics, he tentatively accepted Katz and Postal's suggestion. During the brief *Aspects* period there was rosy optimism that the form of the theory, including the Katz and Postal hypothesis, was stable. The idea that meaning was determined at this "deep" level probably contributed to the aura surrounding the notion of "language as a window on the mind." But it fell apart when quantifiers were noticed. The properties of quantificational NPs (see section 1.1.2) created conflicts between syntax and semantics, kicking us out of our Garden of Eden and into the "linguistic wars."

The papers I wrote in graduate school were on semantic issues, but without any semantics. I wrote papers on (the syntax of) quantifiers, and on negation. I did take Yngve's machine translation course in my first year, and wrote a computer program to translate a small subpart of first order logic into English. In the seminar where we tried out potential dissertation topics, I worked on the *some/any* alternation, worrying about the optional meaning-changing *some-to-any* rule in Klima (1964), a violation of Katz and Postal's hypothesis. But I couldn't solve it without three *some*'s. So I abandoned that topic, which was just as well: it really wasn't a syntax problem, as Ladusaw later showed (Ladusaw 1979).

Later, by accident, I found a completely different dissertation topic, namely the syntax of subjects and objects and transformations that affected what we would now call argument structure. (The term "argument" came into syntax only later, via formal semantics.) Actually, my dissertation proposal, which Chomsky approved with enthusiasm, was for a transformational grammar of English, synthesizing what had been done up until then. But that was too much for one year, and luckily I had never heard of not finishing in a year – I thought it was a kind of one-year take-home exam.³

³ I also didn't know you could discuss your work with your dissertation committee (so I didn't until I was told to). There was a lot we didn't know, being the first class in a new program. Even the faculty were making up rules as we went along: there was a *de facto* merger of the first class (Tom Bever, Bruce Fraser, Terry Langendoen, Ted Lightner, Jim McCawley, me, and one student who dropped out) with the second class (Jim Foley, Samir Ghosh, Jeff Gruber, Paul Kiparsky, Yuki Kuroda, Brandon Qualls, Stan Petrick, Peter Rosenbaum, Sandy Schane, Arnold Zwicky) after it was decided just before our third year that qualifying exams should be in the second year.

Although the center of my life at MIT was syntax, other things I learned during those years helped prepare me for my (unimagined) future in semantics. At MIT, I minored in math, with a course in Automata Theory with E. F. Moore, and a wonderful course in logic and recursive function theory with Hartley Rogers, Jr. I had a philosophy of language course with Hilary Putnam, but not on his later interesting work: we read a book by Paul Ziff. Starting in my second year, I taught a course in mathematics for linguists each fall (which led to Partee 1979a, and my part of Partee et al. 1990). To learn more set theory, I took a course with Quine at Harvard built around a book in progress (Quine 1963); the demanding logic homework probably made Montague's work a little less intimidating. Michael Rabin's model theory course was over my head, but added some background that stood me in good stead later.

I went to UCLA in 1965, and by 1966, Stockwell, Schachter and I were engaged in our English syntax project (1966–8),⁴ with a team of graduate students, which led to our book (Stockwell et al. 1973). We limited our attention to work in transformational syntax up to 1966, with few exceptions (mainly Fillmore's case grammar and Chomsky's lexicalism (Chomsky 1970), which we combined), and stayed out of semantics. That meant keeping away from the generative semantics–interpretive semantics debates, which was hard, but necessary. Each chapter had a faculty-led team; mine were “Determiners,” “Pronominalization,” and “Negation.” With weekly team presentations and group discussion we were able to keep the grammar consistent. And because it was like a seminar with “homework,” I was able to write my share of our 840-page book, although in general I'm too much of a procrastinator for book-sized projects.

But although the bulk of my output⁵ during that period went into that project, I had one eye on the syntax–semantics fights. Lakoff and Ross had started to work together while I was writing my dissertation; their joint paper “Is deep structure necessary?” was written in 1967. My dissertation argued against Lakoff's analysis of transitive *open* as a causative. My classmate Jim McCawley was publishing generative semantics papers by at least 1968. Postal, a staunch Chomskian when I was at MIT, joined the generative semantics movement shortly afterwards. Jackendoff was a student at MIT right after I was, and he and Chomsky launched the counteroffensive of “interpretive semantics.”

My instincts were mixed: the generative semanticists took semantics seriously and uncovered interesting generalizations. But their “abstract syntax” seemed wild (it might not now!). My first published paper (Partee 1970a), centered on example (A1),⁶ was in reaction to Lakoff; Lakoff responded with

⁴ That fulfilled my dissertation dream, which Stockwell and Schachter shared.

⁵ I mean my academic output. A considerable amount of energy also went into “life” issues – my marriage to Morriss Partee, and the birth of our three sons, Morriss, David, and Joel. When we got married, in 1966, I changed my name from Barbara C. Hall to Barbara Hall Partee. After our divorce in 1971, I kept that name in part because of my three Partee sons, and in part because of my publications; only my dissertation and one review were under the name Hall.

⁶ All examples with numbers beginning with A or B are found in the Appendix.

“Repartee” (Lakoff 1970). Soon afterward (Partee 1971), I analyzed the central issues I saw behind the debate, identifying the main problems on each side. I didn’t reach a definite conclusion, and I thought that was a failure. In retrospect I think I helped clarify the issues, and over time I came to appreciate that impartial analysis was probably one of my worthwhile strengths.

Linguists were then beginning to discover that there was interesting relevant work in philosophy of language. I was struck by Donnellan’s work on referential and attributive definite descriptions (Donnellan 1966), and attempted to apply it to the problem of specific and non-specific (and non-referential) indefinites in Partee (1970b, reprinted here, chapter 2), with some good examples, including (A2), but an unsatisfactory analysis.⁷

1.1.2 Montague and the beginnings of Montague grammar

One day David Lewis told me that Montague, whom I knew only by sight, was starting to apply his work in logic to the semantics of natural language. David and I had known each other since Swarthmore, and he knew me well enough to suggest that I would find Montague’s work interesting. I first sat in on one of Montague’s seminars in 1968, along with David and Frank Heny. David helped us decipher the logic.

Montague’s approach was different from anything I had been exposed to. But with help (I had never seen a lambda before, nor did I know about intensions and extensions), I could follow well enough to begin to appreciate it.

Two aspects of Montague’s approach looked especially exciting. The first was the revolutionary (to a linguist) idea that the core data were the truth conditions of sentences. Suddenly there was a non-subjective criterion of “observational adequacy” for semantics.

The second exciting aspect of Montague’s approach was that his powerful logic could do some real work, which in turn could help keep the syntax clean and elegant. Semantic equivalence would then not require some deep (or any other) structure. The use of type theory let Montague interpret basic grammatical relations as function-argument application. In retrospect we can see that generative semanticists were hampered by the mismatch between natural language structure and the structure of first-order logic, the only logic most linguists knew. That’s why lambdas, types, and the example of generalized quantifiers have such prominence in formal semantics.⁸ Without them,

⁷ I reported in a footnote a crucial counterargument from Lauri Karttunen. I’m glad that I didn’t just withdraw the paper, and instead published it with the footnote; otherwise it would be in my list of regrets.

⁸ My Dutch colleagues still recall a sentence I uttered in my 1980 talk in Amsterdam “The first decade of Montague grammar”: “Lambdas changed my life.” Theo Janssen, who cited the quotation in Janssen (1994), says (personal communication) that “this quote is the best summary of Montague’s revolution: it is the feature that made compositionality possible at all.”

there was no way to assign a semantic type to the syntactic category NP. This was undoubtedly responsible for some of the “wilder” syntactic analyses by generative semanticists.

The uniform interpretation of all NPs as generalized quantifiers was one of the most exciting specifically linguistic innovations in Montague’s work, first introduced in Montague (1970; hereafter UG), but best known from Montague (1973; hereafter PTQ). David Lewis (1970) also treated NPs as generalized quantifiers. There may be no way of ever knowing which of them had priority on this idea. But it was through Montague’s work, with his rich and systematic framework, that the idea caught the imagination of linguists and became part of the foundation of formal semantics.

With the help of philosophers (especially David Lewis and David Kaplan), I began to learn, with some shock, that “West Coast” philosophers (especially Tarski and Carnap) had been developing possible worlds semantics and higher-order intensional logics while East Coast philosophers (especially Quine) were insisting that only first-order extensional logic was real logic. I hadn’t understood that philosophers or logicians could be dogmatic or differ so strongly about what their fields were about. And this accident of geography made a huge difference to the field, as it had to my education.

At the fall 1970 conference at which Montague presented PTQ, I put all of my “discussant” efforts into describing key differences between Montague’s syntax and transformational grammar (Partee 1973c). I couldn’t yet say much about the semantics, but could show how it motivated Montague’s syntax. That was my only presentation about Montague’s work with Montague present, just months before he was killed in the spring of 1971. I was glad to have his assurance that I was not misrepresenting him.

I learned about Montague’s shocking death from the newspaper over breakfast the morning after. It was one of those world-stopping moments like the deaths of President Kennedy and Martin Luther King, at least for my world.

Just at that time, I was writing a UCLA grant proposal for a project in which I would exploit Montague’s appreciation of “puzzles” to try to provoke him to work on some constructions which seemed to require the transformational cycle. *There* insertion, *Passive*, and *Raising* together would generate sentences like (1), which I couldn’t see how to derive compositionally.

(1) *There* was believed to be a unicorn⁹ in the garden.

I never had a chance to ask Montague about such sentences, though there is a hint in PTQ that he might not object to the inclusion of meaning-preserving transformations. Michael Bennett, who had been working with Montague, and

⁹ The unicorn is the “mascot” of Montague Grammar because of one of Montague’s key examples, *John seeks a unicorn*. Bob Rodman put a unicorn on the cover of the UCLA volume (Rodman 1972); I did the same with Partee (1976), and unicorn T-shirts proliferated at Montague Grammar workshops. Clever unicorn-pictures head the chapters of Janssen (1983).

whose dissertation (Bennett 1974) David Kaplan and I co-chaired, included a fragment devoted to such constructions.

After Montague's death, the exploration of his ideas became a cooperative group effort, first mainly at UCLA, and then more broadly. I taught a one-quarter seminar on Montague grammar in fall 1971 at UCLA. Then, because we were all just beginning to get the hang of it and didn't want to stop, we continued¹⁶ for a second quarter and all wrote papers, collected in Rodman (1972). I taught and lectured on Montague grammar (MG) a great deal in those first years, and much of my own research was focused on how to combine Montague grammar and transformational grammar. My basic one-hour talk became Partee (1973b), and my evolving course lecture notes became Partee (1975b), which served as a first quasi-textbook. It was replaced in that function by Dowty (1978) and then by Dowty et al. (1981).

Here is an example of an obstacle confronting the integration of Montague grammar into linguistics, whose solution felt like a "light bulb" idea. It was during the Philosophy-Linguistics Institute at Irvine in the summer of 1971 that I finally put my finger on the problem: what to do about deletion rules, such as the rule of "Equi-NP Deletion" by which (2a) was derived from a structure like (2b).

- (2)a. Mary wanted to win.
 b. [_S Mary wanted [_S Mary win]]

In Montague grammar, there is nothing like "deleting" part of the meaning of a constituent. When I finally understood the problem, I had an idea for its solution, finally appreciating my philosopher friends' advice to "use lambdas." (I knew I didn't want them in the syntax, and it took me that long to catch on to how to use them to semantically interpret "syntactic deletion.") The key to the solution is intimately related to the way in which quantifiers destroyed the *Aspects* theory with its incorporation of the elegant Katz-Postal hypothesis.

For years, nobody had thought about the consequences of the analysis in (2) for a sentence like (3a), whose deep structure should accordingly be something like (3b).

- (3)a. Everyone wanted to win.
 b. [_S everyone wanted [_S everyone win]]

The semantic problem is apparent. In Montague grammar, where infinitives were treated as VP type, I could see that the embedded sentence's subject

¹⁶ When Stockwell, my chairman at UCLA, gave me permission to replace my scheduled seminar in mathematical linguistics with a second quarter of Montague grammar, he said, "This is all very interesting, Barbara, but when are you going to get back to doing linguistics?" That question fired me up and gave me my mission for the next half dozen years, to convince him and the world that this *was*, or should be, part of linguistics.

should be a variable that gets bound by lambda abstraction (see the Derived VP Rule of Partee 1973b). In Chomskian syntax, a corresponding change was eventually made, replacing the identical NP by the null element PRO.

While “Montague grammar” was undoubtedly the principal vehicle by which the influence of model-theoretic semantics came into linguistics, there were other related lines of research. David Lewis had some influence on Montague’s work, and did important early work of his own (Lewis 1969, 1970). Cresswell (1973) had a great deal of valuable discussion of foundational issues and many specific grammatical constructions. Parsons, Keenan, and Thomason and Stalnaker were also early and active contributors to linguistics–logic–philosophy exchanges (Keenan 1971; Parsons 1972; Thomason and Stalnaker 1973).

Interaction with philosophers was very important in the 1960s and 1970s. Linguists and philosophers operated by different principles, but one could see mutual respect and learning in conferences such as the 1969 conference that was the starting point for Davidson and Harman (1972), and in the 1971 Irvine summer institute. Philosophers had thought a great deal about reference, quantification, indexicality, intensionality, the semantics of interrogatives, and other semantic issues then new to linguists. Linguists generally knew more about syntactic structure and syntactic constraints on possible interpretations, and were good at generating examples that could challenge any suggested generalization.¹¹

That 1971 Irvine summer institute was a memorable and influential event for me, not only because I figured out how to use lambdas to model “syntactic deletion,” but for the immersion in philosophy of language. There were two three week sessions, each with three philosophers and one linguist; the “students” were young philosophy professors, including Thomason, Stalnaker, Gareth Evans, and many others. I was the linguist for the first session, and attended the lectures by Davidson, Harman, and Grice. I commuted to the second session to attend the lectures by Strawson, Kaplan, Quine, and Haj Ross, plus Kripke’s “Naming and Necessity” lectures. The lectures and discussions were a big part of my philosophical education, and the intense interactions led to a lasting camaraderie, which continued at our 1974 Linguistic Institute at UMass.

1.1.3 The first decade of Montague grammar

I moved to UMass Amherst in 1972, with appointments in Linguistics (my primary home) and Philosophy, and Terry Parsons moved there at the same

¹¹ In my workshop at the 1974 Linguistic Institute, several philosophers offered theories of the distribution of *any*, and we linguists sat there popping off with counterexamples. None of us had a good theory of *any* (this was before Ladusaw’s dissertation), but we knew all the crucial examples.

time, in Philosophy. Emmon Bach came in 1973,¹² and soon started doing Montague grammar as well. Sometimes with National Science Foundation (NSF) grant support and sometimes without, Terry and Ed Gettier and Emmon and I and some graduate students¹³ had frequent meetings to discuss Montague's papers and our own work, and Emmon and Terry and I taught joint linguistics and philosophy seminars in various combinations.

The UMass department was very young (departmental status approved in 1971), and a big event that helped put it on the map was the 1974 Linguistic Institute, in which semantics and philosophy of language, the areas I organized, were major components. I had a big Montague grammar course, and a research workshop whose participants, most also Institute faculty, included Emmon, Terry, David Lewis, Janet Fodor, Thomason, Stalnaker, Jackendoff, Keenan, Kamp, Karttunen, Anil Gupta, and Robin Cooper.

By the mid-1970s, UMass was definitely "on the map" and widely regarded as the center of formal semantics,¹⁴ and we made major contributions to developing versions of Montague grammar (Cooper and Parsons 1976; Bach 1979; Partee 1979b). By then, Montague grammar and related work was flourishing as a linguistics and philosophy enterprise also in other parts of the US, in the Netherlands, Germany, Scandinavia, and New Zealand, and among individuals elsewhere. (By the late 1970s it was no longer possible to keep track.) There were many milestones in the second half of the 1970s: the first published Montague grammar collection (Partee 1976), the first issue of *Linguistics and Philosophy* in 1977, the starting of the biennial Amsterdam Colloquia.

There were interesting foundational issues to worry about. Montague grammar was having great successes, but there were principled objections from two sides: from linguists for whom truth-conditions seemed at odds with grammar being in the head of the native speaker, and from philosophers who found Montague's possible worlds semantics insufficiently intensional. As the field of cognitive science got going in the late 1970s, helped by the Sloan Foundation, such issues had a forum. I worried about them in quite a number of papers (for instance, Partee 1979c, 1980, 1988) but mainly "alongside" the actual "doing" of semantics, except for work on the semantics of propositional attitudes, where foundational issues are unavoidable. Those were not among my most successful papers, and none are included here. I still think these issues are important (see, for instance, Stalnaker 1984), but I haven't worked on them recently.

¹² Emmon and I were married from 1973 to 1996, and were a bit like "Mom and Pop" of the department.

¹³ Dissertations from that period include Robin Cooper, Muffy Siegel, Greg Carlson and Paul Hirschbühler in Linguistics, and Dave Davis and James Waldo in Philosophy.

¹⁴ One indicator was that when Irene Heim received a fellowship to study anywhere in the US, she wrote to Chomsky that she was interested in pursuing syntax and semantics at MIT. He replied that if she seriously wanted to do semantics, she should consider UMass instead. She entered our PhD program in 1977. A later landmark, significant for me given Chomsky's long-standing skepticism about formal semantics, was when MIT hired Irene as their first formal semanticist in linguistics in 1989.

Formal semantics was still mainly “Montague grammar” in the 1970s, and mainly an optional “seminar” topic. At UMass, for instance, all of semantics was still optional in the 1970s. In the late 1980s, after the hiring of Angelika Kratzer gave us three semanticists, we increased the first-year program to 12 credits, and made the first semantics course (by then formal semantics) a required first semester course. By the late 1990s syntax, semantics, and phonology were very nearly co-equal core areas of the curriculum.

1.1.4 From interdisciplinary to disciplinary in the 1980s

My work in the 1980s combined investigations within Montague grammar with interdisciplinary explorations in the emerging field of cognitive science, involving philosophers, computer scientists, and psychologists. I think the height of interaction between linguists and philosophers had passed by 1980, followed by the rise of cognitive science, within which semantics thrived as an inherently interdisciplinary concern, and then by a greater specialization of semantics inside of linguistics proper.

The Sloan grants in Cognitive Science from 1978 through the 1980s were the occasion for many interdisciplinary conferences. I organized one on “Indefinite Reference,” where Irene Heim got her dissertation topic (Heim 1982), and one showcasing the new Discourse Representation Theory (DRT–Kamp 1981) and Situation Semantics (Barwise and Perry 1981). I thought then that Kamp and Barwise might be starting to play Montague-like roles, bringing their logical expertise to bear on the formal structure of semantic theory.

Kamp’s DRT and Heim’s similar File Change Semantics were a big event in the early 1980s; his work and hers have diverged since then, both influential within somewhat different communities. Barwise and Perry’s later work (Barwise and Perry 1983) was disappointing. Angelika Kratzer’s very different situation theory (Kratzer 1989) has much more influence.

Godehard Link’s exciting work on plurals and mass nouns (Link 1983), with his algebraic structuring of the entity domain, was an important new development in the 1980s and influenced developments in event semantics as well.

The period of the Sloan grants in Cognitive Science should have been as wonderful as the earlier linguistics philosophy interactions. But somehow it wasn’t quite as friendly or quite as productive, at least from my perspective. I can think of two possible reasons. One was the competition over domination of cognitive science, the other the competition for large grants. Although we all participated cooperatively in the many interesting conferences, and enjoyed having postdocs and even new hires¹⁵ with Sloan money, the cooperation was tinged with competition, since each of the three successive

¹⁵ Our own psycholinguist Lyn Frazier was hired in a position that wouldn’t have been created without the incentive of a Sloan grant.

rounds of grant competition was for a smaller number of larger grants. And perhaps most divisive was the competition for the leadership of cognitive science between the AI-centered community and the theoretical linguistics-centered community. I still care about foundational issues in cognitive science such as finite representability and mental models, but I work on topics for which I have a good environment, and leave other appealing topics for some other lifetime.

One quiet but important figure for me in the 1980s was Ray Turner, a theoretical computer scientist and philosopher interested in applied logic and semantics. When he came to UMass on a Sloan postdoc in 1981–2, we organized a weekly interdisciplinary Model Theory seminar. A grant from the System Development Foundation (SDF) (1984–7) to work on Formal Foundations of Semantics supported his joint work with Gennaro Chierchia on property theory (Chierchia and Turner 1988), a postdoc for Fred Landman, and our conference on property theory and type theory (Chierchia et al. 1989).

Emmon and I spent 1982–3 at the Max Planck Institute for Psycholinguistics in Nijmegen, attending semantics seminars in Groningen or Amsterdam almost every week. Formal semantics was and is a vibrant enterprise in the Netherlands. The biennial Amsterdam Colloquium was my forum of choice for presenting new research through the 1980s.

In the meantime, there were important developments within linguistics proper. While some of us were still trying to integrate Montague grammar with transformational syntax, others realized that a powerful semantics might allow a more radically constrained syntax, with no transformations at all. Several developments in this direction began at the end of the 1970s and became major research enterprises in the 1980s: especially Generalized Phrase Structure Grammar (GPSG) (Gazdar et al. 1985), categorial grammar (Bach 1984; Bach et al. 1987; van Benthem 1987), and later Head-driven Phrase Structure Grammar (HPSG) (Pollard and Sag 1994). I have always felt very sympathetic to such work; see Partee (1996).

The growth spurt in “straight linguistic semantics” in the 1980s is reflected in the range of work of my dissertation students of that time.¹⁶ Hans Kamp was at UMass briefly in the mid-1980s, and influenced the work of Roberts and Kadmon, as well as having already influenced some of my own work (Partee 1984b; Kamp and Partee 1995). The rethinking of the semantics of NPs in the work of Heim and of Kamp was a major impetus for my work on type-shifting (Partee 1986), which had started with my earlier work on conjunction with Mats Rooth. Type-shifting was a departure from Montague’s strong form of compositionality, with arguments coming largely from linguists. See more notes on this work in section 2 of this chapter.

¹⁶ Elisabet Engdahl, Michael Flynn, Ken Ross, Irene Heim, Gennaro Chierchia, Mats Rooth, Jonathan Mitchell, Craige Roberts, Nirit Kadmon, Jae-Woong Choe, Alessandro Zucchi, Karina Wilkinson. Emmon was involved in all of those as well, and chaired others.

1.1.5 Recent years

By the 1990s, students were not conscious that the core fields hadn't always been "phonology, syntax, semantics." Several semantics textbooks were published around 1990,¹⁷ and more departments came to have at least two formal semanticists (even MIT).

In the 1990s, there was some divergence between formal semantics in the Netherlands and the US, with the Dutch working more on the logical side. In 1990 van Benthem and colleagues founded the European Foundation for Logic, Language, and Information (FOLLI) which sponsors a new journal (*JOLLI*) and annual summer schools (ESSLLI).

In the US, semantics is firmly inside linguistics. After fifteen years in which *Linguistics and Philosophy* was the preeminent journal for formal semantics, Heim and Kratzer launched *Natural Language Semantics* (*NALS*) in 1992. And the US-based conference SALT (Semantics and Linguistic Theory) had its first annual meeting in 1991. Both *NALS* and SALT permit authors to presuppose some syntax and don't require that everything be understandable to philosophers.

And the Bach–Kratzer–Partee NSF project on quantification (1988–92) was my first non-interdisciplinary project since the Stockwell, Schachter, and Partee project. In a foray into semantic typology, we studied determiner quantification and adverbial quantification cross-linguistically.¹⁸

Some semanticists work with Chomskian syntax and others with non-transformational approaches (extended categorial grammar, Lexical-Functional Grammar (LFG), HPSG, or Tree-adjoining Grammar (TAG)). Heim (1982) did much to make semantic sense of a notion of Logical Form in a Chomskian approach. All approaches are represented at conferences like SALT. I tend to declare myself agnostic about syntax, largely in order to co-exist compatibly in my mainly Chomskian department. I greatly value a department like ours in which people can really work together and talk to each other, even if it doesn't involve my own first choice of kind of syntax.

During the 1980s, I developed ties to the Prague school, particularly Eva Hajičová and Petr Sgall. During my fall 1989 semester in Prague, we made a start on connecting our research. With the help of a grant, we continued that work over several years, bringing UMass students to Prague, and Prague colleagues to the US. The UMass students organized a Focus Workshop in 1995, and edited Benedicto et al. (1998). After another sabbatical semester in Prague, we eventually completed a book (Hajičová et al. 1998). In spite of theoretical

¹⁷ The textbook Gamut (1991) is the beginning of an answer to one of my dreams, that lambdas should be introduced in introductions to logic, and not only as an advanced topic, or in semantics courses.

¹⁸ Students in our lively quantification period included several whose dissertations I supervised (Portner, Brennan, Rullmann), and several supervised by Angelika (Berman, Diesing, Schwarzschild, von Stechow).

differences that put certain questions out of bounds, we made some progress on the interaction between quantification and topic-focus structure.

Since a life change in the mid-1990s,¹⁹ I've spent half of every year in Moscow, with a new "mission" of bridging Moscow-school lexical semantics and Western formal semantics (see Partee and Borschev 2001). The Moscow-school lexical semanticists are working to construct a scientific foundation for lexicography, including systematically formulating the "components of meaning" that make up word senses. Vladimir Borschev and I find formal semantics and Moscow-school lexical semantics potentially compatible, each addressing semantic issues neglected by the other.

What's my place in the field now? I remember feeling an initial shock when I was asked to write a "historical article about Montague grammar" for van Benthem and ter Meulen (1997) – my first reaction was "What do you mean, historical?" But then I realized that students rarely study Montague grammar in its original form, just as they no longer study *Syntactic Structures*. And the fact that "Montague grammar" has now made it into the *Oxford English Dictionary* is a sure sign of historical status. So I have come to accept that in some sense, I'm now a historical figure. At the same time, I'm one researcher among many, working on whatever interests me most at the moment, currently the multifaceted problem of the genitive of negation in Russian. When I worked on that problem in graduate school, it was presupposed that a solution would consist of nice explicit syntactic rules. I never dreamed that I would come to see the genitive of negation as involving the interaction of syntax, compositional semantics, lexical semantics and diathesis shift, topic-focus structure, context, presupposition, and some still partly mysterious notion of perspective structure (Borschev and Partee 2002).

The field of formal semantics itself is fully international, heterogeneous but still collegial and friendly. No one figure and no one department is dominant; there are developments of many kinds coming from many places, including computational semantics, psycholinguistics and acquisition. I couldn't list all the journals in which work in formal semantics appears now, nor all the textbooks. And that's great.

1.2 General Reflections

A collegial environment is a wonderful thing. Mike Flynn managed to ask me his "really stupid question" in 1980 about the fact that Edwin Williams and I evidently got along very well and never disputed each other's framework; he wanted to know whether our frameworks were really compatible in some way he couldn't see, or if there was another reason behind our not arguing. Realizing that many students must wonder, I checked with Edwin and found

¹⁹ In 1997, I married Vladimir Borschev, whose background was in mathematical linguistics and the formal semantics of programming languages.

we had the same answer: not wanting to “shoot from the hip” at a theory we didn’t fully understand, but not wanting to put in the effort to study a theory we were skeptical about. So for the benefit of the students and each other we decided to co-teach introductory semantics the following spring, ending with a unit to introduce Montague grammar and Chomskian “Logical Form” and to debate and try to identify some difference in empirical predictions. When we did it, we immediately uncovered misconceptions we had about each other’s theory, and found holes in each theory in places the other theory considered important. When we finally found sentence (4), about which our theories (with the holes filled in on the fly) made opposite predictions, the students split 50-50 about whether control was possible in such a sentence! It was great to have such a spirited non-polemical debate.

(4) On whom can you depend to do the dishes?

Theory diversity is a good thing; it enriches the stock, and it promotes challenging questions. But being able to discuss issues together with some common assumptions is also important. So it’s a challenge to help diverse theories flourish without total fragmentation, within a department or within a field.

Personality diversity is a good thing too, and diversity in intellectual style and intellectual interests. I reassure students that there are many ways to be a good linguist, and no one “great linguist” is a good role model for everyone.

Formalization is an excellent thing in moderation. When there’s too little, claims are fuzzy and argumentation is sloppy. But there can be too much formalization, or premature formalization. So one shouldn’t hesitate to share ideas in an informal state; looking at things from many points of view may help a good formalization emerge.

Compositionality is a powerful working hypothesis, but it is well known that it can’t be an empirical hypothesis all by itself.

Why has Chomsky been so resistant to compositionality? My own speculation – and this is pure speculation – is that it may be related to his deep resistance to any kind of functional explanations in syntax. Semantics may seem too close to general cognitive faculties for comfort,²⁰ so compositionality might seem to weaken the thesis of autonomy of syntax. In fact it doesn’t weaken descriptive autonomy at all: syntax can be described independently of semantics but not vice versa. What it weakens is explanatory autonomy: compositionality makes it natural for the acquisition mechanism to work on syntax and semantics in parallel. But isn’t that reasonable?

It’s sometimes good to write down non-results as well as results. I’ll describe one article I didn’t write that I’ve always regretted. Chomsky attacked compositionality in his 1974 Linguistic Institute Golden Anniversary lecture (Chomsky 1975) with an argument based on sentences like (5). He argued that one has to know whether the “whole sentence” is singular or plural to know whether *have wheels* has a genuinely plural meaning.

²⁰ But I believe formal semantics studies a highly structured language-specific system.

(5) Unicycles have wheels.

In my reply (Partee 1975a) I argued against Chomsky's analysis (see (A7)), but I had no compositional alternative. I spent the following summer looking for one, exploring several approaches but hitting problems with each one. So I didn't write up any paper, but whenever anyone proposed an analysis, I could usually find counterexamples in my notes. So I always regretted not publishing a paper on my attempted solutions and the counterexamples I had found.

You never know in advance what your most valuable contributions will be. I guess I now think my own most valuable contributions have been in two areas.

One is in the synthesis of various diverse ideas. My work synthesizing Montague and generative grammar was my first big effort of that kind. That project has in a sense never ended.

My type-shifting work, which some colleagues regard as some of my best work, was also mostly synthesis. I respected the motivations behind different treatments of NP semantics, particularly Montague's vs. Heim's and Kamp's, and wanted to find a way that they could all be right. Back in Partee (1984a), I had discussed challenges to compositionality from cases where the "meanings of parts" seem not to be autonomous. And Rooth and I argued in our work on conjunction that the meanings of parts may shift in ways that allow composition to work smoothly. I began to see that when one part influences a meaning shift in another part, it isn't a violation of compositionality but something that happens because compositionality is one of several constraints that have to be satisfied.

My current work is also synthesis, of Russian and Western approaches to semantics, and lexical semantics with compositional semantics.

I think my other most valuable contribution has been in teaching, advising, working with students, especially graduate students. I'm very gratified when students say I'm good at making difficult things clear, and that I'm simultaneously demanding and supportive. I am at least as proud of the students I have worked with as of my publications. And I am grateful to them too; they have been as stimulating to work with as anyone could possibly wish for.

I think my strengths in teaching and in research are probably related. It's said that I'm good at understanding where questions are coming from and good at drawing connections. I suppose that's at the heart of my teaching and of my work. I like looking at things from different points of view, finding connections, and finding ways to bring together seemingly incompatible attractive ideas. I have definitely had wonderful environments to work in. I am grateful to colleagues and students in linguistics and philosophy at UCLA and at UMass, as well as in places where I have spent shorter periods. I'm not the kind of person who could have developed ideas in isolation. I'm grateful to all the teachers and students, friends and colleagues and family, who have touched my life and my work and helped to make it such an exciting, rewarding and joyful adventure.

Appendix: Example Sentences

Here is an annotated chronological list of some “Partee examples”.²¹ Following those, to set the record straight, is a “B-list” of examples I didn’t invent which became known through my work or in my variants.

Part I The A-list

(A1) Few rules are both explicit and easy to read.

In: Partee (1970a: 154), also as example (142) in Stockwell et al. (1973: 105).

This example and its non-equivalence to (6a) or (6b) were part of my argument with Lakoff about syntax and semantics.

- (6a) Few rules are explicit and few rules are easy to read.
- b. Few rules are explicit and to read few rules is easy.

The conjunction must be “phrasal conjunction” to get the semantics right, but then *easy to read* must be available as a phrase to be conjoined. Neither a classical transformational derivation nor a generative semantics derivation would provide that. I had no solution then, but it can be solved with the tools Montague provided (Partee 1973b), and with later non-transformational approaches.

(A2) My home was once in Maryland, but now it’s in Los Angeles.

In: Partee (1970b: 369, ex. 37, and in footnote 10, p. 384), reprinted in Partee (1972: 245) and in this volume (chapter 2); also in Stockwell et al. (1973: 202).

This example is related to the well-known example (7).

- (7) The alligator’s tail fell off, but it grew back. (Postal 1967)

The point of my example was to contrast the behavior of *house* and *home* with respect to the kind of identity involved in coreference.

(A3) John is building a house.

In: Bennett and Partee (1972), pp. 13, 15 in 1978 version (chapter 4 in this volume.)

²¹ Many colleagues helped me build a list, remembering examples, helping me figure out whether examples are “mine,” helping track down citations, and giving encouragement with the project. My thanks to Luis Alonso-Ovalle, Enmon Bach, Greg Carlson, Shai Cohen, David Dowty, Elisabet Engdahl, Irene Heim, Paul Hirschbühler, Larry Horn, Theo Janssen, Nirif Kadmon, Angelika Kratzer, Jeff Pelletier, Craige Roberts, Hotze Rullmann, Roger Schwarzschild, Muffy Siegel, Anna Szabolcsi, Rich Thomason, Ede Zimmermann, and Sandro Zucchi. Space limitations required shortening the resulting list. I take full responsibility for inaccuracies, and would like to be informed of them.

I produced this example during discussion with Michael Bennett as he was writing his dissertation. It was a key piece of evidence for interval semantics, first proposed in our paper, reprinted in this volume.

- (A4) The temperature is rising.
The temperature is ninety.
do not together entail: Ninety is rising.**

In: Montague (1974: 267), originally in Montague (1973).

This is sometimes referred to as the “Partee puzzle.” I first gave this example, in the form of a question, to David Lewis. He had stated (Lewis 1970) that intransitive verbs are never intensional with respect to subject position, and I was just learning about intensionality and trying to understand what that meant. So I tried to think what a counterexample should look like, and came up with examples about the temperature or the price of milk rising or changing, and wondered why they didn’t count as intensional. And David told them to Montague, who provided an analysis in PTQ. But Dowty, Wall, and Peters (1981) have arguments against Montague’s solution.

- (A5) I didn’t turn off the stove.**

In: Partee (1973a: 602), reprinted (chapter 3) in this volume.

I used this example to argue against the standard tense-logic treatment of past tense as existential quantification over past times and in favor of considering tenses as pronoun-like. But the argument is not conclusive, because suitable domain restriction on the range of the variables could rescue a narrow-scope existential quantifier analysis, as various authors have pointed out. The debate continues.

- (A6)a. Fred was trying to find the minutes before the meeting began.
b. Fred was looking for the minutes before the meeting began.**

In: Partee (1974: 99, examples 53 and 54).

I used these examples to argue against decomposing *look for* into *try to find*. Sentence (A6a) has a scope ambiguity that (A6b) lacks. (This contrasts with the matched scope behavior of *want* and *want to have*.) This rather slender evidence is pitted against a strong constraint that intensional contexts are always proposition-embedding contexts. I think the jury is still out.

- (A7) The boys gave the girls nickels.**

In: Partee (1975a: 206).

This was my counterexample to Chomsky’s claim that in *Unicycles have wheels*, where *wheels* is not “semantically plural,” plurality should be a feature of the whole sentence. The dependent plural *nickels* can have either subject or object “licenser.”

- (A8) John is being hard to please/*It is being hard to please John.**

In: Partee (1977: 303, examples 34 and 35).

I used this pair as a further argument (cf. A1) for deriving *easy/hard to please* as predicates. It was part of an argument for the existence of an “active verb *be*,” bolstered by

examples (63) *John is being noisy* and (64) **The river is being noisy* on p. 307 of the same article. My conclusion that “active verb *be*” has a meaning something like “act” is challenged by Déchaine (1993).

(A9)a. Green bottles have narrow necks.

b. **The Coke bottle has a narrow neck.**

c. **??The green bottle has a narrow neck.**

(A9a) in Carlson (1977, 1980: 199); (A9b,c) in Krifka et al. (1995: 11, examples 24a, 24b).

Although I was sure that these were all in Carlson’s dissertation, only the first is. The earliest instance (A9b,c) seems to be in Krifka et al. (1995). Greg (personal communication) supposes that he presented them in talks and in unpublished work, because they were definitely well known before 1995. Jeff Pelletier (personal communication) accepts responsibility for the fact that Krifka et al. (1995) say (p. 11) that (A9b,c) come from Carlson (1977) and that Carlson there attributes them to Barbara Partee. Jeff also believed that they were in Greg’s dissertation, and Greg did always credit the examples to me. But Greg and others have searched the dissertation with care, and they’re not there. The issue they illustrate is certainly there: non-natural kinds are fine in bare plurals, but in order to use a definite singular to denote a kind, the kind has to be “well established.”

(A10)a. One of the ten balls is missing from the bag. It’s under the couch.

b. **Nine of the ten balls are in the bag. #It’s under the couch.**

In: Partee (1989a: 363, fn. 13). Earliest occurrence is in Heim (1982: 21), in the following version (credited to me, but this version may be her variation):

(21)a. I dropped ten marbles and found all of them, except for one. It is probably under the sofa.

b. ?I dropped ten marbles and found only nine of them. It is probably under the sofa.

In either version, the contrasting pairs provide an argument for Heim’s file change semantics: the pronoun needs a linguistic antecedent, not just a salient entity.

(A11) The department wants to hire a phonologist or a phonetician.

In: Partee (1983: 375, example 37).

A comparable example, *Mary is looking for a maid or a cook*, occurs in Rooth and Partee (1982: 355, example 13). In both versions, the puzzle is the existence of a reading on which each disjunct is understood intensionally (non-specifically), and yet *or* has wide scope (“but I don’t remember which”). The problem is a central topic of Rooth and Partee (1982).

(A12) Every search for two men with red hair failed.

In: Rooth (1985: 114), attributed to me with no particular source.

I believe I first began using this example in class and in oral discussions in the early 1980s. There is a three-way ambiguity, where one reading (where “two men” has

narrower scope than *every* but is outside the scope of *search for*) requires “Quantifying In” to the CNP, in addition to an *in situ* reading and a widest scope reading.

(A13) any triangle such that two sides are equal.

In: Marsh and Partee (1984: 187), also cited in Pullum (1986: 136).

This NP has a *such-that* relative clause containing no bound pronoun. We used it as evidence that *syntax* does not prohibit “vacuous binding,” although the clause is interpreted as a property of triangles (as if “sides of it”). We noted that putting such constraints in the syntax could affect the generative power required. We suggested that any constraints on vacuous binding belong in the semantics, since semantics must deal with variable binding anyway. Higginbotham (1984) dismissed my example as ungrammatical but interpretable, and argued that English is not context-free. See discussion in Pullum (1986).

(A14) John often comes over for Sunday brunch. Whenever someone else comes over too, we (all) end up playing trios. (Otherwise we play duets.)

In: Partee (1989a: 358, example 27), in this volume (chapter 13).

This example shows that *we* can consist of deictic *I* plus a “coreferential” pronominal part (coreferential with *John*) plus a bound pronominal part.

(A15)a. My 2-year-old son built a really tall snowman yesterday.

b. The DU fraternity brothers built a really tall snowman last weekend.

In: Kamp and Partee (1995: 142).

We used this pair to support the claim, from Kamp (1973), that an adjective like *tall* is extensional but vague and context-dependent. The noun is the same in both cases, but the sentences as a whole evoke contexts with different norms for heights of snowmen.

(A16) a midget giant vs. a giant midget vs. (be) a giant and a midget

In: Kamp and Partee (1995: 159).

We used these to argue that in “calibrating” vague context-dependent Adj-Noun combinations in context, one first calibrates the noun in context, and then one calibrates the adjective in the context as restricted by the noun.

Part II The B-list

(B1) Either this house doesn't have a bathroom or it's in a funny place.

In: Roberts (1989: 702).

The example is mine, generated during a lecture of Hans Kamp's in 1982. But I wasn't the first to think up such examples. Roberts (1989: 702) cites my example, and mentions in a footnote a very similar example from Evans (1977): *Either John doesn't own a donkey, or he keeps it very quiet.* I had read Evans, and I may have unconsciously plagiarized. Nirit Kadmon points out (personal communication) the similarity to Gazdar-Soames

presupposition examples such as *If Mary's boss doesn't have children, then it wasn't his child who won the fellowship.*

But the example is evidently memorable and attribution to me has continued. Roberts (1996: 243), cites it again and states "Examples like [B1], originally due to Partee, are often cited in discussions of anaphora in discourse," and she lists numerous citations. The variant *Either Merrill Hall doesn't have a bathroom or it is in a funny place* is attributed to me in Chierchia (1995: 8, example 23a).

(B2) John needed and bought a new coat.

In: Partee and Rooth (1983: 366, example 15).

This is an example of a conjunction of an intensional and an extensional verb, handled correctly by our rules for generalized conjunction. As our footnote 8, p. 365, says: "We owe the observation that the conjunction of an intensional and an extensional verb fits the predictions of the schema to Wynn Chao."

(B3) Every man faced an enemy.

In: Partee (1984a: example 34).

This example and several other related ones may indeed be mine, but the first such examples came from my dissertation student Jonathan Mitchell, who noticed context-dependence with words like *local*. Mitchell later completed his dissertation (Mitchell 1986), but did no more on the topic. I continued to develop it in Partee (1989a), which includes example (A14) as well as (8) with *local*, attributed to Mitchell, and (9) with *notice*, from Dowty (1987).

(8) Every sports fan in the country was at a local bar watching the playoffs.

(9) Every man who shaves off his beard expects his wife to notice.

Attributions of examples with *local* are a bit circular. In my papers cited above, I present the examples with *enemy* as mine, but I attribute examples with *local* to Jonathan Mitchell in Partee (1989a). But Craig Roberts's memory is that I invented an example like (8) for Jonathan's dissertation. Probably I did invent the specific example (8); I think Jonathan had examples with *local newspaper*, and I was concerned that *local newspaper* had an interfering reading as a *kind* of newspaper.

(B4) There were few faculty children at the 1980 picnic.

In: Partee (1989b: 395), with attribution to Alison Huettner.

The example is due to Alison Huettner, from an unpublished, untitled UMass 1984 term paper on *few* and *many* for a seminar co-taught by Bach, Kamp, and Partee. The point of the example, a novel idea of Huettner's, is that sometimes "few" can be "all," and in such cases it clearly must be cardinal, not proportional.

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