Reflections of a Formal Semanticist as of Feb 2005*

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1. A personal history within the development of formal semantics

What follows will be a very subjective and personal view, as much my own history and development in the field and how things looked through my eyes as about the development of the field itself. I have written more objectively about the history of the field elsewhere (Partee 1988b, 1996, 1997); here I am taking the opportunity to include things that don't always get written down, more of the human side of the story. And I will not suppress my tendency to interrupt myself with relevant anecdotes, most of which will appear in footnotes so as to try not to break the main flow of the narrative too much. Some people hate footnotes and digressions; I apologize, but this is me. Section 1, history, is the longest part of this essay, and the earlier periods (being more 'historical') are given a bit more than

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* This essay is a longer version of the introductory essay in (Partee 2004). The introductory essay was first written in this long form in February 2003, but had to be cut down to about half the size to fit in the book. By agreement with the publisher, I have waited until a year after the publication of the book, actually thirteen months, until February 2005, before putting the long version onto the internet. I have made a few revisions in 2005, partly in response to comments to the published version which I received from Paul Postal, Jeff Pelletier, and Larry Horn, for which I thank them.

I've given this version this peculiar date-including title both to distinguish it from the original published version and because it may be further revised in the future. Comments and suggestions are welcome.

Acknowledgements: I am grateful first of all to Susan Rothstein, the editor of the series, for urging me to write an introductory essay and being a good advisor and sounding board as I proceeded to work in this unfamiliar genre. For help at various points where my memory was uncertain, I am grateful to the following: Tom Bever, Iris Brest, Jerry Fodor, Gil Harman, Paul Kiparsky, Yuki Kuroda, Stanley Peters, Stanley Petrick, Carlota Smith, Peter Unger, David Warren via his daughter Tessa Warren, Beverly Park Woolf, and Arnold Zwicky. To Theo Janssen thanks for help with memory and some references plus very useful comments on an incomplete draft. More thanks to Susan Rothstein, and to Fred Landman, for very helpful advice on the penultimate draft of the published version. Many people responded to my request for help with the examples which appear in the appendix; I thank them separately there. I take full responsibility for any errors of commission or omission, and for all expressions of opinion and judgement.

Special thanks to Ji-yung Kim for excellent help at many points not only during the writing of this essay but at all stages in the preparation of (Partee 2004) and the gathering of permissions, and to Kathleen Adamczyk for typing up the previously unpublished "Airport Squib" and retyping the poorly-reproducible paper "Many quantifiers". This work was supported in part by the National Science Foundation under Grant No. BCS-9905748 to Barbara H. Partee and Vladimir Borschev for the project "Integration of Lexical & Compositional Semantics: Genitives in English and Russian".

their share of the space. Section 2 contains some general reflections about the social dynamics of the field and its 'personality', so to speak, together with some opinions on miscellaneous topics. Then there follows an Appendix containing a list of some of my best known example sentences, plus a few that came from others but became known through my work. I've annotated the examples to say something about their context; they form a little mini-history of their own.

1.1. "Preparation".

My early history is a mixture of lucky timing and of following my interests without any real idea of where they might lead, and of how I ended up in a sense ideally prepared for a field that didn't exist and of which I had no inkling as I was doing my "preparation".

As an undergraduate at Swarthmore College from 1957 to 1961, I majored in math and minored in Russian and Philosophy. In Honors, you were supposed to have a major and two related minors. I went to the head of the Math Department, Mr. Brinkmann, and told him that I wanted to do those three subjects but that I didn't see any relation among them – they were just the three subjects I loved best. Mr. Brinkmann scratched his head and said he thought it might be possible to invent a story that would connect them, because it seemed to him he'd heard of a new field called "mathematical linguistics" or "machine translation" or something like that, and since the philosophy seminars I wanted to do were logic and philosophy of science, well, computers use logic, as well as math, and this being just a few years after Sputnik and well into the Cold War, Russian was a language for which machine translation would be in demand. So if I would write up my request including some such story, my program would probably be approved[1]. And I did, and it was.

Anyway, as so often happens, having never really heard of linguistics before[2], I then began to notice the word more frequently and to begin to wonder if maybe it would be worth finding out what it meant. (Swarthmore had neither linguistics nor philosophy of language[3] then, and yet that's where David Lewis, Gil Harman, Ed Keenan, Peter Unger, and I graduated from at close to the same time, and Ray Jackendoff just four years after me (many others later, after Swarthmore began to have linguists on the faculty).)

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[1] In retrospect I can see that I benefited in a way from the unconscious sexism of the times. Neither I nor anyone else considered it important that my plan of study prepare me for any particular future; I felt perfectly free to just study the things I loved most. I don't think men were quite that free not to think about the future. I also managed to sneak some physics and two music seminars into my honors program. I don't regret any of it, and in fact I worry when students limit their possibilities of discovery by feeling pressure to make career plans early and pursue them "systematically".

[2] That's not quite true. In my freshman course in Psych 1, taught by Henry Gleitman, there was one guest lecture by Lila Gleitman, about phonemes and morphemes. It was fascinating. And for my little term paper/ book report for Psych 1, I chose George Miller's *Language and Communication* to write about, and found it full of interesting things, both empirical and formal. But I didn't catch on to the existence of a field until after my "fictitious" essay about my interest in the prospect of mathematical linguistics or machine translation.

[3] Peter Unger has just told me that the part about no philosophy of language is not true; but I somehow managed to miss it.

Through various bits of serendipity[4], I found my way into a seminar – I think it was called “Descriptive Linguistics” – maybe “Structural Linguistics” or “Linguistic Analysis” -- offered at Penn in the summer of 1960, between my junior and senior years of college, for people with a background in mathematics, philosophy, or psychology – just perfect. David Lewis, Gil Harman and I all had our first introduction to linguistics in that seminar. It was taught by Henry Hiż with guest lectures by Zellig Harris and Hans Herzberger, and Jerry Katz and Jerry Fodor, who had just completed their Ph.D.’s in Philosophy at Princeton, sat in the back of the room and kibitzed. It was a no-prerequisite introduction to the foundations of Harris-style transformational grammar, and it was great fun – I can remember filling out lots of charts working out co-occurrence patterns of various sorts. And the people at Penn knew that MIT was planning to start a graduate program in linguistics a year later, led by someone named Chomsky who had been Harris’s student, and who was doing something similar to what we were learning about. So since it seemed indeed to be something that was going to be great fun for someone with an interest in math and in languages, I applied to graduate school[5] at those two places.

But first, in the fall of my senior year, I went up to Cambridge to meet Chomsky and find out a little more about what it might be like there. And I remember with embarrassment my ignorant opening – I repeated what I had written to him, that I was majoring in mathematics but also interested in languages and that I understood there was a new field called mathematical linguistics or machine translation or something like that and that he did that and it sounded like something I would be interested in. And I remember how gently and calmly he replied, not chiding me at all for not having done more homework in advance of the interview. He explained that he was indeed interested in mathematical linguistics, and saw it as a branch of psychology, trying to figure out something about the structure of the innate human language faculty. And he explained that the field of machine translation was something different, and that he was somewhat skeptical about it, but that there would also be courses offered in it in their program, and I would have an opportunity to study both and figure out which I was more interested in.

I chose MIT over Penn for a rather timid reason – another stroke of luck, because it was the right choice, though I didn’t really know it then. The one person I knew who knew something about both programs was Carlota Smith, who was a graduate student simultaneously at Penn and at MIT, spending 1960-61 at MIT because her husband (a Swarthmore economics professor) was doing something at Harvard. So she knew about MIT’s plans for starting a Ph.D. program in Linguistics in the Fall of 1961. And the main

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[4] At one point, my uncle, an MIT alumnus, having heard somehow of my interest in this unknown field of mathematical linguistics from my father, sent me the program of the conference that was later published as (American Mathematical Society. 1961), and I wrote letters to almost everyone on the program who was in the U.S., asking if they knew of anything I could do in the summer between my junior and senior year that would help me find out what linguistics was. And a surprising number of them wrote nice long letters back to me, including Chomsky; they didn’t have any way themselves to host me for a summer, but several suggested I contact people at Penn, which I then did, and discovered the existence of the seminar described below. The kindness of established linguists in writing thoughtful answers to a letter from a completely naive young student made me want to always answer such letters myself, though I don’t always manage to live up to my intentions.

[5] I also applied for an NSF fellowship, and I remember seeing Linguistics listed under Social Science, and I couldn’t check that box, because I had never taken a single social science course besides History 1. So I checked the box under Mathematics, Other, and wrote in “Mathematical Linguistics”. Of course they sent my application over to Social Science, but at least I hadn’t claimed to know anything about social science.

difference I picked up from her was that the MIT program would be rather structured, with all the first-year students taking a common core of courses, whereas at Penn one was free right from the start to pretty much design one's own program. Well, I didn't know anything about linguistics at all, so 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 from the outset it was also my main interest, and became my dissertation area. (Phonology was also serious business, but that wasn't my main interest.) It was very exciting, and students were immediately made to feel part of the team discovering and inventing all kinds of new stuff – generative grammar was so new that there were limitless boundaries for finding important problems that hadn't been looked at very much if at all. We wrote much more than we read, and it was very heady. We all felt like pioneers in an exciting new venture. Chomsky was an inspiring intellectual leader, and Chomsky and Halle were devoted teachers and mentors. The difference between Chomsky and Klima was interesting: Chomsky was interested in puzzles in English syntax insofar as they provided some evidence for or against some theoretical hypothesis, and Klima was interested in theory insofar as it could help solve puzzles in English syntax. I loved the classes of both of them. I think I felt more Klima-like myself, but it somehow went without saying that I wanted Chomsky as my dissertation director, since it was made clear to us that it was theory that was Interesting with a capital "I".

What little semantics there was around MIT then, mainly due to the developing work of Katz and Fodor and then also Postal, was peripheral. It wasn't part of any courses we took, and it didn't seem very serious. This was undoubtedly due in part to Chomsky's deep skepticism about semantics, which can be seen in the closing chapter of *Syntactic Structures* and which he frequently expressed.

Katz and Fodor were junior faculty members[6] in Humanities at MIT, and they started making proposals about how a semantic theory could be developed in a generative grammar framework. They were clearly concerned with compositionality – what they called the Projection Problem. (The term 'compositionality' didn't come into general use until the development of formal semantics; I don't know exactly when or where[7].) Because such things as negation and question-formation were treated via transformations of affirmative declarative sentences, Katz and Fodor (Katz and Fodor 1963) figured that the meaning must depend on the entire transformational history. Their idea of extending phrase-markers to T-markers showing the transformational history of an expression, and computing the meaning on the basis of the whole T-marker can be seen as aiming in the same direction as Montague's derivation trees. But their semantics was very primitive, especially in comparison to what was going on in syntax. Katz and Fodor worked with "semantic features", and their semantic representations had no real structure, just bundles of features – suitable at best for decompositions of one-place predicates. Later they started adding some bits of structure to try to handle transitive verbs and their two arguments, but

[6] Fodor was definitely a faculty member then; Jerry Katz may have been too, or he may have had some sort of research appointment in the Research Laboratory of Electronics (Building 20). There were always a number of interesting people around whose status I often didn't exactly knew, some visiting, some with some sort of research appointments.

[7] Statements of the principle, but without the word 'compositionality', can be found in my earliest papers on Montague grammar, (Partee 1973b, page 52) and (Partee 1975b, p.203). One early explicit use of the term 'compositionality' is in (Thomason 1976, p.109.)

still very primitively, and with no attention at all to things like quantifiers. And what they were trying to capture was restricted to things that could be expressed in terms of 'readings' – how many, and same or different. The three main things to be captured were (i) ambiguity – having more than one reading; (ii) semantic anomaly – having no reading; (iii) synonymy – sharing a reading (synonymy on a reading), or the stronger version, having all the same readings. The examples of what they could capture didn't seem very exciting, and the accounts were sometimes open to easy counterexamples[8].

Then during my third year or so, Katz and Postal (Katz and Postal 1964) made the innovation of putting such morphemes as Neg and a Question morpheme into the Deep Structure, arguing that there was independent syntactic motivation for doing so, and then the meaning could be determined on the basis of Deep Structure alone. Surface Structure would then be the input to phonology. The claim that transformations should be meaning-preserving was an interesting and provocative one, and even without any 'real semantics' at the foundation, it led to interesting debates about apparent counterexamples. And the architecture of the theory (syntax in the middle, mediating between semantics on one end and phonology on the other) seemed elegant and attractive.

Chomsky's thinking was evolving from *Syntactic Structures* (Chomsky 1957) to *Aspects* (Chomsky 1965) while I was there, and he tentatively accepted Katz and Postal's suggestion of a systematic connection between syntax and semantics at the level of Deep Structure. (His continuing skepticism about semantics would still come out in such comments as "Well, I don't think anyone understands anything about semantics, but maybe what Fodor and Katz and Postal are doing has some promise.") But during the brief period when *Aspects* held sway and there was a rosy optimism that the form of syntactic theory was more or less understood and we could start trying to figure out the "substantive universals", roughly the mid-to-late 60's, before the linguistic wars broke out in full force, I think generative grammarians generally believed the Katz and Postal hypothesis. That was also the period when people entertained the "Universal Base Hypothesis", the conjecture that the grammars of all natural languages have the same base rules. (See brief discussion in (Partee et al. 1990a, p.556) and more in (Peters and Ritchie 1973).) The idea that meaning was determined at this "deep" level was undoubtedly part of the appeal of the notion of Deep Structure beyond linguistics[9] and probably contributed to the aura surrounding the notion of "language as a window on the mind." But at least among working linguists, that all fell apart when the behavior of quantifiers was noticed a few years later. The many differences between quantificational NPs and proper names (see discussion of example (4) below) immediately created great conflicts between syntax and semantics, and in a sense kicked all of us generative grammarians out of our Garden of Eden.

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[8] The earliest definition of 'contradictory sentence' in Fodor and Katz's work was that some semantic features in the predicate had opposite values from some semantic features in the subject, and they thus accounted for the contradictoriness of 'My uncle is a spinster.' But I recall pointing out to them that that only worked when the main verb was 'be', and would incorrectly attribute contradictoriness to 'Old men like young women.' So although there were thoughtful ideas about compositionality in their work, and although the Katz-Postal hypothesis laid some of the groundwork for Generative Semantics, the representation of meanings as bundles of features was pretty obviously an inadequate basis for any serious semantics. But on my own I had no idea what a semantic theory should look like.

[9] See, for instance, the references to Deep Structure in Leonard Bernstein's 1973 Harvard Norton Lectures, (Bernstein 1976).

In retrospect I can see that the papers I wrote in graduate school[10] were related to semantic issues, but without any semantics. I wrote papers on (the syntax of) quantifiers, and on negation. I did take a machine translation course with Victor Yngve in my first year, and for that course I wrote a little computer program (in Yngve's SNOBOL) to translate a small subpart of first-order predicate logic into English. I had to leave out negation, conditionals, and the universal quantifier – really not much left besides existential quantification and conjunction, but that was quite hard enough for me, since the predicates were a mixture of nouns, adjectives, and verbs. In our 'third year seminar', where we tried out potential dissertation topics, I worked on the *some/any* alternation in negative and interrogative sentences. The topic intrigued me in part because Klima's optional *some*-to-*any* rule (under a c-commanding [+Affective] morpheme) in (Klima 1964) was a meaning-changing transformation, in violation of Katz and Postal's hypothesis. But as I worked on it, I couldn't get a satisfactory account without positing three different *some*'s, and that was very unsatisfying. So I abandoned that topic, and that was just as well: I couldn't diagnose the stone walls I was hitting, but it really wasn't a problem to solve with syntax alone.

Later, again by accident[11], I found a dissertation topic completely different from what I had been working on up until then, namely the syntax of subjects and objects and various transformations that changed them around, affecting what we would now call argument structure. (It's hard to realize now that the term "argument" came into use in syntax only later, after formal semanticists had been using it for awhile.) Actually, my dissertation proposal, which Chomsky agreed to with enthusiasm, was to write a grammar of English, synthesizing all that had been done in transformational grammar up until then. But that turned out to be more than I could do in year[12], and luckily I had never heard of the

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[10] I don't have all of titles, but here are some: "All about predeterminers" (my first term paper, 1962), "A preliminary attempt at an historical approach to Modern English predeterminers" (1963), "Pre-articles in English: their contemporary grammar and its historical development" (1963), "Remarks on <u>some</u> and <u>any</u> in negation and interrogative constructions with a note on negation in Russian" (1963), "The auxiliary in English sentences with 'if'" (1964), read at a summer LSA meeting, my first public presentation; "Adverbial subordinate clauses" (1964, written while working at the MITRE Corporation).

[11] The occasion was a conversation with Ted Lightner, who was insisting that syntax was impossible and only phonology and morphophonemics made sense, and I was of course vehemently denying it, and he said, "Oh, yeah? Then what about "John opened the door" and "The door opened" – that's not passive, so what is it?" And by the end of our heated conversation I had invented a version of the unaccusative hypothesis (but only for verbs that occurred in transitive-intransitive pairs), which became the cornerstone of my dissertation (Hall 1965), later published as Partee (1979d). I didn't follow up on that topic in later work except indirectly (Stockwell et al. 1973) and very recently in current work on the Russian Genitive of Negation. See (Pullum 1988) for some humbling documentation about who had had similar ideas earlier that I didn't know about and didn't cite, and who had similar ideas later and didn't cite me. The bibliography of my dissertation has a grand total of 15 references. The first draft had none, but Chomsky suggested that a dissertation should probably have some references in it, and Paul Kiparsky, who was literate, helped me compile some. Citation etiquette was not part of an MIT education in the first generation, although Klima tried. I only really learned about paying attention to prior written work after I got out of there. Here's another related anecdote: while our class was still at MIT, Emmon Bach's first textbook (Bach 1964) appeared. We students were shocked that someone not at MIT could even know about transformational grammar – we thought that we who were sitting at the feet of the master were the only ones privileged to receive the Word. When some of us met Emmon for the first time at a meeting, we asked him how he had found out enough about it to write a textbook without being there. He smiled indulgently and replied, "Well, I can read."

[12] But see below about the UCLA Syntax Project, where a team of did take on that project.

possibility of going away without finishing your dissertation – I thought it was a kind of one-year take-home exam[13].

Although the center of my life at MIT was syntax, some of the other things I learned and did during those years had a lot to do with my future in semantics, although as usual, I was just following my nose with no particular goals[14] in mind. At MIT, you had to have a minor as well as a major subject in the Ph.D. program, so I minored in math. In my first year, I had a course in Automata Theory with E.F. Moore, visiting then from Bell Labs, and a wonderful course in logic and recursive function theory with Hartley Rogers, Jr., which included a little bit of model theory, as well as a philosophy of language course with Hilary Putnam (but he wasn't yet into his later interesting work: we read a book by Paul Ziff[15]).

Starting in my second year, at Chomsky's suggestion (after I had mentioned that I would love to do some teaching if possible) I taught a course in mathematics for linguists, for each of my last three fall semesters (as well as later at UCLA and UMass), the course that became the basis for (Partee 1979a) and my part of (Partee et al. 1990a). In order to find out more about set theory, I took a course with Quine at Harvard that corresponded to his book (Quine 1963), then in proof stage. Unfortunately, that course was mostly about his own new and idiosyncratic set theory. Although I didn't really enjoy that course, I guess I gained some more proficiency in doing logic[16], which probably made Montague's work a little less intimidating when the time came. I also had a model theory course with Michael Rabin, who visited MIT one year. That course was over my head, but the first little bit which I could understand was interesting. All in all, between the math and logic courses I took and the work I did developing and teaching my math for linguists course, I

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[13] I didn't know you were allowed to talk to your dissertation committee while you worked (so I didn't until near the end, when one of my classmates said I was supposed to), and I don't remember who was on my committee besides Chomsky (chair) and Klima. Another thing I didn't know was that they could pass two dissertations that made conflicting claims. Peter Rosenbaum defended a week ahead of me and claimed that reflexivization depended on cyclical clause-mate identity, and they passed his dissertation. Mine claimed that reflexivization depended on deep-structure clause-mate identity -- but to my happy surprise, they passed mine too. There were a lot of things we didn't know, being in the first class in a new program. Even the faculty were making up some of the rules as we went along, so that, for instance, 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 the first year) 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, at the end of my second year, that the qualifying exams should be in the second year, and we ended up taking the exams at the same time as the second class, in our third year and their second.

[14] In fact, it was only half way through the Ph.D. program that I woke up one morning and realized I probably couldn't be a fifth grade teacher, which had been in the back of mind for a long time as one of the nicest grades to teach – in the fifth grade we had started to have interesting ideas but unlike the sixth-graders, we didn't yet think we knew everything. But now it dawned on me that if you get a Ph.D., you don't teach fifth grade. I had stayed in school too long; so it looked like I was going to be a college teacher instead.

[15] In Putnam's course, I got acquainted with Bob Futrelle, a physics graduate student I became friends with who introduced me to his roommate, a Russian instructor at MIT named David Perlmutter. Sitting in on Perlmutter's Russian course for fun, I was blown away by the way he taught Russian. David was a natural linguist, uncovering regularities of all sorts all over the place, and I'm proud that I helped convince him to consider going into linguistics.

[16] I ended up getting credited in a footnote in the first edition for strengthening one of the theorems; it was one that I didn't understand at all, but I had developed some 'clerical' techniques for making my way through proofs that involved not using premises until they were needed, and in that case it turned out that some premises weren't needed.

gained background that stood me in good stead in later years. You can never predict what parts of mathematics will be useful later, but every bit you learn makes it easier to learn more and to know how to ask for help and then to process the replies. (And teaching is for sure the best way to learn – you get to deal with not only your own questions but everyone else's.)

I went to UCLA in 1965, and by 1966, Stockwell, Schachter and I were engaged in our big English syntax project (1966-68)[17], with a big team of graduate students, which led to our book (Stockwell et al. 1973). We self-consciously tried to limit our attention to syntactic work done in transformational grammar up to 1966, with only a few exceptions (mainly Fillmore's work on case grammar and Chomsky's 1968 "Remarks on nominalization"), and to pay attention only to syntax, not semantics. That meant staying detached from the generative semantics-interpretive semantics debates, at least in our work on the project; that was very hard to do, but necessary. We had a team for each chapter, with a faculty leader for each team; my chapters were "Determiners", "Pronominalization", and "Negation". Each week one team had to make a presentation to the group, and by full group discussion we were able to keep the parts of the grammar consistent. And it was enough like being a participant in a seminar that I was able to get my 'homework' done, although in general I find it all but impossible to write books.

But although the bulk of my output[18] during that period went into that project, I couldn't help but get into the syntax-semantics fight a bit. While I had been writing my MIT dissertation, George Lakoff had been writing his Harvard dissertation and generative semantics was getting started[19]. Haj Ross, who started the program two years after I did but with a lot more background (he had been a graduate student at Penn before switching to MIT), was doing syntax, leading up to his monumental dissertation, but I think starting to think about semantics together with George – their joint papers begin in 1966, and their

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[17] This was the fulfillment of my original dissertation dream, which, happily, it turned out that Stockwell and Schachter independently had thought of doing. We got an Air Force grant to do it, thanks to Jay Keyser's remarkable persuasive abilities during his AFROC-mandated stint as an Air Force lieutenant, and to Bruce Fraser's good work as his successor. For a while, the Air Force was convinced that supporting pure research in generative grammar was a national priority, and we all tried to convince ourselves that taking Air Force money for such purposes was consistent with our consciences, possibly even a benign subversion of the military-industrial complex. A very big grant from the Air Force was also part of what supported graduate students in linguistics at MIT for a number of years, and the Air Force funded the MITRE Corporation, where many of us had interesting summer jobs in syntax and computational linguistics during our graduate careers under the very benevolent and enlightened leadership of Don Walker.

[18] I mean my academic output. A considerable amount of my energy during my UCLA period went into "life" issues – my first marriage, to Morriss Henry Partee, and the birth of our three children, Morriss, David, and Joel. When I got married, in 1966, I changed my name from Barbara C. Hall to Barbara Hall Partee. When the marriage ended in divorce in 1971, I kept that name in part because my three sons were Partees, and in part because I had by then published a number of things with the name Partee, and only my dissertation and my one and only review (Hall 1964) under the name Hall. And although I felt more emotional attachment to the name Hall than to Partee, I realized it could be an advantage to have a less common name; there were already a few linguists named Hall, and no others named Partee; that wasn't one of my reasons for keeping the name, just a reason not to regret having taken it.

[19] I don't remember the exact nature of Lakoff's connection to MIT (I'm sure it's documented in (Newmeyer 1980a), as is much about MIT in those years), but he was certainly around, and he had an unpublished paper in 1963 in MIT's Research Laboratory of Electronics, an umbrella for all of linguistics at MIT, called "Toward generative semantics".

joint paper "Is deep structure necessary?" was in 1967, the same year as Ross's dissertation. I can remember arguing with George about causatives during 1964-65, and my dissertation has some examples arguing against analyzing transitive *open* as the causative of intransitive *open*. McCawley, my classmate, was publishing generative semantics articles by at least 1968. Postal, who was a postdoc and sometimes a lecturer at MIT during my student years, and was an early Chomskyan, contributed to the generative semantics movement shortly afterwards; his "Linguistic Anarchy Notes" started coming out of IBM about 1967. Jackendoff was a student at MIT right after I was, 1965-69, and he and Chomsky launched the counteroffensive of "interpretive semantics" during that time and continuing afterward.

My instincts were mixed: the generative semanticists were taking semantics seriously and uncovering a wealth of interesting generalizations. On the other hand, they kept putting things into the syntax in ways that seemed wild[20]. I thank some of Lakoff's excesses for helping me get over my inhibitions about publishing (I still felt more like a student and had trouble imagining myself as an "author"). My examples (**A1**)[21] formed the core of the argument in (Partee 1970a), which was answered in the same issue by (Lakoff 1970)[22]. Shortly afterward I published a paper (Partee 1971) in which I analyzed some of central issues I saw behind the debate, focusing on the Katz-Postal hypothesis that transformations preserve meaning and identifying the main problems I saw on each side. I didn't reach any definite conclusions, and at the time I saw that as a shortcoming of the paper, but in retrospect I think the paper helped clarify the issues, and over time I came to appreciate that impartial analysis was probably one of my strengths, and worthwhile.

Linguists were also beginning to discover that there was interesting work in philosophy of language directly applicable to questions of semantics. As I began to discover the philosophy of language literature, 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), with some good examples, including (**A2**) and (**A3**), but an unsatisfactory analysis[23].

1.2. Montague and the beginnings of Montague grammar.

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[20] What seemed wild then might not now: the shocking number of clauses (7?) in Ross's deep structure for "Floyd broke the glass" does not come close to the number of functional projections that now intervene between various pairs of "familiar" syntactic categories in respected generative analyses such as (Cinque 1999).

[21] All examples with numbers beginning with A or B are found in the Appendix.

[22] Frits Staal invited Lakoff and me to publish my paper and Lakoff's reply simultaneously in his journal so as to air the debate accessibly, and we were able to correspond and to modify both our papers in advance. That was nice; I saw in Lakoff's first draft that he had rightly criticized one of my arguments as a non-argument, and we jointly agreed that I could just remove that non-argument and he would remove his critique of it, so that we could focus on our substantive disagreements. We disagreed strongly, but it was good to be able to air some of our strong emotions about some of the then-current debates in private correspondence, and keep the published articles civil and constructive.

[23] In a footnote I report a critique I had received from Lauri Karttunen which gave good evidence that my proposed binary feature [+/- Specific] was inherently incapable of making distinctions that could be made with a scope analysis. I'm glad that I didn't just withdraw the paper then, and instead published the paper with the footnote; otherwise it would be in the list of regrets in Section 2.2. of things I didn't publish just because I couldn't reach an unequivocal conclusion.

I had seen Montague in some Philosophy department colloquia, and knew of his sharp tongue[24] but knew nothing of his work, when David Lewis one day told me that Montague was starting to apply some of his work in logic and recursive function theory to the semantics of natural language, and I might find it interesting. David and I had known each other since Swarthmore, and he did his Ph.D. in Philosophy at Harvard while I was at MIT. David knew me well enough to guess that I would find it interesting to sit in on Montague's seminars, which I did for the first time in 1968, along with him and my Ph.D. student and friend Frank Heny. In post-seminar discussions, David helped us greatly in deciphering the logic.

Montague's way of looking at natural language was strikingly different in many ways from anything I had been exposed to in linguistics. But at least I knew (and had been teaching to linguistics students) what "models" meant in logic, so that with some considerable help (I had never seen a lambda before[25], nor did I know anything about intensions and extensions), I could follow it well enough to begin to appreciate it.

Two aspects of Montague's approach looked especially exciting. The first was the then revolutionary (to a linguist) idea that the core data to be accounted for were the truth conditions of sentences, and semantic values of other constituents should be worked out so as to compositionally combine to give the right truth conditions for the whole sentences. Suddenly there was a non-subjective criterion of "observational adequacy" for semantics, where there had been none before[26].

The second exciting aspect of Montague's approach was that it incorporated some powerful tools that would let semantics do some real work, which in turn could help keep

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[24] I witnessed during colloquium discussions what others said was quite general: Montague would simply say what he thought, no matter how negative, and had little use for tact, diplomacy, or silence. I don't believe he was ever malicious, but it must not have been pleasant to be on the receiving end of some of his comments.

[25] My Dutch colleagues still recall one of the sentences I reportedly uttered in my 1980 talk in Amsterdam "The First Decade of Montague Grammar": "Lambdas changed my life." I probably did say that, because it was certainly true. Theo Janssen, who cited the quotation in (Janssen 1994), says (p.c.) that "this quote is the best summary of Montague's revolution: it is the feature that made compositionality possible at all, and the quote expresses its impact."

A few more of many possible lambda-anecdotes. (i) Edwin Williams told me that when he included lambdas in a paper he wrote as an MIT student, Chomsky asked him if he was becoming a Montague grammarian. (ii) Joan Bresnan used lambdas in an early draft of one of her early LFG papers, written during the brief time when she taught at UMass, but in the final version she replaced them with some *ad hoc* notation, perhaps so as not to be seen as a Montague grammarian. (iii) When Ivan Sag and some colleagues and students began working on a project at Hewlett-Packard, there was an obligatory interview with a company lawyer, for the purposes of exploring whether anything in their approach might be patentable. After they had explained GPSG and its semantics, the lawyer asked them whether anybody had any prior claim on the use of lambdas. They appreciated the lawyer's taste, but had to disappoint him and explain that Alonzo Church had invented the lambda calculus some decades earlier.

[26] Objections have been raised to truth conditions on various fronts, some based on misunderstandings, and others related to philosophical problems surrounding propositional attitude constructions, problems that are not simple but that don't have to be solved in advance of making progress on most other kinds of constructions (my views on these matters can be found, for instance, in (Partee 1988a, 1988b)). Most of the simpler objections can be answered just by recognizing that the models relevant for linguists' use of model theoretic semantics (including for truth conditions) should be based on what Emmon Bach has christened "Natural Language Metaphysics" (Bach 1986a). What must not be forgotten is that when it comes to trying to evaluate or compare semantic analyses, even an oversimplified notion of truth-conditions provides a gigantic advance over reliance on intuitions about "semantic representations" whose "real semantic" content is unspecified.

the syntax clean and elegant. In particular, Montague's rich type theory (whose antecedents are acknowledged in his work) made it possible to analyze virtually all of the most basic grammatical relations in terms of function-argument structure. In retrospect we can see that the generative semanticists and other linguists who tried hard to make semantics compositional were hampered by the mismatch between natural language syntactic structure and the structure of first order logic, the only logic most linguists knew. This is why lambdas, types, and the example of generalized quantifiers have such prominence in formal semantics: before their advent, there was simply no way to treat a quantificational NP as a semantic constituent, and no imaginable way to treat proper names and quantificational NPs as semantically of the same type, hence no way to imagine the syntactic category NP as a semantically significant category on its own. Just as lexical decomposition was rampant, being the only way linguists could think of to capture semantic properties of lexical items, so syntactic "decomposition" into ever deeper and deeper underlying structures was the only way linguists could explicitly represent the content of sentences containing quantifier phrases. More generally, linguists tried to capture semantic equivalence through identity at some representational level; this was undoubtedly responsible for some of the more skepticism-provoking syntactic analyses by the generative semanticists. It was a major innovation for linguists to be given the idea that syntactically distinct structures could be shown to be semantically equivalent using explicit logical tools.

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 "UG" (Montague 1970b), but best known from "PTQ" (Montague 1973). David Lewis (Lewis 1970) also had the idea of treating NPs as generalized quantifiers and having the subject take the VP as its argument. Lewis and Montague talked to each other regularly, so there is probably no way of ever knowing which of them had priority on this idea. But it was through Montague's work, embedding that idea in a broader systematic framework of great richness, that the idea caught the imagination of linguists and became part of the foundation of early formal semantics; it is still a textbook staple[27].

Jumping ahead a bit, it's probably worth emphasizing that none of the arguments for Montague's approach to semantics were in any way "anti-syntax". Just as Grice's pragmatic principles were motivated in large part by a desire to keep the semantics clean and principled, so having semantic tools that could do some real work could help keep the syntax clean and principled. Compositional semantics is dependent on having a good syntax to build on. Montague's own syntax didn't appeal to everyone, but his principle that the syntax should be autonomously described and should simultaneously determine well-formedness and provide a foundation for semantic composition was congenial to many linguists[28].

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[27] I discussed the treatment of NPs as generalized quantifiers in all my earliest lectures on Montague Grammar and in (Partee 1973b, Partee 1975b). It figures prominently not only in textbooks on formal semantics (Bach 1989b, Chierchia and McConnell-Ginet 1990, de Swart 1998b, Dowty et al. 1981, Gamut 1991), but also as the prototypical example to use in giving students a first taste of formal semantics, as in chapter 14 of (Partee et al. 1990b) and in (Larson 1995), the semantics chapter of an introduction to cognitive science.

[28] There was, of course, one very influential skeptic about compositionality of Montague's or any other sort; see (Chomsky 1975) and my reply (Partee 1975a). See discussion in Section 2.

In bits and pieces with the help of several philosophers (especially David Lewis and David Kaplan, later also Saul Kripke, Jaakko Hintikka, and Julius Moravcsik), I began to learn about what had been going on in "West Coast" semantics in logic and the philosophy of language during the years when I was getting my education on the East Coast. It was shocking to me to realize that West Coast philosophers (with Tarski and Carnap as important influences) were busily developing possible worlds semantics and higher order modal and intensional logics while East Coast philosophers (with Quine as a central figure) were adamantly insisting that only first-order extensional logic was real logic. I hadn't understood that philosophers or logicians could be dogmatic[29] or differ so strongly about what their fields were about. And this accident of geography made a huge difference to the field.

By the time of the fall 1970 conference at which Montague presented PTQ, I was ready to put all of my 'discussant' efforts[30] into a first attempt at describing for linguists some of the key differences between Montague's approach to syntax and transformational approaches (Partee 1973c). I was not yet ready to say much about the semantics, although I could describe some of the semantic motivation for how the syntax was done. That was the only occasion on which I made a presentation about Montague's work with Montague present; his tragic death[31] occurred just months later. I was glad to have had his assurance that I was not misrepresenting him, and I was able to get an answer to the question of whether there was any reason other than uniform category-type correspondence to treat *John* as a generalized quantifier (no)[32].

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.

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[29] I met Saul Kripke, one of the founders of possible worlds semantics, at a Summer Institute in Philosophy of Language and Linguistics at UC Irvine, organized by Donald Davidson and Gil Harman in 1971. I was just learning about this 'East Coast – West Coast' split, and I asked Kripke if he could tell me *why* Quine was so opposed to modal logic and intensions. Kripke's reply was that before 1959 Quine was right that modal logic and intensional notions were in a rather confused state, but that after 1959 (the date of (Kripke 1959)), it was just lingering prejudice. See (Partee 1988b, 1997) for more on the philosophical and logical background of Montague's work.

[30] My own paper at that conference, Partee (1973d), was my first attempt to come to grips with the semantics of propositional attitudes, and to worry about whether what was embedded was semantically a proposition (set of possible worlds) or something closer to a quoted sentence; the example (**A6**), of a sort philosophers had not discussed, comes from that paper.

[31] What is known about Montague's death is described by Feferman and Feferman (2004), who also offer vivid glimpses into Montague's relation to Tarski, and into his life (he was an accomplished musician, became wealthy in real estate, and was a member of the homosexual community).

[32] The treatment of proper names as generalized quantifiers seemed initially to be a price one had to pay for the elegance of Montague's uniform category-type correspondence, and all of us who taught Montague grammar in the early years had to try to persuade our students that it was not inconceivable that it could be the right treatment, even though we all acknowledged that there was no evident independent motivation for it. It was not until later that several kinds of motivation for type-shifting led to the alternative proposal, 'in the air' and included in Partee and Rooth (1983) but perhaps first systematized in (Partee 1986), that generalized quantifiers are probably only one of two or three kinds of NP interpretation. Motivations for type-shifting came from several sources, including my work with Mats Rooth on conjunction (Partee and Rooth 1983), the work of Irene Heim (Heim 1982) on indefinite and definite NPs and the related independent work of Hans Kamp (Kamp 1981) on Discourse Representation Theory, and ideas about replacing individual rules with general principles of type-driven interpretation (Klein and Sag 1985).

At the time of his death, I was in the middle of writing a UCLA grant proposal for something that could not exactly be called a joint project with Montague – that would have been presumptuous – but a project in which I would exploit his appreciation of "puzzles" to try to provoke him to figure out what he would do about some constructions which linguists used to argue for the transformational cycle, constructions which seemed problematic for his bottom-up surface-y compositional derivations. I was particularly concerned with the threesome of *There*-insertion, Passive, and Raising, which together would generate sentences like (1), which I didn't see any way to derive compositionally bottom up from surface constituents.

(1) There was believed to be a unicorn[33] in the garden.

I never had a chance to ask Montague how he might handle such sentences, though there is a hint in PTQ that he might not object to the inclusion of transformations if they were meaning-preserving. He mentions Moravcsik's example (2a) as an apparent case of intensionality in subject position, and notes that he would not try to generate it directly but would more likely account for it indirectly via the derivation of a sentence like (2b), which some of us interpreted as countenancing a possible transformational derivation of one from the other.

(2) a. A unicorn appears to be approaching.
b. It appears that a unicorn is approaching.

Michael Bennett, who had been working with Montague, wrote his dissertation (Bennett 1974) with David Kaplan and me as co-chairs[34], and among his three different fragments extending Montague's work, devoted the third one to such rules and constructions, and showed how it could indeed be done, although what the best way is to handle such cases remains a matter of debate and ongoing research.

In the aftermath of Montague's death, carrying on with the exploration of his ideas became a cooperative group effort, first mainly at UCLA[35], and then more broadly. I was

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[33] After I told the first-year semantics class in the Fall of 2002 about how the unicorn came to be the 'mascot' of Montague Grammar, they made me smile by bringing colored paper unicorn horns to all their classes on Halloween. It started from one of Montague's key examples, *John seeks a unicorn*, an example that creates an intensional context without any embedded proposition on the surface; Montague showed that if you have a rich intensional type theory, you don't need to assume any syntactic decomposition. ("Unicorn" is not a random choice; it's relevant that the truth of the sentence doesn't require the existence of unicorns.) Bob Rodman had the idea of putting a unicorn on the cover of the UCLA Working Papers in Linguistics volume on Montague Grammar which resulted from the two-quarter seminar I taught in 1971-72 (Rodman 1972). I did the same with the first published collection of papers in Montague Grammar (Partee 1976b), and unicorn T-shirts proliferated at Montague Grammar workshops and Linguistic Institutes. Theo Janssen's dissertation (Janssen 1983b) has a clever series of appropriately designed unicorn-pictures heading each of the chapters.

[34] So did Enrique Delacruz: see (Delacruz 1976), a revision of his 1972 paper of the same name in (Rodman 1972), and the kernel of his 1974 dissertation.

[35] Outside of UCLA there was Rich Thomason, who edited and wrote a very substantial introduction to Montague's papers related to language (Montague 1974) and was heavily involved from the beginning, and there were colleagues like Max Cresswell in New Zealand and C.L. Hamblin in Australia and former students of Montague's like Dan Gallin and Hans Kamp, but I didn't become acquainted with most of them until later.

permitted to teach a one-quarter seminar on Montague grammar[36] in fall 1971 at UCLA, and then based on student demand (because we were all just beginning to get the hang of it after one quarter) was permitted[37] to continue for a second quarter, in which we all wrote papers, which Bob Rodman had the good idea of publishing (Rodman 1972). I taught courses on Montague grammar in other places as well, and gave many talks; much of my own research at that time was focused on how to combine Montague grammar and transformational grammar (MG and TG)[38]. My "standard" one-hour talk was written up as Partee (1973b), and the lecture notes that evolved as I taught seminars on Montague grammar at UCLA, at the California mini-Linguistic Institute at Santa Cruz in the summer of 1972, at UMass, and in the 1974 Linguistic Institute at UMass were published as Partee (1975b), which served as a first quasi-textbook for linguists. It was replaced in that function first by (Dowty 1978b) and then by (Dowty et al. 1981).

Let me give one example of the sort of thing that was a big problem confronting the integration of MG into linguistics, but not even a question that would come up now, because so much progress has simply been absorbed into the background. It was during the Philosophy-Linguistics Institute at Irvine in the summer of 1971 (more on that later) that I finally put my finger on a major obstacle to integrating MG and TG: what to do about deletion rules, such as the rule of "Equi-NP Deletion" which deleted one of two identical copies of an NP in control structures. In classical TG, (3a) was then derived from a structure something like (3b).

(3) a. Mary wanted to win.
 b. [$_S$ Mary wanted [$_S$ Mary Tns win]]

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[36] The term "Montague grammar" was my invention, as a handy short name for the system spelled out in Montague's "UG" (Montague 1970b) and applied to English in that paper, in "EFL" (Montague 1970a), and especially in PTQ (Montague 1973). I'm pretty sure the term was first used in the Montague grammar seminar I taught at UCLA in 1971-72, and first appeared in print in (Rodman 1972). The Oxford English Dictionary added an entry for "Montague grammar" in December 2002; see http://dictionary.oed.com/cgi/entry/00315183.

[37] Bob Stockwell was my chairman at UCLA; and he gave me permission to replace my scheduled seminar in Mathematical Linguistics with a second quarter of Montague Grammar. Then he said something like, "This is all very interesting, but when are you going to get back to doing linguistics?" That question really 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.

[38] In settling on the contents of (Partee 2004), Susan Rothstein and I decided that although my first papers in that direction (Partee 1973b, 1975b) were influential, there is no active demand for them now. It seems that they have become "historicized", with some of the ideas absorbed into the field and others discarded or supplanted. I realize on reflection that this is how we solve the apparently insuperable problem that every new generation of students should have to master more literature than the generation before, and yet the length of graduate programs can't just grow. Sometimes a piece of literature survives in the form of 'obligatory citations' as the source of certain ideas. Other times the ideas become so assimilated that students no longer have any idea that they were once somebody's research. It was a long time before I stopped teaching semantics students Montague's PTQ, but I haven't done it for years now. The first textbooks in the field (Dowty et al. 1981, Gamut 1991, several years earlier in Dutch, Link 1979), and several other German textbooks, all introduced Montague's work very explicitly. In more recent textbooks (Chierchia and McConnell-Ginet 1990, de Swart 1998a, Heim and Kratzer 1998), Montague's contributions themselves have been absorbed into the background of more recent work.

But MG works by building up the meanings of constituents from the meanings of their subconstituents, and there is nothing that could correspond to "deleting" a piece of a meaning of an already composed subpart. When I finally understood the problem, I had an idea for its solution, finally appreciating why all my philosopher friends had been urging me to "use lambdas". (I had been resisting because I didn't want to put them into the English syntax, and it took me until that summer 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 fundamental incorporation of the elegant Katz-Postal hypothesis.

It may be hard to believe now, but for many years, nobody thought at all about the consequences of the analysis in (3) for a sentence like that in (4a), whose deep structure should accordingly be something like (4b).

(4) a. Everyone wanted to win.
b. [$_S$ everyone wanted [$_S$ everyone Tns win]]

The semantic problem is apparent: a second identical NP gives the wrong meaning. Once such sentences were taken account (and I'm not giving attributions here, because I think quite a number of people figured this out at around the same time, and I have no idea who was first[39]. I agree that I was part of it, and I certainly helped to spread the word about how important this issue was, but I don't think I was by any means the first to see it.), then thinking in terms of Montague grammar, where infinitives were anyway treated as VP type rather than sentence type, I could see that what we wanted as "underlying" subject in the embedded sentence was a variable which could get bound. Debates continued: I followed Montague's line and let it be bound by lambda abstraction to make a VP type; others who believed in keeping an S type for the infinitive let the variable be bound by the higher quantifier. Either way, there was just one occurrence of the quantifier, in the matrix sentence, and the 'subject' of the infinitive, if represented at all, was a bound variable. In Chomskyan syntax, a corresponding change was eventually made, replacing the ordinary identical NP by the special null element PRO, interpreted as a bound variable. Other syntactic theories, like GPSG, HPSG, and LFG, and modern versions of Categorial Grammar, were developed after the quantifier issues had become well known, so they were designed from the start not to run into the problems of the old Equi-NP Transformation.

Back when almost all examples used proper names, the problem had not surfaced. It is probably not at all transparent to students now which of the many changes between *Aspects* (which in this particular respect did not differ significantly from *Syntactic Structures*) and more recent versions of generative grammar were necessitated by the "discovery" of quantifiers and variable binding. Issues of reference, "referential indices", coreference, binding are still central to questions about the relation between syntax and semantics, and are still difficult.

[39] Thanks to Paul Postal (p.c.) for reminding me that Jim McCawley was undoubtedly one of the first to notice this point and its importance. Paul notes that Jim "had noticed the issue and proposed a representation in which what was deleted by Equi was identical indices at least as early as (McCawley 1968). This didn't involve lambda abstracts but certainly was a long way from the previous view, and it seems to have preceded the Monatgue grammar induced ideas by a couple of years."

While "Montague grammar" was undoubtedly the principal vehicle by which the influence of model-theoretic semantics came into linguistics, there were other more or less connected lines of similar research which contributed to the ensuing cooperative linguistics-philosophy enterprise. The work of David Lewis is important in this regard, both because Lewis, who knew the work of Chomsky and other linguists quite well, was an important influence on Montague's own work via conversations and his participation in Montague's seminars, and because Lewis (1968, 1969, 1970) presented many of the same kinds of ideas in a form much more accessible to linguists. Cresswell (1973) was another related work, a book-length treatment of a similar semantic program, with a great deal of valuable discussion of both foundational issues and many specific grammatical constructions. Also Parsons (1972), Keenan (1971a, 1971b), and Thomason and Stalnaker (1973) were early and active contributors to linguistics-logic-philosophy exchanges. Keenan was in some respects an individualist, interacting in friendly ways with others but going his own way in his own work. The others that I just named all knew and interacted with one another. Cresswell spent a year at UCLA; Thomason and Stalnaker did joint work on the semantics of adverbs (Thomason and Stalnaker 1973) when they were both at Yale, and Thomason edited and wrote a substantial introduction to Montague's collected papers on topics in formal philosophy (Montague 1974), including his three seminal papers on universal grammar and the semantics of natural and formal languages. Terry Parsons, then at the University of Illinois at Chicago Circle, made a visit to UCLA, where he and I got acquainted a couple of years before we both moved to UMass Amherst in 1972.

1.3. The first decade of Montague grammar.

The 1970's was a period of great flowering of MG, with a great deal of interaction between linguists and philosophers. With the rich tools that Montague's typed intensional logic (especially lambdas!) provided, it was suddenly possible to transcend most of the generative semantics – interpretive semantics debate[40]. One could provide semantic analyses that captured the kinds of generalizations the generative semanticists had discovered and still work with a syntax that stayed remarkably close to surface structure, often even less abstract in many respects than the relatively conservative grammars preferred among interpretive semanticists. For instance, as noted above, infinitival complements were generated as VP complements rather than as sentential complements; and phrasal conjunctions with "sentence-conjunction meanings" were generated directly rather than via Conjunction Reduction. Prenominal adjectives, for semantic reasons, were not in general derived from relative clauses, as had been standard in the syntactic literature up until then, even among generative semanticists[41]. And David Dowty, after writing a

[40] That is not to suggest that the debates suddenly ended, or that the rise of Montague grammar was the sole reason for the decline of generative semantics. In fact Adrian Akmajian and I, upset that some of our students seemed to be scoffing at generative semantics without knowing anything about it, taught a seminar on generative semantics at UMass during 1973-74 in which we took the part of the generative semanticists in presenting some of their best work and made the students find counterarguments if they could (to which we could often come up with good generative semantic rebuttals).

[41] There was a small conference of linguists and philosophers at UCLA in 1970, memorable in part because it was moved to the basement of a church after Reagan closed the University of California in the wake of protests over the bombing of Cambodia. Montague and Lakoff were among the participants, and I remember a moment in which Montague had said something about generating adjectives in prenominal position, and Lakoff had jumped up and asked didn't he know that adjectives were derived from relative clauses, and

generative semantics dissertation, had quickly discovered and switched to Montague Grammar, where he showed how many generative semantics analyses could be recast and improved. Dowty also made one of the first major contributions to reducing the role of syntactic transformations with his idea in (Dowty 1978a) that all governed transformations might better be reformulated as lexical rules.

I moved to UMass Amherst in 1972, with appointments in both Linguistics and Philosophy (but with my primary home clearly in Linguistics), and Terry Parsons moved there at the same time, in Philosophy; in fact we encouraged each other to make the same choice, and submitted a joint grant proposal to NSF even before we got there. (It was turned down – they claimed that we couldn't vouch for the quality of the graduate students we would want as RAs --, but the next one wasn't.) I was still seen as, and saw myself as, a syntactician, with interests also in semantics. I remember being surprised that such a small department could consider hiring another syntactician, but they explained that they cared more about having faculty who could talk to each other and work together with the graduate students than having everything 'covered'. And Emmon Bach came in 1973[42]. Emmon soon started doing Montague grammar as well, and Terry had his own approach, similar in spirit, but in the end didn't publish his book-length manuscript (Parsons 1972) and joined us in working with students in Montague's framework. Sometimes with NSF grant support and sometimes without[43], Terry and Ed Gettier and Emmon and I and some of our graduate students[44] 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 as well as separately. The UMass department was very young then (departmental status approved in 1971), and one of the big events that helped to put it on the map was the 1974 Linguistic Institute, in which semantics and philosophy of language were major components, with courses, seminars, and workshops by top scholars from all over, and hundreds of students and visiting scholars. I think my Montague Grammar course had about 80 people in it, and I had grant support for a closed workshop on "The Semantics of Non-Extensional Constructions", whose participants, most of them also Institute faculty, included Emmon, Terry Parsons, David Lewis, Rich Thomason, Bob Stalnaker, David Dowty, Ray Jackendoff, Janet Fodor, Ed Keenan, Hans Kamp, Lauri

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Montague, dumfounded, had asked why would anyone think THAT!?, and I jumped up to intervene and gave a 1-minute account for Montague of why syntactic parsimony suggested deriving adjectives from relative clauses and a 1-minute account for Lakoff of why a uniform treatment of adjectives plus the existence of adjectives like *alleged* and *former* suggested deriving all prenominal adjectives *in situ*. Afterwards in the coffee break I got the nearest I ever got to a compliment from Montague when he said, "Barbara, I think you're the only linguist who it is not the case that I can't talk to." I'm grateful to Larry Horn for corroborating my (indeed vivid!) memory of that remark; it turns out he wrote it down verbatim in his negation notebook and cited it in a footnote in (Horn 2001): "Barbara, I think that you are the only linguist who it is not the case that I can't talk to."

[42] Emmon and I were married from 1973 to 1996. I say more about our joint role in the department in Section 2.

[43] The worst disappointment came when the proposal that would have supported Muffy Siegel's dissertation year was turned down at the same time that UMass had one of its budget crises, and Muffy ended up teaching part-time in Hartford while writing her dissertation (Siegel 1976).

[44] Dissertations from that period, in most of which Terry and Emmon and I were all involved, include Robin Cooper, Muffy Siegel, Greg Carlson and Paul Hirschbühler in Linguistics, and James Waldo and Dave Davis in Philosophy.

Karttunen, Michael Bennett, Enrique Delacruz, and two graduate students, Anil Gupta (Thomason's student) and Robin Cooper (mine).

So by the middle of the 70's, UMass linguistics was definitely "on the map" and UMass was pretty widely regarded as the center of formal semantics[45], and we and our students were making major contributions to developing versions of Montague grammar that could make sense as part of linguistic theory (Bach 1979b, Cooper 1975, Cooper and Parsons 1976, Partee 1975b). "Constraining Montague grammar" was one central theme in many of our seminars and in much of my work (e.g. (Partee 1976a, 1979b, 1980a)). Extending Montague's work to cover a wider range of linguistic phenomena had been a central concern from the very beginning (see the content of (Rodman 1972) and the titles of (Bach 1976, Bennett 1974, Partee 1973b, Thomason 1976)), and that continued, with work on relative clauses (Bach and Cooper 1978), tense and aspect (Bach 1980a, Bennett and Partee 1972), anaphora (Bach and Partee 1980, Parsons 1978), passives and control (Bach 1979a, 1980b).

And by the middle of the 1970's, Montague grammar and related work in formal semantics was flourishing as a cooperative linguistics-and-philosophy enterprise not only at UMass, but in some other parts of the U.S., the Netherlands, Germany, Scandinavia, and New Zealand, and among individual scholars elsewhere. (By the late 1970's it was no longer possible to keep track.) The first published collection, Partee (1976b), contained contributions by Lewis, Partee, Thomason, Bennett, Rodman, Delacruz, Dowty, Hamblin, Cresswell, Siegel, and Cooper and Parsons; the first issue of *Linguistics and Philosophy* contained Karttunen (1977) as its first article; the biennial Amsterdam Colloquia, still a major forum for new results in formal semantics, started up in the mid-70's and opened its doors to scholars from outside Europe by the late 1970's. Other conferences and workshops on or including Montague grammar were held in various places in the U.S. and Europe from the mid-1970's onward.

At the same time, there were foundational issues to worry about. On the one hand, Montague grammar was clearly having great successes in solving linguistic problems; on the other hand, there were principled objections coming from two different sides: linguists who saw the concentration on truth-conditions and entailment relations as at odds with the characterization of grammatical competence as something in the head of the native speaker, and philosophers who saw Montague's characterization of intensionality in terms of possible worlds as insufficiently intensional to cope with some of the problems of propositional attitudes. As the field of cognitive science got going in the late 1970's, helped by the support of the Sloan Foundation, issues such as these were often at center stage in discussions. I worried about these issues a lot in the 70's and 80's (Partee 1977b, 1980b, 1988a, 1988b, 1973d, 1979c, 1982), but mainly 'alongside' the actual 'doing' of semantics, except for work on the semantics of propositional attitudes, where the

[45] I associate Irene Heim with two different landmark indicators. First, when she had received a German fellowship to let her study anywhere in the U.S., she had written to Chomsky about coming to MIT, telling him she was interested in pursuing syntax and semantics. And he wrote back saying if she was really serious about wanting to do semantics, she should consider UMass Amherst instead, since there was still no semantics at MIT. So she entered our Ph.D. program in 1977. A later landmark, which for me was quite an emotional moment, in the light of Chomsky's long-standing skepticism about formal semantics and my feeling that he thought I had gone astray, was when MIT hired Irene as their first formal semanticist in 1989.

foundational issues have always seemed to me most squarely relevant. Those papers of mine were not among my most successful, and none of them are included in (Partee 2004); the last one to go, when tough choices had to be made within the page constraints for (Partee 2004), was (Partee 1988b); I still often give that one to students to read to get some idea of where possible worlds semantics came from and what some of the main controversies surrounding it are about. I still think these issues need more work, but I haven't been working on them actively myself in the last dozen years.

Formal semantics was still almost exclusively "Montague grammar" in the 1970s, and was mainly taught as an optional "seminar" topic. At UMass, for instance, the first semester Ph.D. courses were just syntax (6 credits) and phonology (the normal 3), with an introductory semantics course "Semantics and Generative Grammar", covering a range of approaches to semantics and its relation to syntax, as well as some classics in philosophy of language, as an optional course in the second semester. Montague grammar was taught as an optional "topics in semantics" seminar for students beyond the first year (or exceptionally well-prepared first-year students). I believe that it was not until the late 80's, after the hiring of Angelika Kratzer gave us semanticists, that we increased the first-year program to 12 credits and moved the first semantics course (by then an introduction to formal semantics) into the first semester and made it required along with syntax and phonology, with a second-semester semantics course part of a 3-out-of-5 menu of options, so only slightly less obligatory than second semester phonology and syntax. At that time, we also introduced a third semester "proseminar" (topic varying from year to year) in formal semantics as a bridge between the two first-year courses and the seminars; the same was done in phonology. Much later, in the late 90's, the three first-semester courses, which had been 6 credits of syntax and 3 each of phonology and semantics, were restructured to 4 credits each.

1.4. From interdisciplinary to disciplinary in the 1980's.

My work in the late 70's and into the 80's was a combination of continuing investigations within Montague grammar and interdisciplinary explorations in the emerging field of cognitive science, including interactions with philosophers, computer scientists, and psychologists. I think the height of interaction on semantics between linguists and philosophers had passed by 1980, followed first by the rise of cognitive science, in which semantics was one of the highly interdisciplinary concerns, and then by a greater specialization of semantics inside of linguistics proper, though always with many individual scholars maintaining links of various kinds within and across the disciplines. The Sloan grants in Cognitive Science, which had a big impact on the field, went from 1978 through the end of the 80's, and were the occasion for many interdisciplinary conferences, including two on semantics that I organized at UMass. One in 1978 was on "Indefinite Reference", where Irene Heim got the topic for her dissertation (Heim 1982).

Another workshop I organized a few years later showcased the new Discourse Representation Theory of Hans Kamp (Kamp 1981) and the new Situation Semantics of Barwise and Perry (Barwise 1981, Barwise and Perry 1981, Barwise and Perry 1983). For a time in the early 80's it seemed that Hans Kamp on the one hand and Jon Barwise on the other might be starting to play the kind of role Montague had played earlier, bringing their logical expertise to bear on rethinking the formal structure of semantic theory. Kamp's work on Discourse Representation Theory made a big initial splash as the same time as Irene Heim's dissertation; his work and hers have diverged since then, both highly influential, with Irene staying firmly focused on semantic theory within linguistics and doing a lot of important work on the semantics of a range of constructions, with particular attention to the formalization of presuppositions, the treatment of variables and variable binding, and the relation between syntax and semantics. Hans has branched out in a number of directions including interface with computational linguistic projects, and with considerable work on aspects of the lexicon that go beyond what many linguists consider linguistics proper. His work has generated continuing interest up to the present, and it continues to evolve and to influence the work of linguists and computational linguists. Barwise and Perry's work, on the other hand, while starting off from some very interesting ideas about "scenes" and "situations" as ontologically important categories to include in the foundations of semantics (potentially related to emerging ideas about the Davidsonian "event argument"), suffered from two problems that made it become less attractive to many of us than it seemed like it was going to be. Firstly, Jon Barwise had some deep aversion to possible worlds, which extended to 'possible situations', but the things they tried to do in the theory to avoid possibilia never seemed very promising, which was especially problematic since propositional attitudes was one of the topics they were interested in, as evident from the title of their eagerly awaited but disappointing book (Barwise and Perry 1983). And secondly, they never satisfactorily answered the Montague grammar community's questions about how they planned to treat quantificational NPs like *every student*; Montague had shown how it could be done with generalized quantifiers, and to propose a theory as a 'successor' to Montague grammar but not have any way of handling quantificational NPs seemed a step backwards rather than forwards. Some scholars have continued to develop Barwise and Perry's situation semantics, and certain of its ideas were readily borrowed into other approaches, but it soon became peripheral as a wholesale theory. Angelika Kratzer's very different conception of situation semantics, developed toward the end of the 1980's, with possible situations as parts of possible worlds, now has much more influence than Barwise and Perry's ideas.

When I organized the conference showcasing Kamp and Barwise and Perry, I think I still saw myself as a kind of guiding light in the field, helping to introduce the 'next big thing'; but really, in retrospect, I may have been excessively enthusiastic, especially about Barwise and Perry. (They had semi-facetiously printed up "bumper stickers" proclaiming "Another Family for Situation Semantics", and I happily glued one on my office door, but removed it shortly after the publication of their book.) Irene Heim and Emmon each used to chide me, probably appropriately, for sometimes putting my logician friends on some kind of pedestal and not recognizing the extent to which it was now linguists who were leading the way in developing formal semantics. And in any case, the field was already mature enough that my "endorsement" of anyone didn't have great effect; for that matter, it was not my "endorsement" of Montague that had an effect in the early years but rather the work that I put into making his formal theory more accessible and giving examples of its usefulness.

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

The period of the Sloan grants in Cognitive Science (late 70's to mid 80's) should have been just as wonderful as the earlier linguistics-philosophy interactions – they certainly supported lots of interdisciplinary conferences (which gave rise to the verbal expression "Sloaning around"). But somehow it wasn't quite as friendly and wasn't quite as productive, at least from my perspective. I can think of two possible reasons. One was the competition over domination of cognitive science, and the other was the competition for large grants. When the Sloan Foundation was in the early planning stages for supporting cognitive science, they held two preliminary discussion sessions with groups of relevant researchers, one on the East coast and one on the West coast. That was the year that Emmon and I were out at CASBS (1976-77), so I was invited to the West coast one. The East coast one had already happened, and stories about it were flying around. One of the things the Sloan people wanted to find out was whether there was already one institution in the U.S. positioned to be a major center, where large grants could be concentrated from the outset, or whether they needed to start with smaller grants to many institutions and work up through successive competitions to one or more larger longer-term grants. Apparently MIT had hopes of being designated as the major center from the start, but Chomsky and Minsky, while both in support of that idea, were so hostile to one another (each declaring that his own kind of work was the obvious cornerstone for the further development of Cognitive Science) that it was all too apparent that MIT was not the home for interdisciplinary cooperation that the Sloan Foundation was looking for. The West Coast group, which of course tended to think that there was as much potential for work in Cognitive Science on the West Coast as on the East, favored the idea of starting with many small grants and working upward from there. And that was what happened; so although we all did our best to participate cooperatively in all the many interdisciplinary conferences that followed, and enjoyed having postdocs and even new hires[46] with our Sloan money, the cooperation was tinged with competition, since each of the three successive rounds of grant competition was for a smaller number of larger grants. And perhaps more difficult to cope with constructively was the competition for the soul of cognitive science between the AI-centered community and the theoretical linguistics-centered community, foreshadowed in the early Minsky-Chomsky blowup. We had a bit of it even within UMass, where we successfully competed for grants in the first two rounds, the first with just Computer and Information Science (COINS) and Linguistics, and the second with Psychology included. (For the third round, I lobbied hard to get some philosophers included, which required convincing some philosophers as well as the other departments already in the program. It was tearing me apart to be wrapped up in interdisciplinary work that didn't include philosophy, where all my earlier interdisciplinary work had been concentrated.) Michael Arbib of COINS and I were co-directors of the program, and we were not natural bedfellows, but managed to get along for the sake of the program. I can very well

[46] Our own Lyn Frazier was hired in a position that was created with a Sloan grant. With our relatively small department , we would never have thought of adding adult psycholinguistics as an area without the incentive of the Sloan grant, which paid for the first few years of the position on condition that the University pick up the funding after that. Lyn is an example of the most successful side of the Sloan project, since she has been doing joint research with Charles Clifton and Keith Rayner in our Psychology Department ever since, and our students have been benefitting from that strength in our program, which for many years now has included a Psycholinguistics Training Grant. And Lyn's work has expanded into semantic processing, and the possibility of combining theoretical and experimental work in semantics is a great strength for our students and our department.

remember one seminar that I co-taught with Elliot Soloway, a young COINS faculty member in AI specializing in computer-assisted instruction, and Doug Moran, a Sloan postdoc who had received his Ph.D. at the University of Michigan under Joyce Friedman, working with her on computer models of Montague grammar. We tried to educate each other and our graduate students about the main issues in semantics from our respective perspectives. But the differences were much harder to bridge than any differences between linguists and philosophers; Elliot and Doug, for instance, had a shocking (understandable from their perspective, but shocking from mine) attitude toward logical consistency (with which Arbib agreed, I learned): any inconsistency that isn't derivable in n steps (where you get to choose the n) doesn't count as inconsistency because your program won't find it. I'm afraid that one of the main things I learned was the difference between AI and theoretical computer science. I'm much more compatible with somebody like Ray Turner, who came later, with whom I could discuss finite representability of infinite domains (which was what I had hoped to get some help with in that seminar), with shared assumptions about basic notions like entailment and consistency. (It's not that I'm totally dogmatic; Fred Landman has argued with me about the need for a less absolute respect for consistency, and I can listen to him because he knows what my background is and knows what kinds of arguments will count as arguments for me.)

Ray Turner was a quiet but important figure for me in the 1980's; he never tried to promulgate some special theory of his own, but I was very glad to get to know him and have a chance to learn from him. I first learned about him from Hans Kamp, the supervisor of his second Ph.D. dissertation, which was in philosophy and was about analyzing counterfactual conditionals without possible worlds – making use of sequences of finite approximations to possible worlds, approaching infinitude in the limit. Given my worries about finite representability and possible worlds, I was very interested in trying to understand more of Ray's work, which I couldn't read by myself. When Ray Turner visited UMass, first on a Sloan postdoc in cognitive science in 1981-82 and later when I had gotten an SDF grant for 1984-7 in which he played a major role, we organized a weekly evening Model Theory seminar with colleagues in philosophy, mathematics, computer science, linguistics, and psychology. Ray helped us learn about Scott-Strachey semantics for programming languages (and some of the tricks of finite approximations to infinite domains), and later Ray and Gennaro Chierchia began doing some of their joint work on property theory (Chierchia and Turner 1988) as a way to provide a more intensional foundation for semantics than set theory. We brought together more colleagues interested in the development of property theory and type theory in a conference which produced a pair of volumes of papers (Chierchia et al. 1989a, 1989b), but I don't think we really succeeded in bridging the gap between the logicians working on the technical issues and the linguists interested in semantic issues, much less drew the interest of the cognitive science community. Flexibility in type theory, which didn't need the high-powered logic of property theory, was an issue that was more accessible for more of us. If I could have had Ray as a permanent colleague, I would have tried to continue learning about property theory and thinking about the kinds of linguistic applications that Chierchia and Turner were developing. And I would have wanted to work on issues in ontological foundations and alternative logics for natural language semantics; but that was not the kind of thing I could work on without a logician colleague with similar interests. And there was plenty of interesting work to do within linguistics.

Emmon and I spent 1982-83 at the Max Planck Institute for Psycholinguistics in Nijmegen, not doing psycholinguistics ourselves but interacting with the psycholinguists there (particularly Emmon, who got involved in issues of parsing Dutch vs. German embedded constructions which show word order differences that are relevant to debates about the possible context-freeness of natural language). While we were there, we attended semantics seminars in either Groningen (where van Benthem was at that time) or Amsterdam (Groenendijk and Stokhof and their colleagues) just about every week. Formal semantics was a vibrant enterprise in the Netherlands, and the Dutch original of the wonderful textbook (Gamut 1991)[47] was published in 1982. (That two-volume textbook, which combines an introduction to logic with an introduction to formal semantics, was a beginning of an answer to one of my dreams, that lambdas should be introduced in introductions to logic, and not left only to advanced special topics, or to be introduced in semantics courses. It was partly at my enthusiastic urging that the book was translated into English and published in the U.S.) It was a great experience to interact so closely with our Dutch colleagues for a year; those ties have remained strong, and Holland is one of the countries where I continue feel very much at home. I used the biennial Amsterdam Colloquia, which I first attended in 1980, as the occasion for presenting my major research work for the whole decade of the 1980's; at that time it was the foremost international conference in formal semantics. Fred Landman's[48] time at UMass on my SDF grant strengthened the connections. Starting in the 90's, when van Benthem had moved to Amsterdam and the major forces of formal semantics in the Netherlands were all concentrated there, a split began to develop between the focus in Amsterdam-centered formal semantics and that centered in the U.S., with the Amsterdam group developing more of the logical and mathematical side and U.S.-based formal semanticists working more within linguistics. I'll continue discussing these developments in the next section.

In the meantime, there were important developments within linguistics proper. While some of us were trying to figure out how to integrate Montague grammar with transformational syntax, others had the idea that the availability of a powerful semantics might make it possible to consider an even more radically constrained syntax, with no transformations at all. Several developments in this direction began around the very end of the 70's and took off as a major direction of research in the 80's: especially GPSG (Gazdar 1982, Gazdar 1983, Gazdar et al. 1985), categorial grammar (Bach 1981, Bach 1984, Bach 1987, Bach et al. 1987, van Benthem 1986, van Benthem 1987), and the categorial-grammar-influenced successor to GPSG, HPSG (Pollard and Sag 1994). I have always felt very sympathetic to such work, although I haven't been an active contributor; see more discussion in (Partee 1996).[49]

The liveliness of the 'straight linguistic semantics' that was moving ahead at full speed all this time is reflected in the range of UMass dissertations that I supervised that were

[47] The Gamut textbook 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. It was partly at my urging that the book was translated into English.

[48] When I first met Fred in Amsterdam in 1980, he was the first student I had met anywhere who had learned Montague grammar before learning anything about generative grammar.

[49] GPSG = Generalized Phrase Structure Grammar; HPSG = Head-Driven Phrase Structure Grammar. LFG, Lexical-Functional Grammar (Kaplan and Bresnan 1982), had a separate history, and was provided with a formal semantics by Halvorsen (1983); it is in a sense part of the same broader community of 'mono-stratal' (non-transformational) theories.

completed in the 1980's or right after: Elisabet Engdahl (1980), Michael Flynn (1981), Ken Ross 1981, Irene Heim (1982), Gennaro Chierchia (1984), Mats Rooth (1985), Jonathan Mitchell (1986), Craige Roberts (1987, 1990), Nirit Kadmon (1987), Jae-Woong Choe (1987), Allesandro Zucchi (1989), Karina Wilkinson (Wilkinson 1991). Emmon was involved in all of these as well, as well as chairing (Jones 1985, Miyara 1981, Stein 1981), and since Terry Parsons had left UMass in 1978, Ed Gettier was the outside member on many of them. Hans Kamp was at UMass briefly in the mid-1980s, and had an influence on the work of Roberts and Kadmon, as well as having already influenced some of my own (Kamp and Partee 1995, Partee 1984b); and the big rethinking of the semantics of quantificational and non-quantificational 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.

As the number of dissertations may suggest, by the middle of the 1980's the department felt the need for an additional faculty member in semantics, and we were able to hire Angelika Kratzer in 1985. It was soon after that that the revisions to the curriculum mentioned earlier were developed. The increasing recognition of semantics as part of the core curriculum in linguistics was seen in the growing number of departments with more than one semanticist, and a few, like ours, with more than two by the end of the decade. As for textbooks, the classic (Dowty et al. 1981) had a monopoly in this country. There were several textbooks on Montague grammar in Germany, of which one of the most interesting because of its nice exposition of the algebraic interpretation of the compositionality principle was (Link 1979).

My description of this period in the field and in my work is brief, but developments in this period are well documented in other places, and my own work of this period is well represented in (Partee 2004).

1.5. Recent years

By the beginning of the 1990's, formal semantics (no longer called Montague grammar, since so many developments had gone in so many directions) was clearly a fully formed field within linguistics, and students were not conscious that there hadn't always been 'phonology, syntax, semantics' as core fields. Several semantics textbooks were published around 1990, including (Bach 1989a, Chierchia and McConnell-Ginet 1990, Gamut 1991), and the number of departments with at least one or two formal semanticists increased in the 1990's; even MIT had two after they hired Kai von Fintel in 1994.

By the 1990's, there was noticeably less direct interaction between linguists and philosophers in semantics, in part because within philosophy interest in the philosophy of language had declined as interest in philosophy of mind increased. There was also some divergence between directions in formal semantics in the Netherlands and directions in the U.S., which previously had been very close. The founding in the late 1980's of the predecessor of the interdisciplinary ILLC (Institute for Language, Logic, and Computation) at the University of Amsterdam led to the founding in 1990 of the European Foundation for Logic, Language, and Information (FOLLI) which sponsors a new journal of the same name (*JOLLI*) and annual summer schools (ESSLLI) that always include courses on the latest developments in formal semantics. Within that framework, linguistics is just one contributor to the semantics enterprise, and perhaps subordinate to logic and computation.

In the U.S., on the other hand, there has been a move toward putting semantics more firmly inside linguistics. After fifteen years in which *Linguistics and Philosophy*, founded in 1977, was the preeminent journal for formal semantics, a new journal conceived and edited by Irene Heim and Angelika Kratzer was launched in 1992, specifically aiming to integrate formal semantics more closely into linguistic theory, as suggested by its name, *Natural Language Semantics*. Heim and Kratzer are also the authors of what one might call a fully post-Montague textbook in formal semantics, (Heim and Kratzer 1998). And the conference and the American conference series SALT (Semantics and Linguistic Theory) had its first annual meeting at Cornell in 1991. What is new in both the journal and SALT is that it is permitted to presuppose that the audience knows some contemporary theory of syntax. Whereas articles in *Linguistics and Philosophy* are in principle supposed to be readable by both linguists and philosophers, that is not a requirement for *Natural Language Semantics*, nor for SALT talks. So one can see an increasing specialization into more logical, computational, and linguistic aspects of formal semantics, albeit with continuing overlap and interaction.

It may be part of the same trend that the NSF project on quantification which Emmon, Angelika and I had from 1988 to 1992, with a companion grant for a workshop at the Linguistic Institute in Arizona in the summer of 1989, was the first non-interdisciplinary project I had been involved in since the Stockwell, Schachter and Partee syntax project in the late 1960's. The new direction in that project was cross-linguistic, looking at the way quantification is expressed in different languages and at the similarities and differences between familiar determiner quantification and different sorts of adverbial quantification. The generation of graduate students involved in that project at one time or another included some whose dissertations I supervised, Paul Portner (1992), Virginia Brennan (1993), Hotze Rullmann (1995), and others supervised by Angelika (Berman 1991, Diesing 1990, Schwarzschild 1991, von Fintel 1994).

In the U.S., where there is a great deal of emphasis on the interface between syntax and semantics (which is after all central to the compositionality principle) there has developed some difference between groups and individuals who work with some sort of Chomskyan (broadly speaking) syntax, led by Heim and Kratzer, and groups or individuals who find some of the non-transformational approaches to syntax more appealing (extended categorial grammar, LFG, HPSG, or TAG). I think Heim's 1982 dissertation was one of the first works in formal semantics to start from a transformational syntax with "rules of construal" transforming S-structures into logical forms, which served as input to semantic interpretation, and Heim did a great deal to show how to modify and make semantic sense a notion of logical form (or LF) in a Chomskyan approach. Communication is still fairly good across these lines, with all approaches represented at conferences like SALT, but it's safe to say that approaches using Chomskyan syntax are better represented in *Natural Language Semantics*, while non-Chomskyan approaches are more often seen *Linguistics and Philosophy* as well as among most (thought not all) computational linguists. Although I started out as a syntactician, I haven't been able to call myself a working syntactician for some time. In the 1970's I still co-taught graduate syntax courses as well as teaching introductory and advanced undergraduate syntax, but by the late 1980's I had been eased out of even the undergraduate courses (where by the mid-1980's I was introducing students to GPSG as well as the obligatory transformational grammar, which I no longer had much enthusiasm for.) Since then I have tended to declare myself agnostic about syntax, largely in order to co-exist compatibly with my mainly Chomskyan colleagues in the department and to work with students whose syntax has almost always been Chomskyan (unless they were working with Emmon, who helped mentor those who were interested in non-transformational approaches.) But I have sometimes regretted that we don't have non-transformational syntactic theories represented in our department[50]. On the one hand, I myself feel more attracted to non-transformational approaches, and I've been sorry not to be part of a community of colleagues working with theories of that kind. On the other hand, I wouldn't wish for a department such as SUNY Buffalo once aspired to be, with "one of everything", one representative of every competing theory – I would rather be in 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, than in one that had more different theories but less productive interaction among colleagues. But still I wish I knew more ways to help our students learn something about the existence and potential attractions of approaches that aren't instantiated among our own faculty.

During the 1980's, I developed ties[51] to the Prague school, particularly Eva Hajičová and Petr Sgall, and when Emmon and I had a sabbatical year in 1989-90, with the kids grown up and gone, we no longer had to choose a sabbatical location good for the whole family, and we took the opportunity to follow up on some of our separate interests. Emmon spent the year in Kitimat, British Columbia, doing fieldwork with the Ha'isla tribe, and I spent the fall semester in Prague and the spring semester in Mexico, where I also have a good friend and colleague, Josefina García Fajardo. In Prague in the fall, besides living through the Velvet Revolution together with my colleagues (an event of a lifetime, but not a subject for this Essay), we made a start on connecting Praguian work on topic-focus structure with formal semantics work on quantification, building especially on work of Irene Heim and of Mats Rooth, later also of Kai von Fintel. With the help of one of the first joint Czech-U.S. Science and Technology Development grants, awarded to Eva and me[52], we continued that work over several years, bringing Christine Bartels to Prague over three summers and several other UMass students for one or two summers, and bringing Eva and Petr and their colleagues Jarmila Panevová and the philosopher Jaroslav

[50] Emmon did manage to give occasional seminars on extended categorial grammar; but our MIT-oriented syntactic colleagues always referred to those as semantics seminars, much to Emmon's (and my) chagrin.

[51] The seeds for these ties were planted in 1974, when Eva Hajičová and I got acquainted at a summer school organized by Antonio Zampolli in Pisa and felt a great mutual affinity. Eva and her colleague Petr Sgall proceeded to send me annual Christmas cards asking when I was coming to Prague, and I finally managed a one-week visit in 1981, followed by a two-week stay in 1985, after which I resolved to spend the next available sabbatical semester there, which I did in fall 1989, and we've kept close ties ever since.

[52] Applying for that grant was one of my first experiences in the wonders of the changing world of technology. When I was in Prague in the fall of 1989, Prague had no internet connections to the U.S., and telephoning the U.S. was still quite a complicated business. But by the spring of 1991, when Eva got word of the existence of this new grant opportunity only about two weeks before the deadline and with a requirement of documents signed by the administrators of both our universities, the availability of fax and internet made it possible for us to get a good grant proposal prepared in two languages, approved by both administrations, and submitted by the deadline. The fact that we had already been trying to find sources of support for a joint project meant that we were all ready to put together a proposal, and that may be one of the reasons we succeeded, since anyone trying to start from scratch to develop an international collaborative proposal on such an impossible deadline would hardly have had a chance.

Peregrin[53] to the U.S. for working visits. Three of the UMass students who were involved in the project organized a project-related conference on Focus in December 1995 at UMass, with Eva and Petr and me invited to give a keynote talk, and edited the papers from the conference into a UMOP publication (Benedicto et al. 1998). I had another sabbatical semester in Prague on another IREX grant in spring 1995, and Eva and Petr and I eventually completed a book (Hajicová et al. 1998) on our joint work. That joint work was partly frustrating because of many deep differences between our theories that made certain kinds of questions impossible to work on together. But it was also very rewarding in many ways, and I believe we made some real progress both on understanding the interaction between quantification and topic-focus structure as well as gaining a better understanding of the goals and limitations of both our own and each other's approaches and the beginnings of a good explicit comparative description of our theories. I have visited Prague just about every year since that first sabbatical semester, in recent years usually to give a lecture series in the Mathesius Institute, a two-week institute organized by Eva and her colleagues once or twice a year for students from post-communist countries.

My semester in Mexico has not led to long-term collaborative work, even though I could imagine such possibilities in another possible lifetime. There is an interesting range of work going on in anthropological linguistics, ethnolinguistics, typological studies, philosophy of language, semantics, and other areas of linguistics. One striking thing about the linguistic scene in Mexico was how little the Mexican linguists worked collaboratively in the realm of ideas. This became clearest to me when I was reading and discussing papers on the semantics of determiners in Spanish by two young semanticists, and I asked them (separately) why they weren't making more reference to one another's work. I got the surprising reply that they considered it easier to remain cordial if they didn't, and that it was in fact considered slightly aggressive to read and comment on a colleague's work. I have to confess that I was shocked, and realized that I couldn't really plant roots in an academic community that lived like that. I was also surprised to learn that it was absolutely forbidden for Ph.D. students to publish as articles anything they might want to use in their dissertations; the work in the dissertation had to be absolutely new, none of published anywhere before. That seems to me a much less productive policy than our policy of encouraging students to work up their ideas through seminars, generals papers, conference papers, and sometimes also journal papers on their way to their dissertations. Mexico seemed to me in many ways a place with unfulfilled potential. A few linguists mentioned how good it could be to establish an analog of the LSA's Linguistic Institute in the alternate years, when there isn't one in the U.S., in one of the cooler and prettier parts of Mexico such as Oaxaca, and indeed I would think that such a program could be wonderful in many ways both for Mexican linguistics and for the international community (not least because of the fantastic wealth of diverse indigenous languages in Mexico), but I don't see

[53] I've known Peregrin since my second visit to Prague in 1985, and helped him get hold of books by western philosophers like Hilary Putnam when such books were inaccessible in Communist countries. He was a good example of an aspiring young philosopher who couldn't really "be" a philosopher under Communism because of the prevailing Marxist-Leninist dogma dominating such fields. He survived by a combination of a day job as a computer programmer and an academic home within the linguistics group headed by Sgall and Hajičová, doing his philosophical work and thinking at home on his own until after the fall of Communism, when he was able to start teaching and working as a real philosopher. Linguistics, logic, and computer science often served as "shelters" in the Communist countries for many who would otherwise have preferred to have a home in philosophy or other humanistic disciplines.

any signs of the collective will to make it happen[54]. Such experiences heighten my admiration and respect for the energy and skill of those people who have made comparable institutions come into existence and thrive elsewhere, like the Mathesius Institutes in Prague, and the summer schools generative linguistics held in different places in Eastern Europe over the last dozen years or so, and the big linguistic fieldwork expeditions to the Caucasus and elsewhere organized for the last 35 years under often extremely difficult conditions by Aleksandr Kibrik[55].

After some life changes in the mid 1990's[56], I began to spend half of every year in Moscow, with the new "mission" of building bridges between lexical semantics, which has been the major focus of Moscow-school semantics, and western formal semantics; this collection contains one work representative of this new direction, (Partee and Borschev 2001). Getting better acquainted with Russian lexical semantics has also caused me to read more of those western works that are more popular in Russia, including various approaches to lexical semantics and work in functional and cognitive approaches and in Construction Grammar. Bridge figures respected on all sides include Fillmore, Talmy, Beth Levin, Östen Dahl, Karttunen, Jackendoff, Bierwisch, Haspelmath, Bybee, Croft, McCawley, and figures in related areas such as Comrie, Jerry Fodor, Vendler, Quine, Searle, Hintikka.

It turns out that the term "lexical semantics" means many different things to different people. Within the Moscow tradition, lexical semantics evolved from a very rich tradition of real dictionary-making, and represents the attempt of theoretical linguists to construct a more scientific foundation for lexicography. Kinds of questions that are important include how to find criteria for dividing a polysemous word into a particular number of distinct lexemes, and how to organize and systematically formulate the various "components of meaning" that make up the full meaning of each lexeme. Apresjan, the central figure in the Moscow school, is quite skeptical about the project of abstracting out a single core meaning for a word and deriving its full range of meaning via general principles, a common goal in various western approaches. Vladimir Borschev and I, in order to make the gap with formal semantics smaller, construe Apresjan's statements of "components of meaning" as meaning postulates, structured in particular ways. We see formal semantics and Moscow school lexical semantics as in principle compatible but each focussed on parts of a larger task that are neglected by the other. Formal semanticists take lexical meanings as inputs to their compositional rules, and study the "innards" of lexical meanings only insofar as certain properties of lexical meaning interact crucially with semantic composition – such as properties involving argument structure or aspect. Moscow school

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[54] And I recently learned that the 17th International Congress of Linguists was scheduled to take place in Mexico in 2002, but had to be moved and postponed because planning went poorly.

[55] Kibrik's expeditions, primarily for Russian students and young colleagues, with occasional international participants, are described in my husband's expedition diaries (Borschev 2001). Related English-language publications include (Kibrik 1977, Kibrik 1996). I had hopes of going on one of the expeditions to Dagestan, but before a possible occasion came in 1998, Dagestan became too dangerous; having a Westerner along (with presumed kidnap-ransom potential) could have actually endangered the members of the expedition that year, we learned afterwards from Kibrik.

[56] I married Vladimir Borschev in 1997. Since 1996, we have spent half of each year in Moscow and half in Amherst, with occasional exceptions such as a semester in Leipzig in 1999 and a full year in Amherst in 2001-02. His background was in mathematical linguistics and the formal semantics of programming languages, and he moved into natural language semantics through long professional association with Elena Paducheva at VINITI, an institute of the Russian Academy of Sciences, and through joint work with his late wife Lidia Knorina.

lexical semanticists consider "combinatory potential" an important aspect of lexical meaning but have done much less work on that side of lexical meaning than on fine-grain studies of similarities and differences between lexical items in a given semantic field. We believe that each of these approaches could supplement and enrich the other, and are proceeding to find projects to work on where we believe the interaction of lexical and compositional semantics is crucial, such as the semantics of possessives in combination with relational and non-relational nouns, and the semantics of the Genitive of Negation in Russian.

Moscow is like the Netherlands in having what seems to be a huge amount of semantics per capita, and seminars of one sort or another to go to just about every week. For historical reasons, it is a somewhat more insular world, but with freedom to travel and internet, that is changing, and young people are eager to learn about western approaches, while, thankfully, mostly not abandoning the many great strengths of their Russian linguistic heritage. (Of course there is considerable emigration, but not quite as much as one might have feared.)

When I first began spending only half of every year at UMass, students stopped asking me to chair their dissertations; it took some time to convince myself and everyone at UMass that I could stay involved with students during my off semesters via the internet. I am now[57] chairing three dissertations in progress (J. Michael Terry, Meredith Landman, and Ji-yung Kim), as well as serving on a number of recent and current committees (Jo-wang Lin, Christine Bartels, Satoshi Tomioka, Eugenia Casielles (Spanish), Adriane Rini (Philosophy), Maribel Romero, Winfried Lechner, Bernhard Schwarz, Kiyomi Kusumoto, Elisabeth Villalta, Marianella Carminati, Ana Arregui, Marcin Morzycki, Paula Menendez-Benito).

What's my place in the field now? As Theo Janssen noted when he read an incomplete draft, the writing is a bit drier and more simply factual once I finish describing the 70's. And I suppose that's because by the end of the 70's, the field was established, and I could just be a normal participant in a normal field – I was no longer shepherding it into existence and serving as a hub of activities, and UMass, while still a leading institution in the field, was happily one of an increasing number of institutions with lively semantics programs, several of them led by our Ph.D.s. And in fact I remember feeling an initial bit of shock when Alice ter Meulen and Johan van Benthem asked me to write a "historical" article about Montague grammar to serve as the lead article in their Handbook of Logic and Language (van Benthem and ter Meulen 1997) – I think my first reaction was "What do you mean, historical?" But then I realized that the relation of Montague grammar to the field of formal semantics as a whole had indeed become a historical relation. Students rarely study Montague grammar in its original form, just as they no longer study *Syntactic Structures* or *Aspects*. And over the last decade I've been asked to write several encyclopedia entries on Montague grammar; and as I mentioned above, "Montague grammar" has just made it into the Oxford English Dictionary, a sure sign of historical status. So I have come to accept that in the history of the field as a whole, I'm now a

[57] "Now" was Feb 2003, and I haven't tried to revise this section. Now in Feb 2005, Mike Terry and Ji-yung Kim have finished their Ph.Ds, and the two dissertations in progress that I'm chairing are Meredith Landman and Uri Strauss. And add to the list of recent or current committees that I'm on: Luis Alonso-Ovalle and Shai Cohen. And add three Russian students in Moscow whose completed or in-progress "diploma thesis" I'm co-director of: Julia Kuznetsova, Lisa Bylinina, and Igor Yanovich.

historical figure[58]. And with respect to contemporary semantics, I'm one researcher among many, continuing to do whatever happens to interest me most at the moment. Right now I'm completely fascinated by the multi-faceted problem of the genitive of negation in Russian, which I first worked on in graduate school. It seemed a nice straightforward problem back in the early 60's when it was right for rules to be explicit and stipulative and, more importantly, when I had access to only a very simplified view of the data, so a nice clean rule seemed quite adequate (genitive when c-commanded by a negative morpheme and [-specific], accusative otherwise, or something like that. I don't remember anymore exactly how I did it, given that we didn't yet have any analog of scope or quantifying in, but I think that must be related to why I thought that for the English *some-any* alternation I would need at least two *some*'s, one [+specific] which would stay *some* and which contrasted with *others* and one [-specific] (the one that's pronounced *sm*) which would change to *any*. But in any case, back then, when everything was seen as a syntactic problem, it was presupposed that solutions would take the form of nice clean explicit rules, and I couldn't dream that I would now see the genitive of negation as a great challenge for studying the interaction of syntax, semantics, lexical semantics and diathesis shift, topic-focus structure, presupposition, some still partly mysterious notion of perspective structure (Borschev and Partee 2002), and contextual influences.

The field of formal semantics itself is fully international, stronger in some places than others, quite heterogeneous but still collegial and friendly. No one figure and no one department is dominant, there are developments of many different kinds coming from many different places, and I probably couldn't even list all the journals in which work in formal semantics appears now, nor all the available textbooks. And that's great.

2. General reflections.

2.1. Social dynamics and personalities.

Along the way in the historical reflections in section 1, I have had occasion to mention the environments I've worked in, and it's probably clear how important I consider the "social dynamics" of institutions and people in the history of a field. Here I will generalize a little bit about some of those factors.

For instance, I think it has made a big difference to the fields of syntax and semantics that Chomsky has remained a living and productive and dominant figure in the field of syntax for more than forty years, while Montague, whose work at the end of the 60's was as revolutionary for semantics as Chomsky's at the end of the 50's was for syntax, died only a few years into that part of his life's work, and the further development of the field of formal semantics has not been dominated by any one person. One result has been that syntax has tended to be defined in relation to Chomsky for all this time: the theories[59] have changed as Chomsky's ideas have changed, but people, and whole departments, tend to be Chomskyan or anti-Chomskyan. (And even that is too simple; there is documentation

[58] I guess I should have seen 'historical' status coming with the conferring of various recognitions in the 80's – the presidency of the LSA (1986), honorary doctorates from Swarthmore (1989) and Charles University (1992), election to the American Academy of Arts and Sciences (1984) and the National Academy of Science (1989).

[59] I don't know how to refer concisely to "syntactic theories in the Chomskyan camp"; I could ironically just say "The Theory" in allusion to students who simply say "the theory of syntax" meaning whatever they take to be the current Chomskyan theory.

already in (Newmeyer 1980b) of the complex relations successive generations of Chomsky's students have had to Chomsky and to one another as on the one hand they[60] strive to establish their independence and on the other hand Chomsky keeps changing his theories 'out from under them'.) And MIT has remained the dominant institution in syntax ever since the 1960's. It's also part of the social dynamics of the field that Chomsky values theoretical argumentation above all else, and the sense of some things being "Interesting with a capital I" that I picked up on in graduate school hasn't changed: I had to learn not to say too publicly that my reason for being in this field is just that I think it's fun, and I had to learn to slightly self-censor my choice of problems to work on to be able to argue that there was some ulterior theoretical importance to such-and-such a problem. Actually, I suppose I have internalized that last part by now. It's hard to tell whether it has become part of my own internal make-up, or whether I have simply learned to look for theoretical relevance, but I certainly do look for it now, both for my own sake in writing grant proposals, and in advising students in their choice of topics and in writing referee reports for conference abstracts, journal articles, etc. In any case, it has seemed to me that syntax has been a more aggressively competitive field than semantics, and that semanticists tend to debate their differences in a more friendly and constructive way than syntacticians do. I'm not positive that that's true, and I'm not sure how much of it is due to personalities. But I think it's at least partly true, and at least partly due to Chomsky's continuing influence as a leading figure in the field and as a role model in a rather aggressive style of argumentation[61].

Because Montague died before his theory had any impact on linguistics, it was left to others to carry on his work, and that developed in a very cooperative spirit. Because of where I happened to be, and the fact that I had just come to the conclusion that his work was indeed important for linguistics, it was natural that I played a major role at the beginning. And as I mentioned earlier, I'm not comfortable with a combative style, and much happier in a cooperative environment like that of the very first seminar in Montague grammar at UCLA, where the logic and philosophy students helped the rest of us understand Montague's logic, and the linguists helped the logicians and philosophers appreciate the importance of systematically connecting the semantics with an independently justifiable syntax, and the philosophers helped with issues like intensionality and theories of demonstratives and indexicals. And as I began to understand Montague's work better and became more convinced than ever that it had a lot to offer for linguists, I

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[60] I should say "we"; I'm not immune from sensitivity to where I stand vis-a-vis Chomsky, as these remarks have undoubtedly betrayed. When I went to UCLA, I turned down a job offer to stay at MIT; I figured I needed to gain greater independence before I could imagine teaching at MIT without regarding the senior faculty as my teachers. But I fully imagined that I would get another job offer there later, and I confess to considerable disappointment when it turned out that Chomsky thought that my work in Montague grammar was not an enrichment of generative grammar but a misguided 'going astray'. That is of course a large part of why it was such an emotional day for me when MIT offered Irene Heim their new semantics position. But in the end I'm not at all sorry with how things have gone; the idea of having a top-rate and congenial linguistics department in a lively academic and cultural environment in a beautiful part of rural New England was and is my idea of Utopia.

[61] It's with slight embarrassment that I have reread the review I wrote while I was a graduate student (Hall 1964), although I have to say in my defense that the MIT slash and burn style was exacerbated by Bernhard Bloch's editing: he was an autocratic editor (I never got to see proofs), and among other things, he had no use for hedges, and removed all of my modals and "it would seem that", etc., which I think would have softened the tone at least slightly.

gave a lot of talks about it and wrote articles about it, and about how it could be put together with transformational grammar, and I think the tone was mostly one of "look how we can now resolve some of the difficulties that have been dividing generative from interpretive semanticists, and can have a theory that has the most important properties that each side cares about." Of course that is a way of arguing against those theories, but respectfully.

The field continued to develop in a friendly and cooperative manner; debates about how best to extend Montague's work in new directions were rarely polemical. Different linguists suggested different kinds of syntax to use with Montague's semantics; and the joint paper by Terry Parsons and Robin Cooper (Cooper and Parsons 1976) showed how one might design grammars in the style of Generative Semantics or Interpretive Semantics that would be provably equivalent to Montague's PTQ, not taking any stand on possible advantages of one formulation or another. When Barwise and Perry came on the scene in the 1980's, I was slightly taken aback when, after the receipt of their big SDF grant to establish the Center for the Study of Language and Information (CSLI) at Stanford, they seemed not to be just offering some new ideas but to be trying to establish some "alternative" center of gravity and to reject much of what seemed to bind the rest of the formal semantics community together. Hans Kamp was maybe a bit similar with respect to liking the idea of a community of researchers working with him on the development of his own DRT theory, which is fair enough; but he has always stayed a constructive member of the larger formal semantics community.

And within the UMass department, Emmon and I were very similar in wanting to foster a friendly and cooperative community of colleagues and students[62]. Both of us always gave our students great freedom in what to pursue and how to pursue it, and very few of our students' dissertations can be said to be in the mold of working out their "supervisor's" ideas; virtually all of them are highly independent, and our role has been rather that of mentor and sounding board and friendly critic. That approach, natural to most of our faculty, has been a strength of our department, especially since the graduate students themselves have been so able and independent. A tradition of that kind is happily self-propagating, since we then attract the kind of students who thrive in such a program. And it seems that not only our own Ph.D.s but those from other programs as well have helped to give the field itself a cooperative and friendly spirit. I don't want to sound overly pollyannish, though; of course there are serious disputes and not always friendly. But I think most semantics conferences are a real pleasure, and people listen to each other, and if someone seems to have made some kind of an error, and not just willfully, others will usually try to argue for a correction without anyone embarrassing anyone.

[62] As the two most senior members of the department as well as the two senior semanticists, Emmon and I felt a lot like the "Mom and Pop" of the department; Emmon was department head from 1977 to 1985, and I was head from 1987 to 1993. Some of the alumni of the department say that we did a lot to help give the department its character as a very friendly, close-knit and cooperative community, almost like a family (though not everyone likes that image, I have learned). Naturally, such a spirit depends on the whole department, and I think we've been very lucky through the years. It's true that Emmon and I both cared a lot about that aspect of the department, but so had Don Freeman and Adrian Akmajian and Dick Demers before us, and so have many of the rest of the faculty since then, not to mention the importance of Lynne Ballard and Kathy Adamczyk and their constancy through so many years, and the wonderful graduate students themselves.

There's another important property that our department has that helps make it a good environment for working and a good environment for our graduate students, which I might not have realized wasn't universal if it weren't often remarked on. And that is the kind of feedback we give to our students (and to each other) on their papers and their presentations: we are a "very tough audience" but a very supportive one. When students do a practice presentation in advance of a conference, they get tough questions and challenges, but also all kinds of constructive suggestions, so they can then go and face a public audience with some confidence. Similarly, it's a tradition that we don't let a student defend their dissertation until we're sure they can pass the defense, but then they also understand that in the defense we can ask any question we like and will often come up with unexpected tough ones – which is not traumatic because of the prior understanding, and in fact having the whole committee focussed on the dissertation at the defense often leads to interesting interchanges and sometimes to substantial new ideas.

And we do the same for one another, reading each other's papers or sitting in on each other's seminars.

Here's an anecdote that relates to the open and collegial spirit in the department. When Mike Flynn was just finishing up, around 1980, he and I went to out for a farewell lunch (he had been my "personal R.A." (an out-of-pocket jack-of-all-trades position) for several years). During lunch he said, "I want to ask you what's probably a really stupid question that I've been curious about for a while. You and Edwin Williams – it has always seemed to me that your theoretical frameworks are at odds with one another, and yet you seem to get along very well and never dispute each other's frameworks. Are your frameworks really compatible in some way that I just don't see, or are they indeed incompatible but you just don't bring the subject up for some reason?" And I thought about it, and realized that if he wondered, probably other students did too. And I told him my half of the answer, and then went and checked with Edwin, who indeed had the same answer. Namely, each of was indeed skeptical about the other's framework, though each of us recognized that the other was doing very good specific work within his/her own framework. And neither of us wanted to do a kind of "shoot from the hip" criticism[63], trying to give arguments against the other theory without really understanding it well, but because of our skepticism, neither of us wanted to put in the effort to really understand the other theory. So we kept quiet. Well, having realized the questions that raised in our students' minds, we immediately decided to co-teach the introduction to semantics the following spring, and to include at the end of the course a mini-introduction to Montague grammar (which in those days came only in a second-year fall semester course), a mini-introduction to "logical form" within the "Revised Extended Standard Theory" (Chomsky's theory at the time), followed by two or three weeks for debate, in which we would answer each other's questions and challenges and would jointly try to identify some empirical prediction on which the two theories disagreed. (We were in general agreement about linguistic and philosophical literature to include as general background in semantics for all our students; that wasn't a

[63] Chomsky had been guilty of such a kind of uninformed criticism of Montague grammar in (Chomsky 1980), criticizing Montague's uniform interpretation of NPs as generalized quantifiers on the basis of differential behavior of *John, a soldier, every soldier* with respect to possible discourse anaphora across sentences, constructions that were outside of Montague's fragments and about which his theory made no predictions one way or another. Bach and Partee (1980) was in part a response to Chomsky's criticism, offering proposals for extending earlier work in Montague grammar to deal with reflexive and non-reflexive pronouns and to distinguish bound variable anaphora from coreference.

problem.) So we did that, and immediately discovered misconceptions that each of us had about the other's theory. For instance, Edwin and presumably many other syntacticians thought that compositionality should require that a "syntactic operation" like Subject-Aux inversion should have a constant semantic interpretation, which it pretty clearly does not. The distinction in Montague grammar between 'syntactic operation' and 'syntactic rule' was not widely understood outside of formal semantics proper, it turned out; and now that so few people study Montague grammar *per se*, it is probably not well known even by students of formal semantics now. And we also discovered that each theory had big holes in places that the other theory considered important, so we had to fill in the holes with our own hypotheses on the fly in order to continue to try to make comparisons. By the end we had identified one sentence type, illustrated in (5) below, about which our theories, as provisionally supplemented, made opposite predictions[64].

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

And when we asked the students in the class for their judgments about whether control was possible in such a sentence, that is on whether the sentence is well-formed, with the "whom" individual controlling the implicit subject of the infinitive, the class split 50-50. (We should have written down their names, to see who then went into semantics and who into syntax!) Well, that's the amusing part; the great part was being able to have such a discussion and debate for the benefit of the students (and for each other's enlightenment as well) in a completely non-polemical spirit. One of the students taped the whole thing, and someone did a rough transcription, and I'm only sorry we never managed to turn it into a coherent written dialogue.

Let me add a word about interaction with philosophers, which was very important in the 60's and 70's. David Kaplan once articulated his reaction to linguists in a comment to Emmon and me after a conference: "Linguists are like vacuum cleaners! Philosophers are rather like black holes. Philosophers react to every theory by constructing arguments against it. Linguists react to every theory by taking it in and using it to explain some of their millions of examples." Actually, aside from the fact that what vacuum cleaners normally take in is dirt, we rather liked the analogy. And it was clear that while linguists and philosophers lived in different worlds and operated by different principles (just consider the ratio of arguments to examples, for instance: philosophers go on and on not just for pages but for decades of exchanges about one or two key examples, whereas linguists almost always think each example has to be fleshed out with as many related and contrasting examples as possible and generally say less about each one), there was a great deal of mutual respect and learning from one another in conferences where both were present, such as the 1969 conference[65] that resulted in the volume (Davidson and Harman 1972), and whose success undoubtedly was part of the background for the 1971 Irvine

[64] See Bach and Partee (1980) for discussion of the different predictions; Edwin and I put the example into interrogative form to more strongly eliminate the possibility of "reconstruction".

[65] Montague was not invited to that conference. I had the impression at the time that the organizers considered him likely to be overly polemical, though Gil Harman's memory is that they wanted to include David Kaplan and didn't want to over-represent the west coast. And I don't remember anything negative in Montague's participation in the 1970 conference which led to the publication of (Hintikka et al. 1973).

summer institute mentioned earlier. Philosophers had thought a great deal about reference, quantification, and indexicality, about logical structure, about intensionality, about tense, aspect, and modality, the logic of modifiers, the semantics of interrogatives, and many other semantic issues which were very new to linguists in the 60's and early 70's. Linguists, on the other hand, had generally thought more about syntactic structure and syntactic constraints on possible interpretations, and were very good at generating examples that could push the limits of any suggested generalization[66].

That 1971 Irvine summer institute was a memorable and influential event for me, not only because it was the occasion where I figured out how to make use of lambdas to model 'syntactic deletion', but for the intense immersion in a philosophy of language environment that it afforded. It lasted six weeks, in two three-week sessions. Each week had lectures by three philosophers and one linguist, and the "students" were themselves all young philosophy professors, including Rich Thomason, Bob Stalnaker, Gareth Evans, Dick Grandy, Peter Unger, Steven Stich, Bill Lycan, Bob Martin, Oswaldo Chateaubriand, Carl Ginet and his linguist wife Sally McConnell-Ginet, and many others; and many of them gave evening lectures. I was the linguist for the first session, and also attended the lectures by Davidson, Harman, and Grice[67]; and I commuted regularly (with Michael Bennett) to the second session as well to attend the lectures by Strawson, David Kaplan, Quine, and Haj Ross, as well as the extra lecture series by Saul Kripke on his new work, "Naming and Necessity". The long discussion periods following each lecture, as well as many discussions with Thomason, Kripke, Michael Bennett, and others outside of the regular lectures were a large part of my own philosophical education, and the intense interactions led to a lasting camaraderie among all of those who were together there for those six weeks. The 1974 Linguistic Institute at UMass also brought in quite a number of philosophers of language, and continued the lively and productive interaction.

I wrote earlier about how the Sloan-prodded growth of cognitive science, while also very fruitful, seemed less purely wonderful than the earlier linguistics-philosophy interactions. I still care about issues at the foundations of cognitive science, and there are places like the University of Pennsylvania where there is a really good mix of diverse but theoretically compatible people (led by Lila Gleitman and Aravind Joshi) who I think can make real progress on hard questions. But it's rare to have a really good combination of compatible colleagues across more than two disciplines. So I work on topics for which I have a good environment, and leave other topics that I would love to work on for some other lifetime.

And I have definitely had wonderful environments to work in, as I hope I've made clear above. I am grateful to colleagues and students in linguistics and philosophy at UCLA, which was a great place to spend my first seven professional years, and at UMass,

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[66] In the earlier mentioned workshop of linguists and philosophers at the 1974 Linguistic Institute in Amherst, I recall that several philosophers including Hans Kamp had theories of the distribution of *any* to propose, and we linguists sat there like duck-hunters, ready to pop off with counterexamples to any proposal. None of us had a good theory of *any* (this was before Ladusaw's dissertation (Ladusaw 1979), and long before Kadmon and Landman (1993) and other more recent work), but we were very well armed with examples against any theory that any of the philosophers had to offer.

[67] Another difference between linguists and philosophers shows up in schedules for conferences and institutes. There were just four lecturers in each three-week session, and each gave just two hour-and-a-half lectures per week, each lecture followed by an hour and a half discussion. I had Monday and Thursday afternoons. Wednesdays were free. It was a schedule that really allowed time to think and discuss.

which has been a great place to spend the rest of my professional life, as well as in all the places where I have been for shorter periods. I'm not the kind of person who could have developed ideas in isolation; I've been absolutely dependent on having colleagues and students to interact with, and have been marvelously lucky in having such fine ones.

2.2 Some opinions.

Theory diversity is a good thing, like biodiversity and diversity of languages: it enriches the stock, and it prods people to keep asking challenging questions. But it's not easy to maintain, because would also be impossible to carry on fruitful discussions if everyone had their own theoretical framework. Being able to discuss issues together with some common assumptions is important. So it's always a difficult challenge to help enough diverse theories flourish without total fragmentation, either within a department or within a field as a whole.

Personality diversity is probably a good thing too, and diversity in intellectual style and intellectual interests. For a while I thought I would never be very successful in linguistics because my strength does not lie in the invention of new theories, and my education led me to internalize the idea that the invention of new theories is the really important thing. And what I was better at was just looking at what different people were doing and seeing what seemed good and what seemed problematic in different approaches, and trying to find ways to put together what was best in different approaches – I think it was a natural extension of my mother's tendency to be unhappy if two of her good friends were at odds with each other, and to look for some way to help make peace between them. And that never seemed a very bold or independent kind of thing to do, but gradually I was made to realize that that's valuable too, and that not everyone is good at it or temperamentally suited for it. So now I try to help reassure students that there are many different ways to be a good linguist, and no one "great linguist" is a good role model for everyone.

And here's another small comment related to personality diversity. Letting collegiality be a factor in hiring is tempting because it's really nice to have a congenial department. But I can't forget that Montague was one of the least congenial people I have known, and that certainly didn't and shouldn't mean that he shouldn't have had a position in a top department.

Formalization is an excellent thing in moderation. When there's too little, claims tend to be fuzzy and untestable, and argumentation can't help but be somewhat sloppy. But there can be such a thing as too much formalization, or premature formalization, or formalization that has too little relation to anything empirical. So even a formal semanticist or formal syntactician or formal anything else shouldn't hesitate to write things down and share them with colleagues when they are still in an informal state; often it requires looking at things from many points of view before a good path to formalization emerges.

Theo Janssen reminded me of something similar that I once wrote about the "method of fragments", which he quoted in (Janssen 1983a, p.96). "It can be very frustrating to try to specify frameworks and fragments explicitly; this project has not been entirely rewarding. I would not recommend that one always work with the constraint of full explicitness. But I feel strongly that it is important to do so periodically, because otherwise it is extremely easy to think that you have a solution to a problem when you in fact don't." (from Partee (1979b, pp. 94-5)) The method of fragments, complete explicit grammars (syntax and semantics) of a small subpart (fragment) of a language, was, as Theo notes, characteristic of the way of working that Montague introduced, but by the end of the 1970's it was becoming rare to actually write a complete fragment. Syntacticians outside of computational linguistics had long since stopped writing fragments (such as can be found in Chomsky's earliest papers, in *Syntactic Structures,* and in Lees's 1959 MIT dissertation (Lees 1963)). I suspect that by the 1980's the only place that complete fragments could be found was in computational linguistics projects. I know from experience that in order to write a complete fragment, you normally have to include decisions about things for which you have no principled basis to choose, or for which you know that none of the available alternatives are really good. But the other side of the coin is expressed in the quote, and even if one's goal is to have as much as possible follow from general principles and not be "stipulated", we can't have much hope that we know the right general principles yet. And even if we think we do, it would undoubtedly be a most enlightening exercise to try to actually derive what would be the explicit version of the rules (or constraints, or whatever they should be) from the assumed principles.

Compositionality can't be an empirical hypothesis all by itself. We have known that for a long time. In the mid-70's, when we were working a lot on how to constrain Montague grammar, Emmon proposed a name for our implicit working methodology: the "No Funny Business Constraint." It just meant that you shouldn't do something ugly or gratuitously innovative or unmotivated in the syntax or the semantics just to make things come out compositional. It's what often underlies the phrase "independently motivated." But compositionality is a powerful working hypothesis. See discussion and examples in (Janssen 1997, Partee 1984a). There are some current debates about the status of compositionality in the literature, but surprisingly or not, I haven't gotten around to looking at them – I think I don't actually enjoy dwelling on methodology.

Why has Chomsky been so resistant to compositionality? It's such a natural hypothesis. Semantics has to have some kind of syntax to build on – we can't put the meanings of the parts together without some notion of what the "parts" are, and that's syntax. So it would seem an unpleasant design flaw if natural languages had to have two different syntaxes, one just to describe what sentences there are and their structures, and another to provide the syntax that the semantics needs. Of course things aren't always simple, and there can be mismatches of various sorts around the edges, as there seem to be between any two components. But Chomsky's resistance to the idea of compositionality has seemed much deeper than any rational arguments could explain, as far as I could see. My own speculation – and this is pure speculation – is that it may stem from the same source as his deep resistance to any kind of functional explanations in syntax, which also seem deeper than a simple rational skepticism. Chomsky has often pointed to some of the oddest principles in syntax (e.g. "disjunctive ordering") as evidence for an innate special-purpose language faculty, citing as a plausibility argument that something as syntax-specific as disjunctive ordering couldn't very well follow from any general cognitive principles, and certainly couldn't be learned, so it must be language-specific and it must be innate. It seems that any kind of outside explanation of anything in syntax is unwelcome because it might weaken the argument for this special innate faculty. Semantics may seem too close to general cognitive faculties for comfort[68], and compositionality requires a homomorphism between semantics and syntax, so compositionality would seem to weaken the important 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 language acquisition mechanism to work on acquiring syntax and semantics in parallel, with any evidence for how the semantics of one's language works providing indirect evidence for how its syntax is structured and vice versa. I have not done any serious research in this area and haven't written anything about it in any scholarly publications, but in this "opinions" section[69] I will assert that I think you can learn a language a lot better by watching TV than by only listening to the radio, and that that's one of many bits of evidence that we learn syntax and semantics together. Another anecdotal bit is the constant folk etymologizing of idioms, suggesting a very strong drive to understand language compositionally. More serious work on the interaction of syntax and semantics in acquisition has been carried out by Lila Gleitman and her students, and some by Stephen Pinker.

Here is a totally different and much less polemical opinion. I think it's a good idea to write down non-results as well as results, somehow. (But I don't think a journal of non-results would sell very well, though I've seen the idea proposed. So you have to find a way to fit your discussion of non-results into an interesting article, probably one that includes some results.) This is something I only gradually came to realize and to advise students about. I sometimes tell my students about an article I didn't write that I've always regretted. Chomsky's attack on compositionality in his 1974 Linguistic Institute Golden Anniversary lecture (Chomsky 1975) included an argument based on sentences like (6). His claim was that *have wheels* in (6) doesn't have the plural meaning that it would have in a sentence with a singular subject, and therefore one can't build up the meaning of the whole sentence compositionally: one has to know whether the "whole sentence" is singular or plural to know how to interpret such a predicate.

(6) Unicycles have wheels.

In my reply (Partee 1975a) I was able to argue against Chomsky's proposed analysis in terms of plurality as a property of the whole sentence (using examples like (A12), discussed in the Appendix), but I didn't have a compositional analysis to offer. So I spent the whole following summer looking for one. I explored three or four possible approaches, but I found problems with each one. So I didn't write up any paper, and that's what I came to regret, because various people would from time to time propose an analysis of some such sentences (involving what are often called *dependent plurals* (de Mey 1981)), and I could usually go back to my notes and find some counterexamples to their proposals. So I came to realize that it could have been helpful to publish a paper on three non-solutions to

[68] But the closeness of formal semantics to general cognition can certainly be questioned. Formal semantics is itself a highly structured language-specific system whose interface with general cognitive systems may be almost as indirect as their interface with syntax.

[69] This section of this essay suddenly reminds me of the "Theses" that Dutch Ph.D. candidates always have to include on an insert sheet in the front of the copies of their dissertation before the defense, and there they can express judgments about anything from whether Frege really asserted "Frege's principle" to what the best current rock band is.

the problem of dependent plurals, just so the steps I had managed to take and the counterexamples I had found wouldn't have to be repeated over and over. (There have been some classic papers and squibs that present problems and non-solutions, such as (Bach 1970) with its introduction of the Bach-Peters paradox.) Another regret concerns the parts of our work that we omitted in the final version of (Stockwell et al. 1973), particularly the appendix with rule ordering. We had figured out a careful partial ordering for all of the many transformations in the book, but late in our work we discovered that our rule ordering ran into impossibilities with respect to the transformation of Conjunction Reduction, which was responsible for deriving all (Boolean) phrasal conjunction. It turned out that basic transformations like Passive had to operate both before and after Conjunction Reduction within a single transformational cycle, which was impossible. And because we couldn't find any way to "Solve" that problem, we left out the whole appendix on rule ordering. In retrospect I'm sure it would have been better to include all of the rule ordering that we had worked out ignoring conjunction, plus a clear and frank discussion of the "ordering paradoxes" created by Conjunction Reduction. I don't have as strong a conviction that it was a mistake to leave out our chapter on Adverbs when we realized that we still had a lot of uncertainties about the analysis; but I am sure it was a mistake to leave out that appendix on the ordering paradox. A third place where I came to regret the omissions was my dissertation. There were a lot of things I worked on but didn't reach definite conclusions about and therefore simply left out of the dissertation. I realized it when I was reading Pullum's "Topic-comment" column about the history of the Unaccusativity hypothesis, and he mentioned that my dissertation, while relatively early in that history, discussed only those intransitive verbs that had transitive counterparts, and did not say anything about a distinction among verbs that have only an intransitive form. My immediate reaction was to think I was being maligned, because I could remember thinking about many such verbs. But when I looked in the dissertation, I realized that Pullum was right: non of that discussion was there anywhere, because I had no conclusion to offer. I'm definitely sorry that I didn't at least raise the puzzles.

You never know in advance what your most valuable contributions will be. When we wrote (Stockwell et al. 1973), we thought the most valuable part was our grammar, but in fact it was all the discussion of all the then existing alternatives for each rule and all the arguments we were able to marshall for and against each alternative. A lot of that is still valuable.

I guess I now think my own most valuable contributions have been in two areas.

One is in the synthesis of various diverse ideas. The UCLA syntax project was designed exactly as a big synthesis project, and we certainly knew that it would require a lot of original work to accomplish that, which it did. The Montague grammar work, synthesizing Montague and Chomsky, was my own first big self-directed effort of that kind. That project in a sense has never ended, but has broadened into "the syntax-semantics interface", and is no longer just mine and involves many variant theories of syntax and many variations on the semantics as well.

My type-shifting work, which some colleagues regard as some of my best work, was also in my own mind mostly synthesis. I respected the motivations behind the different ways different people were treating NP semantics, particularly Montague vs. Heim and Kamp, and wanted to find a way that they could all be right. In my paper on compositionality (Partee 1984a), I had discussed as one of the important potential challenges to compositionality Keenan's "functional principle" (Keenan 1974). That principle, which states that the form and meaning of a functional expression may vary with the form and meaning of its arguments, suggests that the "meanings of parts" are not autonomous. The work with Mats Rooth on conjunction seemed to show beyond doubt that indeed the meanings of parts do necessarily sometimes shift in ways that allow the compositional combining principles to work smoothly. And with that work I think I began to be able to see that when one part influences a meaning shift in another part, it isn't a violation of compositionality but something that happens under the pressure of compositionality: compositionality is one of several constraints that have to be satisfied simultaneously. The issues of what causes what to happen are far from settled (and a perspective of simultaneous constraint satisfaction may eventually render such questions moot), but the area of meaning shifting and type shifting principles and the nature of the constraints on category-type correspondences and other aspects of the syntax-semantics interface has turned out to be a very rich area for continuing investigation.

Later there came the synthesis of Prague School work with formal semantics, which was less successful in part because of some rather deep theoretical differences which remained unbridgeable. My current work is also synthesis on two dimensions at once, Russian and western approaches to semantics, and lexical semantics with compositional semantics, and about that I'm quite optimistic.

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

I think my strengths in teaching and in research are probably related. They say that I'm a good listener and good at suggesting connections to be drawn between one thing and another. I suppose that's at the heart of my teaching and also at the heart of my work – I like looking at things from various points of view, spotting connections, and trying to figure out what different ideas may have in common as well as where the crucial obstacles are to putting them together. For quite a few years I felt a slight inferiority complex because I wasn't gifted at inventing new theories that I could put my own name on, but rather worked at analyzing strengths and weaknesses of various theories, and putting together other people's theories, and solving problems. I contented myself with the idea that if everyone was always coming up with their own theory, it would be hard to make any collective progress. Gradually I learned to value synthesis more and to value my work. I'm grateful to all the teachers and students, friends and colleagues, who have touched my life and my work and helped to make it such an exciting, rewarding and joyful adventure.

Appendix: Example sentences[70].

This appendix consists of an annotated list, in chronological order, of some of the examples that I've thought up over the years that have been influential in one way or another. The list includes ones that have appeared in my own papers (like (**A7**) 'I didn't turn off the stove') as well as ones that have appeared in other people's dissertations or articles, like the 'marbles' example (**A16**) that I contributed to Irene Heim's dissertation, and the 'temperature' example (**A5**) in PTQ. Following all of those, the "A" list, is a short "B" list of examples which I didn't originally invent but which became best known through my work or in my 'variants'. I'm including the B list both because those examples have become part of my history, too, and more importantly in order to make sure it's known that they weren't my invention.

As some of my correspondents have noted, this list forms a sort of mini-history of its own. I've tried to add just enough annotation to put the examples in context and to indicate what they were used to show. Cited works include the first occurrence and in some cases works where there is further discussion.

Part I. The A-list.

A1. a. Few rules are both explicit and easy to read[71]. (Partee 1970a, p.154)
b. Few rules are explicit and few rules are easy to read.

This example also occurs as example (142) in (Stockwell et al. 1973, p.105).

This example and its non-equivalence to (7a) or (7b) were part of my argument with Lakoff about his association of underlying structures with semantic interpretations, but it had broader implications as well.

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

[70] Many friends and colleagues answered my request for help with this assemblage of examples, suggesting examples to include, helping me figure out whether some examples should or shouldn't count as "mine", helping track down citations and their historical contexts, and giving me encouragement with the project. My thanks to Luis Alonso-Ovalle, Emmon Bach, Greg Carlson, Shai Cohen, David Dowty, Elisabet Engdahl, Irene Heim, Paul Hirschbühler, Larry Horn, Theo Janssen, Nirit Kadmon, Angelika Kratzer, Jeff Pelletier, Craige Roberts, Hotze Rullmann, Roger Schwarzschild, Muffy Siegel, Anna Szabolcsi, Rich Thomason, Ede Zimmermann, and Sandro Zucchi. I take full responsibility for any inaccuracies. If any readers spot inaccuracies, particularly if I'm taking credit for examples which are of types for which there are earlier sources, please let me know so that I won't repeat the errors further.

[71] When Emmon and I were fellows at CASBS in 1976-77, there was a tradition of weekly talks by the fellows to give their colleagues in other disciplines a taste of their research. Emmon and I shared an evening's presentation and decided to talk about our most famous example sentences and the issues behind them. Emmon talked about the Bach-Peters paradox (Bach 1970) and I talked about these. As we introduced our talk, one of the other fellows (Emmon thinks that maybe it was Michael Kamin, the historian) asked with great puzzlement, 'What do you mean, you "discovered" these sentences?' That was an interesting question to answer; any non-linguist reading this might be puzzled by this whole appendix.

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. So compositional semantics is incompatible with any transformational derivation involving tough-movement as a sentential transformation (not to mention sentential conjunction-reduction). There was no solution to the problem in my early paper, but it can be solved with the Montague Grammar + Transformational Grammar tools proposed in (Partee 1973b), and with the non-transformational approaches proposed later by various people.

A2. My home was once in Maryland, but now it's in Los Angeles. (Partee 1970b, p.369, ex. 37, and in footnote 10, p. 384), reprinted in (Partee 1972, p.245) and in (Partee 2004, p.35); also in (Stockwell et al. 1973, p.202).

This example is a variant of a well-known example of Postal's, (8) below, which is also cited in the references above.

(8) 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*. "Note, incidentally, how (10-2) [**A2**] seems to justify the adage that 'a house is not a home', since if 'house' were substituted for 'home', the sentence would be true only if the actual physical object had been moved cross-country" (Partee 1970b, fn. 10, p. 384)

A3. John won't buy a car because he wouldn't have room for it in his garage. (Partee 1970b, p. 372, ex. 50), reprinted as (Partee 1972) and in (Partee 2004, p. 37).

This example is a *prima facie* counterexample to a claim of Karttunen's concerning the lifespan of discourse referents: normally an indefinite inside a negated clause can't license a pronoun outside that clause. In this case it seemed that what allowed the pronoun was a tacit *if*-clause 'if he did buy a car'; but there was no place in any theory of the time for unexpressed clauses to provide antecedents for explicit pronouns, and it was not clear how such possibilities could be constrained if they were permitted at all. Such issues are addressed in Roberts's work on modal subordination (Roberts 1989, Roberts 1990).

A4. John is building a house.
(Bennett and Partee 1972) (pp. 16, 18 in 1972 version, pp. 13, 15 in 1978 version.)

Michael Bennett and I decided in 1978 to distribute our 1972 paper even though we were still not satisfied with it, because the paper contains the first proposal for interval semantics and was being cited for that in its manuscript form. This example, which I produced during discussions with Michael as he was writing his dissertation, was a key piece of evidence. On Montague's analysis of the progressive, the progressive form of a sentence is true at t iff the corresponding non-progressive is true at every point in some neighborhood surrounding t; but that can't be right for an example like this one. Our rule (which Dowty argued against on the basis of his "progressive paradox" examples like *John was crossing the street when he was hit by a truck*) was that the progressive of a sentence like (**A4**) is true at t if t is part of an interval at which the non-progressive form is true. The paper is in

(Partee 2004); see the discussion there. Our 1978 appendix explains why we considered the paper incomplete and still unsatisfactory but still worth at least semi-publication.

A5. The temperature is rising.
The temperature is ninety.
do not together entail: Ninety is rising. (Montague 1974, p. 267), originally in (Montague 1973).

Montague cited me as the source of the example, which 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 in (Lewis 1970) that intransitive verbs are never intensional with respect to subject position, and I was just learning about intensionality and trying to see if I could understand what that meant. So I did what I often do, and 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 made a provision for them in PTQ. But as shown in Dowty, Wall, and Peters (1981), Montague's solution doesn't really do what we would want it to do, reinforcing the opinion of Jackendoff (1979) and others that the time-dependent functions involved in examples like that aren't really the same as normal intensionality.

Greg Carlson notes that I often used a related example in class: the ambiguity of *The president is gaining power.* Similar examples were discussed by Sebastian Löbner (1979, 1981) – ones like "the football coach is changing" (but in German), and by Theo Janssen in (Janssen 1984).

A6. She giggled that she would feel just too, too liberated if she drank another of those naughty Martinis. (Partee 1973d, p. 326, example 26)

That example, in my first paper about *belief*-sentences, showed that some kinds of verbs of 'manner of saying' require, or at least can be sensitive to, details of wording even in indirect quotation, though the "I" changes to "she", etc. in the usual way. It was discussed with attribution to me by Creswell (1985, p.43). .

A7. I didn't turn off the stove. (Partee 1973a, p.602), reprinted in (Partee 2004).

This is now probably one of my most famous examples, partly because the issue it raised is still not fully settled. I used the example to argue against the standard treatment of past tense as existential quantification over past times, arguing that neither of the possible relative scopings of existential quantifier and negation gave a suitable reading, and that the past tense should be considered analogous to a pronoun, whose value in this case should be supplied by the context. 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.

A8. John left at 3AM, and Mary left early (too). (Stockwell et al. 1973, p. 270, example 114).

This was an example of mine in our syntax book, illustrating the realization that had just dawned on me that the rule of *too*-addition cannot be syntactically conditioned, and does not even depend on semantics. Semantics could tell us that "3AM" would count as a case of "before 4am", but semantics can't tell us whether 3AM counted as early on that occasion or not; the use or non-use of *too* in this case reflects a judgment made by the speaker in the context. The conclusion was that the syntax should always permit *too* in conjunctions, (not stated explicitly there, but what I would say now: and that the use of *too* should carry a presupposition to the effect that the property expressed by the VP in the first sentence is an instance or a subproperty of the property expressed by the VP in the second sentence.)

A9. Many men date many women. (Bennett 1974, p. 105)

This example, which I suggested to Michael Bennett for use in his dissertation, shows that different occurrences of context-dependent *many* with similar nouns nevertheless may get very different contextual values. That shows that one cannot take the relevant "context" for calibrating a vague quantifier to be the sentence as a whole. The example is also cited, with reference to Bennett, in (Partee 1989b, p.384).

A10. a. Fred was trying to find the minutes before the meeting began.
b. Fred was looking for the minutes before the meeting began. (Partee 1974, p. 99, examples 53 and 54.)

This pair of examples is the basis for an argument against the decomposition of *look for* into anything with two clauses like *try to find*. Sentence (**A10a**) has a scope ambiguity that (**A10b**) lacks. (And this contrasts with certain other "decompositions" like *want* into *want to have*, where the two forms show matching scope behavior.) As I noted in the paper, (**A10**) seems like rather slender evidence when pitted against a very strong constraint that would say that intensional contexts are always really proposition-embedding contexts, a constraint implicit in Quine's work and much other work in the philosophy of language but explicitly denied by Montague. I think the jury is still out; lexical semantics and issues of lexical decomposition have not advanced far enough to settle it.

A11. a. Only John believes it would be inadvisable to vote for himself.
b. Only John believes it would be inadvisable for him to vote for himself.
(Partee 1975c, examples 9 and 10.)

This pair was one of my contributions to debates about the distribution of bound variable interpretations and "coreferential" interpretations of pronouns, reflexives, and "zero" forms. In this minimal pair (involving the configuration of "Super-Equi Deletion"), the overt pronoun in (**A11b**) can be interpreted as coreferential with *John* and cannot be interpreted as a bound variable, whereas the corresponding "null" element in (**A11a**) can only be interpreted as a bound variable bound by *only John.*

A12. **The boys gave the girls nickles.** (Partee 1975a, p.206).

This was my counterexample to Chomsky's claim that in a sentence like *Unicycles have wheels*, where *wheels* is not 'semantically plural', plurality should be a feature of the whole sentence. It also shows that the dependent plural *nickels* can have either subject 'control' or object 'control.'

A13. John is being hard to please/ *It is being hard to please John (Partee 1977a, p.303, examples 34 and 35.)

I used this pair as an argument for having some way to derive *easy/hard to please* as predicates, not derived via a sentence-to-sentence transformation. It was also part of an argument for the existence of an 'active verb *be*,' further bolstered by the pair of examples (63) *John is being noisy* and (64) **The river is being noisy* on p. 307 of the same article. The occurrence of the progressive on *be* in these examples is limited to animate subjects; I argued that this is a property of the 'active verb *be*', whose meaning is something like 'act'. (This conclusion has been challenged by Déchaine (1995, 1993).)

A14. a. Green bottles have narrow necks. (Carlson 1977, 1980, p. 199)
b. ??The green bottle has a narrow neck. (Carlson and Pelletier 1995, p. 11, example 24b)
c. The Coke bottle has a narrow neck. (Carlson and Pelletier 1995, p. 11, example 24a)

Although I felt sure that both of these examples (as a contrasting pair) occurred in Carlson's dissertation, only the first does. The earliest instance he or I or anyone who has helped me has found of the second is in the introductory paper (Krifka et al. 1995) in *The Generic Book*, (Carlson and Pelletier 1995). Both Greg and I thought it had occurred earlier; he supposes that he probably presented it in talks and in unpublished work, because it was definitely well-known before 1995. *The Generic Book* itself says (p.11) that (**A14b,c**) come from (Carlson 1977) and that Carlson there attributes them to Barbara Partee. But several people, including Greg, have searched the dissertation with care, and it turns out that 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". There are many more recent references to these examples, mainly citing *The Generic Book*, which has made them particularly accessible. Jeff Pelletier agrees that he is the one who put that attribution into (Krifka et al. 1995), fully believing (as did many of us!) that the examples were in Carlson's dissertation, and having known and orally reported those examples for many years with attributions to Greg's work and to me as the source of the examples. (All the attributions are correct except for the detail about what got written down in Greg's dissertation.)

A15. Smith believes that <u>that</u> door is locked, but she doesn't believe that <u>that</u> door is locked. (Partee 1979c, p.4, example 2)

This example is discussed in (Chierchia and McConnell-Ginet 1990, p.257). The example is meant to show that belief cannot be a relation to a sentence type or the meaning of a sentence type, or a "semantic representation", and hence to provide an argument against Fodor's "methodological solipsism". The sentence is non-contradictory iff the two occurrences of the demonstrative *that* are associated with two different demonstrations; the identity of the two sentences on a purely linguistic level does not settle the issue.

A16. 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.

This pair first occurred in print in (Heim 1982, p.21). Her version (credited to me):

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

My original version above (as I recall it) is cited (just as 'my old examples') in (Partee 1989a, p. 363, fn. 13).

Roger Schwarzschild notes that the following pair, from (Carlson 1984, p.320) is similar.

(9) a. I did not catch all of the words. They were spoken too indistinctly.
b. I missed some of the words. They were spoken too indistinctly.

I believe that Lauri Carlson almost certainly got the examples from Irene Heim, if not indirectly from me. Lauri attended my workshop in which Irene Heim found her dissertation topic, and he and she discussed the problems of anaphora with indefinite antecedents quite a bit. I also repeated the ball/marble examples during discussion at a conference in Norway in approximately 1980 or 1981 where I believe Hintikka was present, and Carlson and Hintikka worked together on anaphora around that time.

A17. The department wants to hire a phonologist or a phonetician. (Partee and Rooth 1983, p. 375, example 37)

A closely related example, *Mary is looking for a maid or a cook*, occurs in Rooth and Partee (1982, p. 355, example 13). In both versions of the example, the puzzle is the existence of a reading on which each noun phrase is understood intensionally (non-specifically), and yet the disjunction is understood to have wide scope, so that the sentence could be followed by "but I don't remember which." This is incompatible with a Montague-style analysis in which the entire disjoined NP is a single generalized quantifier; there is no simple way that the *or* could have wide scope while the two disjoined NPs each have narrow scope. The problem is a central topic of Rooth and Partee (1982).

A18. Every search for two men with red hair failed.

I believe I first began using this example in class and in oral discussions in the early 1980's. The earliest citation that any of my correspondents have found is (Rooth 1985, p. 114), where it is attributed to me but with no particular source. The example has a three-way ambiguity that shows the need for being able to "Quantify In" to the CNP, in order to get the reading where 'two men' has narrower scope than *every* but is outside the scope of *search for*. At first I thought I would find this example back in Partee (1975b). But it's not there; the only examples used in Partee (1975) to argue for quantifying in to CNP are of a different sort, and I wouldn't use them to argue for that now.
There I used (60) *Every man who has lost a pen who does not find it will walk slowly,* based on an example of Joan Bresnan's cited in footnote 44: *Every girl who attended a women's college who made a large donation to it was included in the list*.

Now I think that I may have come up with examples like (**A18**) in reaction to something of Higginbotham's; he was resisting *in situ* generation of NPs, and we may have had some discussion of such examples around 1980 or soon after. I think he or possibly

Bob May may have had some similar examples but without noticing all three readings and the need to have both an *in situ* derivation and a derivation with quantification into CNP.

A19. Either this house doesn't have a bathroom or it's in a funny place.

This is an example that I generated in the discussion period after a lecture Hans Kamp gave in the UMass Philosophy Department in approximately 1982. The pronoun *it* should be impossible, as it seems to be in the example in (Geach 1962), which was something like "Either Smith doesn't own a donkey or he beats it" (thanks to Anna Szabolcsi). But evidently I wasn't the first to think up such examples. This example is attributed to me by Roberts (1989, p.702), who 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 that paper of Evans, and I may have been unconsciously plagiarizing.

But attribution to me has continued. Hotze Rullmann found in Roberts (1996), p.243, citation of the example, with the statement "Examples like (40) [Either there's no bathroom in this house or it's in a funny place.], originally due to Partee, are often cited in discussions of anaphora in discourse (see, for example, Heim (1982), Roberts (1987, 1989), Groenendijk and Stokhof (1990a), Dekker (1993b), van der Sandt (1992), Beaver (1992).) And Luis Alonso-Ovalle found that Chierchia, in his dynamic semantics textbook (Chierchia 1995, p. 8, example 23a) has *Either Morrill Hall doesn't have a bathroom or it is in a funny place* and attributes it to me.

Nirit Kadmon points out that the example is also similar to Gazdar-Soames examples such as *If Mary's boss doesn't have children, then it wasn't his child who won the fellowship,* in that the right-hand clause, q, has a presupposition which is incompatible with the left-hand clause, p, and therefore global accommodation either of this presupposition itself, ps(q), or of the Karttunen conditional presupposition p --> ps(q) is impossible.

A20. any triangle such that two sides are equal (Marsh and Partee 1984, p. 187).

This example is cited in Pullum (1986, p. 136). This NP contains a *such-that* relative clause with no bound pronoun in it, and we used it to argue that in the *syntax* there is no prohibition on 'vacuous binding', although semantically the clause is interpreted as a property of triangles (as if 'sides *of it*'). We noted that whether such constraints are in the syntax or the semantics can make a difference to the generative power needed for the syntax. Since the semantics has to be able to deal with variable-binding in any case, we suggested that any possible constraint on vacuous binding might belong in the semantics as well if anywhere. Higginbotham (1984) cited my example (we had already argued about it orally) but dismissed it as ungrammatical but interpretable, and used the claimed ungrammaticality of such examples to argue that English is not context-free. See discussion in (Pullum 1986).

A21. Mary considers John competent in semantics and an authority on unicorns.
(Partee 1986, p.119)

I used this example to argue that NPs can sometimes be type <e,t>, since they can sometimes be conjoined with APs used in <e,t> position (really this is a simplification of what should be an intentional type, as noted in the paper). APs are generally assumed to be of type <e,t>, and the complement of *consider* is also generally assumed to be of type <e,t> (or an intensional version thereof), and Partee and Rooth (1983) had argued that conjuncts must agree in semantic type. So several properties of this example conspire to argue that the final NP must be of a predicative type, and not a generalized quantifier nor a Heimian indefinite.

A22. We found part of a Roman aqueduct. (Bach 1986b, p.12, example 21)

This example was created jointly with Emmon Bach as we discussed the project that he eventually wrote up. We called it the "partitive puzzle" analogous to Dowty's "imperfective paradox": the sentence can be true even if no complete aqueduct exists or ever did or will exist.

A23. a. The trees are denser in the middle of the forest.
b. All the trees are denser in the middle of the forest. (Dowty 1987)

I came up with this pair in discussion with Remko Scha in Nijmegen during the 1982-83 academic year, along with analogous examples distinguishing between *the pie* and *the whole pie*. I didn't put it into any of my own papers as far as I can discover, but Dowty attributed the pair to me in his *ESCOL '86* paper. They show that *all*, like *whole*, while not requiring distribution all the way down to atoms, does require distribution down to some salient partition.

A24. A computer computes the daily weather forecast. (Carlson 1989, p.172, p. 185), also in (Carlson and Spejewski 1997, p. 121).

This example is also cited in (Krifka et al. 1995), as "due to Barbara Partee, reported in Carlson 1989". The example is analogous to Milsark's *Typhoons arise in this part of the Pacific*, also cited in (Krifka et al. 1995), but with a singular count noun. What's important is the ambiguity; the subject NP may be understood either generically or existentially.

Another generic sentence of mine cited by Carlson is "#John builds a cabin", the puzzle being that the sentence seems deviant even if we know that John has made a long-term project out of building a cabin and works on it on all his weekends and may or may not ever complete it[72].

A25. John often comes over for Sunday brunch. Whenever someone else comes over too, we (all) end up playing trios. (Otherwise we play duets.) (Partee 1989a, p.358, example 27).

This example shows that *we* can consist of deictic *I* plus a "coreferential" pronominal part (coreferential with *John*) plus a bound pronominal part. The paper is in (Partee 2004).

A26. - Eva only gave xerox copies to the GRADUATE STUDENTS.
- (No,) PETR only gave xerox copies to the graduate students. (Partee 1991)

Since my work on focus and quantification was inspired by my stays in Prague and my interaction with colleagues there, I used Eva (Hajičová) and Petr (Sgall) in this example[73]. This is an example of what I later learned is called "second occurrence focus" by Prague school colleagues. The puzzle is that in the second sentence, *only* continues to associate

[72] As Greg reminds me, the original (real-life) example was #*Emmon builds a cabin*.
[73] And I have just seen that they in turn have taken this example as an epigraph for their paper in the forthcoming festschrift (Carlson and Pelletier In preparation).

with "the graduate students", even though focus doesn't appear to be there any longer in that sentence. More recent work from many quarters has helped to give a better understanding of everything from the phonetics to the semantics and pragmatics of such examples.

A27. a. **Henrik likes to travel. He goes to France in the summer and he usually travels by car. He goes to England for the spring holidays and he usually travels by ferry.**
b. **Henrik likes to travel. he usually travels by car and he goes to France for the summer and he usually travels by ferry and he goes to England for the spring holidays.** (Partee 1993)

This example is cited in Gawron (1996, example 2.) as a nice illustration of the dynamic behavior of quantificational domains. In that paper I was illustrating parallels among anaphora, domain restriction, and presupposition on a Heimian treatment.

A28. a. **My 2-year-old son built a really tall snowman yesterday.**
b. **The D.U. fraternity brothers built a really tall snowman last weekend.**
Kamp and Partee (1995, p. 142).

We used this pair to support the claim, originally due to (Kamp 1975), that an adjective like *tall* is not intensional (like *good* or *skillful*) but is extensional but vague and context-dependent. A crucial property of the pair of examples is that the noun to which the adjective is applied is the same in both cases, but the sentences as a whole make it clear that there are different norms for heights of snowmen in the two cases.

A29. ***a midget giant* vs. *a giant midget* vs. (*be*) *a giant and a midget*** Kamp and Partee (1995, p. 159).

We used this pair 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.

A30. a. **Endangered species are common.**
vs.
b. **Endangered species are rare.**

And yet another generic pair of mine which I had forgotten about until I found them now in (Krifka et al. 1995, p.98, examples 156a-b.); this pair is an interesting case of interaction of quantificational predicates with kind reference. The most natural readings on which they can simultaneously be true are ones where (**A30a**) says there are many endangered species, and (**A30b**) says that an endangered species is generally a rare species, i.e. one that has few specimens. Jeff Pelletier (p.c.) believes that either he heard them from me in 1986 or 1987, or heard them around then from Greg Carlson with attribution to me.

A31. Groenendijk and Stokhof are one person. (Schwarzschild 1996, p.10)

Schwarzschild attributes the sentence to me, I think correctly. The issues concern syntactic and semantic number and ontology. The sentence is false, but certainly grammatical and interpretable.

A32. a. Why don't you love me any more?
vs.
b. Why don't you love me some more(?)

Larry Horn reports learning these from me, with their interesting illocutionary difference. I think I designed them as a variant on examples I had learned from Bolinger (1977) like "Didn't you write some poetry last year?" and "Didn't you write any poetry last year?", combining his observations with the special pragmatics of some positive polarity *Why not X?* questions.)

Part II. The B-list.

The example sentences in this list are due to other people but first were published in my work, hence known through my work.

B1. John needed and bought a new coat.

This example is analyzed in Partee and Rooth (1983), where it occurs as example (15), p. 366. It is an example of a conjunction of an intensional and an extensional verb, handled correctly by our rules for generalized conjunction. 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."

B2. Every man faced an enemy. (Partee 1984a, example 34)

That example and several other related ones may indeed be mine, but the topic of the "partly variable meaning" of many content words was first brought to my attention by my dissertation student Jonathan Mitchell, who noticed context-dependence with words like *local*, *faced, is approaching.* Mitchell finally completed his dissertation (Mitchell 1986), but did no more on the topic. I continued to develop it in (Partee 1989a), which includes example (**A25**) as well as (10) with *local*, attributed to Mitchell, and (11) with *notice*, from Dowty (1982).

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

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

The attributions of examples with *local* seem to have become a bit circular. In my two papers cited above, I present the examples with *enemy* as my own, but when I discuss examples with *local* in (Partee 1989a), I attribute them to Jonathan Mitchell. But Craige Roberts' memory is that I invented examples like (10) for Jonathan's dissertation. I think I did invent the specific example (10), but I think Jonathan had similar ones himself, except with something about a local newspaper rather than a local bar, and I think I was concerned that 'local newspaper' had an interfering and perhaps dominant reading in which a local newspaper is a kind of newspaper rather than whatever newspaper is near a given reference point.

B3. There were few faculty children at the 1980 picnic.

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. It was published with citation of Huettner in (Partee 1989b, p.395). 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, since 'all' as a proportion could never count as an instance of proportional 'few'.

B4. What do dogs sweat through their?

This was uttered by my son David Partee (at some young age, I've forgotten just when) as a quiz-type question to his younger brother Joel; the answer was supposed to be "feet", I think, if not "tongue". But David actually got some negative feedback, because Joel failed to understand the question until David reformulated it somehow. Emmon used the example in his 1996 LSA Presidential address.

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