Irene Heim in the
History of Formal Semantics.

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1. Introduction

- Irene Heim's work played a pivotal role in the "naturalization" of formal semantics, making it a component part of linguistic theory.

- In the first decade of Montague Grammar, there had been many important advances, with contributions relatively equally by linguists and philosophers/logicians.

- Montague's uniform treatment of noun phrases as generalized quantifiers was a radical development, and had led to a burst of work on determiners and generalized quantifiers, but puzzles about indefinites and anaphora remained.

- Heim's dissertation, which I consider a good candidate for best semantics dissertation ever, offered elegant solutions to a family of interconnected problems -- donkey anaphora, discourse anaphora, the semantics of indefinites, the nature of the distinction between definite and indefinite articles

Introduction, *cont'd.*

- Heim's dissertation introduced the fruitful ideas of "tripartite structures" and of meanings as context-change potential, and the radical idea that indefinites and definites may both be best viewed as variables accompanied by certain presuppositions and certain restrictions on their values.

- Heim built on work of Stalnaker, Lewis, and other philosophers, but her work was clearly linguistic in argumentation and execution, and marked an important step in bringing generative syntacticians and semanticists closer together, a step towards what I often refer to as the "naturalization of formal semantics".

- In this talk I want to put this work into its historical context, both in terms of the field as it was developing in the circles I was part of, and in terms of what was going on in Germany where Irene was studying before she came to UMass. Then I'll say a bit about her time at UMass and just after, her dissertation, and her legacy.

2. The first decade of formal semantics, 1968-77

- The work that developed into contemporary formal semantics can be traced mainly to Montague, David Lewis, Cresswell.

- Of course the story starts earlier and has other players, but those three probably had the biggest early impact.

- Within the US, the strongest influence came from Montague, through the work of Thomason, who collected and published Montague's papers with a substantial introduction in 1974, and Partee, whose "MG + TG" efforts started right after Montague's death.

- At UCLA late 60's: Montague, Lewis, Kaplan, Partee, Donnellan

- David Lewis's 1970 "General Semantics" was a great work and helped educate linguists, but he didn't continue developing it into a 'framework', although he continued making important contributions.

- I don't think we knew about Cresswell or his work until his 1973 book came out; I first got acquainted with him at the Linguistic Institute at UMass in 1974.

The first decade, *cont'd.*

- David Kaplan and I co-chaired Michael Bennett's dissertation (1974) after Montague's death in 1971.

- Terry Parsons' early work was less known: he had a big 'Semantics of English' ms with a combinatorial variable-free approach, first draft 1968, second in 1972, the year he and I both came to UMass. He never published it, but worked with me and then also Emmon and our students on developing the Montague grammar program.

- Terry was known for his early work on modifiers (1970), and became better known for his later work on events in the semantics of English (1985, 1990). He played an important role at UMass while he was there, from 1972 to about 1979.

- Emmon Bach came to UMass in 1973 and soon started working in the Montague Grammar framework as well; the three of us gave UMass a 'critical mass' in semantics. Terry and I had an NSF grant 1973-75, and Emmon and Ed Gettier participated in it. My first UMass Ph.D.s were Robin Cooper (1975), Muffy Siegel (1976), and Greg Carlson (1977), all with Emmon and Terry on the committee.

The first decade, *cont'd.*

- Ed Keenan was an insider-outsider: he wasn't part of any group, didn't follow Montague or Lewis or anyone else, but he organized a conference on Formal Semantics of Natural Language in 1973 while on a visiting appointment at Cambridge. (book published 1975)

- Quite a few of us met there for the first time – participants included David Lewis, Barbara Partee, John Lyons, Pieter Seuren, Östen Dahl, Colin Biggs, Hans Kamp, Renate Bartsch, Carl Heidrich, Arnim von Stechow, George Lakoff, Stephen Isard, Petr Sgall, Theo Vennemann, Yorick Wilks, Joe Emonds, Maurice Gross, Ed Keenan, Haj Ross, me, and some others.

- Groenendijk and Stokhof attended as student members of a search committee for the University of Amsterdam; there they interviewed Renate Bartsch, who moved to Amsterdam starting in 1974.

The first decade, *cont'd.*

- Hans Kamp was also an important early figure, in part because he worked closely with Montague while he was his PhD student and in one document Montague mentions him as a potential co-author of "English as a Formal Language". (Hans says he never heard that.)

- Although we overlapped at UCLA, I didn't meet him until the Keenan conference in 1973, I believe, and we didn't get well acquainted until the 1974 Linguistic Institute at UMass.

- That Institute included bringing together for 8 weeks a great group of linguists and philosophers working on semantics, as well as for putting UMass on the linguistic map.

- Participants in workshops and teaching included me, Emmon, Terry Parsons, David Lewis, Rich Thomason, Bob Stalnaker, David Dowty, Ray Jackendoff, Janet Fodor, Ed Keenan, Hans Kamp, Lauri Karttunen, Michael Bennett, Enrique Delacruz, and two graduate students, Anil Gupta (Thomason's student) and Robin Cooper (mine).

1974 Institute at UMass –

Terry, Barbara, Emmon

photo by Lauri Karttunen

The first decade, *cont'd.*

- 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, ed. (1976), 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.

The first decade, *cont'd.*

- What were people working on in the earliest years in 'my world'?

- Generalized quantifiers – Montague, Cooper, Peters, Partee

- Anaphora – Montague, Karttunen, Bach, Peters, Partee, Cooper

- Tense and aspect – Montague, Kamp, Bennett and Partee

- Categorial grammar etc – Montague, Lewis, Bach, Dowty

- Combinatory semantics – Parsons

- Plurals – Bennett, Carlson, Pelletier (mass)

- Combining MG + TG – Partee, Bach, Lewis, Siegel, Dowty, …

- Demonstratives – Kaplan, Bennett

- Pragmatics and context-dependence – Montague, Kaplan, Cresswell

- Questions – Karttunen

- Adjectives – Kamp, Parsons, Siegel

- Intensionality – Montague, Cresswell, Thomason

The first decade, *cont'd.*

- Of particular relevance to Irene's later work on her dissertation: what did people think about in the realm of definite and indefinite NPs, pronouns, donkey-sentences?

- Enchantment with Montague's and Lewis's demonstration that English syntax is not crazy, if all NPs can in fact be treated uniformly and categorematically as generalized quantifiers.

- Russell had thought it was crazy for a language to put *every man* in the same syntactic category as *Jones.*

- Montague and Lewis, also independently Parsons, showed that if you employ a higher-order logic, they can be treated uniformly.

- Generalized quantifier theory was accepted by formal semanticists; really blossomed a little later with the work of Barwise and Cooper.

- Indefinites and definites treated as GQ variants of familiar Russellian analyses as quantifiers. Puzzles only later.

The first decade, *cont'd.*

- For me and many others, Montague's theory not only gave a nice uniform NP semantics, but it also got rid of the presuppositions of the generative semantics – interpretive semantics war.

- You don't need unmotivated abstract syntax if your semantics can do real work. We had been held back by not knowing about higher order logic, type theory, intensional logic. The generative semanticists were trying to work compositionally, but their ideas about logical form were tied to first order logic plus a few operators.

- For me, "lambdas changed my life". But lambdas are not themselves essential – as Keenan has emphasized, it was understanding what you can do with *functions*, whether or not you use lambdas to express them. And that goes back to Frege.

The first decade, *cont'd.*

- **Anaphora**

- In syntax, even the distinction between bound variable anaphora and 'coreferential' anaphora was not widely appreciated before the late 70s. Reinhart's dissertation was 1976, her book 1983.

- Among semanticists, bound variable and 'coreferential' types were distinguished; not much more. Montague treated only bound variable anaphora explicitly; variables left free were regarded by followers as like demonstratives, getting value from the context.

- Puzzles well known from Geach, Karttunen, Bach & Peters, but formal semantic treatments had not progressed much beyond bound variable anaphora as in Montague, and some sort of 'coreferential anaphora', vaguely pragmatic.

- Not until Bach and Partee 1980 did reflexives even get added to MG, and some initial discussion of "fct-arg command".

The first decade, *cont'd.*

- Historical note on early generative grammar and the Stoics (Egli).

- Earliest treatment of anaphora in generative grammar was Lees and Klima (1963). Their rules involved substitution of a pronoun or reflexive for an identical NP.

- Lees and Klima focused on the differences in distribution between pronouns and reflexives, arguing with many interesting examples for the claim that the antecedent to a reflexive pronoun in English is always in the same clause, and the antecedent to a pronoun is always within a higher clause.

- Their work was the earliest precursor to Chomsky's Binding Theory, and some of their examples remain classic. There are many interesting and relevant constructions discussed in their work.

- (12) The men threw a smokescreen around themselves. (p.18)
  (13) The men found a smokescreen around them. (p.18)

The first decade, *cont'd.*

- (14) I told John to protect himself. (p18)
  (15) I told John to protect me.
  (16)*I told John to protect myself.

- (29) John bought Mary a car to drive herself around in. (p.20)
  (30) John bought Mary a car to drive him around in. (p.20)

- But in their work, and even with the later addition of "referential indices" on noun phrases to indicate coreference in (Chomsky 1965), almost all the examples used proper names.

- Thus the history of generative grammar had to repeat the history of the Stoics (4th – 3rd centuries BCE), as noted by (Egli 2000):

- (4) If somebody/a man is in Athens, it is not the case that he is in Rhodes.

- (5) If Socrates is in Athens, it is not the case that he is in Rhodes.

- If I try to apply this [substitution] theory to sentence (4), I quickly come to a kind of paradox for which the Stoics had a proper designation: it is the *nobody paradox*.

The first decade, *cont'd.*

- Previous to the discussion of these data definite pronouns were all treated [by the Stoics] as deictic – demonstrative –, but then the Stoics had to consider two uses of pronouns: a deictic one and an anaphoric one; the latter being anaphorically related to an antecedent. (Egli 2000, pp. 19-20)

- In the case of the Stoics, solving the pronoun problem led "the Stoics [to invent] a variant of predicate logic, in addition to the propositional logic contained in their theory of syllogisms, as they used both constants going beyond propositional logic, as well as predicate and subject variables in the form of demonstratives in their texts" (Egli 2000, p.20).

- An interesting historical snapshot can be seen in the Pronominalization chapter of the book (Stockwell et al. 1973), written from 1966 to 1968 and then slightly revised for publication. That work was a big grammar of English that discussed and attempted to synthesize what had been done in transformational syntax (of English) up until late 1966.

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- An interesting historical snapshot can be seen in the Pronominalization chapter of the book (Stockwell et al. 1973), written from 1966 to 1968 and then slightly revised for publication. That work was a big grammar of English that discussed and attempted to synthesize what had been done in transformational syntax (of English) up until late 1966.

- Although the linguistic wars had begun in earnest by then, we tried to stick to syntax, and to be relatively faithful to the *Aspects* model (Chomsky 1965), with only a few post-1966 innovations, mainly the addition of Fillmore's (1968) Case Grammar and the lexicalist hypothesis of (Chomsky 1970, circulated in early 1968).

- We discussed the semantic problems; our "solution" was to adopt rules like those of Lees and Klima (1963), without Chomsky's referential indices, explicitly giving up the Katz-Postal hypothesis. We made both the pronominalization rule and the reflexivization rule optional, letting the choice to apply the rule correspond to a decision to regard the two noun phrases as coreferential.

The first decade, *cont'd.*

- As you can imagine, it was slightly schizophrenic for me by the end of that project, when I was working hard on 1965-style syntax while simultaneously getting my first introduction to Montague's work and beginning my path from syntactician to semanticist.

- The Stockwell, Schachter and Partee book points at a number of semantic problems that faced the existing syntactic analyses, but leaves it to future work to solve them. E.g.,

- Thus for example no simple unitary referential index feature will account properly for the following:

- (44) (a) Every philosopher argues with himself.
           (b) Every philosopher argues with every philosopher.

- (45) (a) Only Lucifer pities himself. (Geach 1962)
  (b) Only Lucifer pities Lucifer.

- (46) (a) Most of the boys expect most of the boys to pass.
  (b) Most of the boys expect the boys to pass.
  (c) Most of the boys expect to pass.

3. In Germany – Irene and early formal semantics

- (Much thanks here to Irene, Angelika, Arnim for interviews.)
- Early formal semantics in Germany –von Stechow was at the center.
- Konstanz university founded in 1968 or 69, started full operations around 1970. Founding documents name formal work on language as something they should excel in.
- Beginning of 1970s – Arnim came to Konstanz from Münster, one of the first hires, part of the founding group.
- Irene has a paper called 'Formale Semantik' by Arnim von Stechow published in 1971, from a 1970 conference in Regensburg. It's Arnim's first semantics paper, and maybe the first use of the term 'formal semantics' in linguistics. (The earliest I had known of was Ed Keenan's, for his 1973 conference.)
- Irene was nearing the end of high school in Munich, where she was resisting being pigeonholed as science or humanities, since she was interested in math and philosophy and had read about attempts to treat language formally.

In Germany – Irene and early formal semantics, cont'd

- University career – Munich – Konstanz – Munich, 1972 – 76-ish.

- At Munich she first had a logic course with Blau and some unexciting courses on linguistics; then in 2nd semester a great class in Philosophy, 'Speech Act Theory in Linguistics and Philosophy' with Gunter Grewendorf – in her third semester she gave a presentation on the performative hypothesis and critiques of it.

- Around the end of the second semester, she wrote to Peter Lutzeier for advice (because he did "Linguistics and Mathematics"). He replied (July 73) with a good 5-page letter encouraging her interests and describing linguistics in departments all over Germany.

- About Konstanz he wrote, "fast paradiesiche Zustände, … Traditionell Schwerpunkt Syntax, jedoch wollen sie nun auch verstärkt Semantik treiben (Stechow). …"

- That summer she looked at some places, fell in love with Konstanz, and moved there starting Fall 1973.

In Germany – Irene and early formal semantics, cont'd

- Fall 1973 class with Arnim -- read Schnelle's translation of UG. Liked the group of people there. Important influences Cresswell and Lewis as well as Montague. (In Angelika's time, little Montague.)

- When she arrived in Konstanz, Arnim, Angelika and Eberhard Pause had just finished a book on syntax of German, a CFG that dealt with many complex phenomena. So at that point the attitude there was that transformational grammar was a thing of the past. (And when GPSG came later, they weren't that impressed.)

- Then they turned their attention to working on the formal semantics of German, working largely in the style of Cresswell.

- After 2 years in Konstanz in linguistics, she felt a lack of attention to foundational issues – how are the concepts of linguistics grounded in empirical reality, etc. ("a bit of behaviorist influence"), and went back to Munich to do Philosophy some more..

In Germany – Irene and early formal semantics, cont'd

- Did her MA there in Philosophy, finishing in 9th semester, with minors in linguistics (PTQ exam …) and mathematics. Spent a lot of time in Konstanz at the same time.

- More lasting connections with Konstanz than with Munich in the end.

- Her MA thesis, on some relations between the theoretical concepts of speech act theory and those of formal semantics was in a sense an attempt to convince the ordinary language philosophers at Munich and the formal semanticists at Konstanz that they should have some interesting things to say to each other.

- Towards the end, she "got enough of foundations" and wanted to go back into linguistics.

- In summer 1975 she had written to David Lewis about wanting to spend a year visiting and working with him, supported by a stipend from the Studienstiftung des deutsches Volkes. He wrote a friendly but discouraging reply, explaining that a student can't simply come and 'visit for a year'.

In Germany – Irene and early formal semantics, cont'd

- Before the next year, she got a lot of useful advice from Max Cresswell about how the system works, and wrote to people at all the universities in the US that seemed to have some good formal semantics: Princeton, Stanford, UCLA, Pittsburgh, UT Austin, and UMass Amherst. And it happened that the only real letter she got back (as opposed to just brochures) was from me at UMass. This was in the summer of 1976.

- So when she finished her MA, she came to UMass in the Fall of 1977.

4. Irene at UMass, 1977 - 1981

- Irene had learned that she had to pretend to be enrolling in the PhD program to be able to spend a year, even with outside funding. But she made it clear to us that she didn't really plan to stay.

- But she reports that when she came, she found it wonderful. It seemed like the best of what she'd seen before, like in the SfB project in Konstanz, but with more faculty members involved and with many more advanced serious semantics students concentrating on it.

- The level of work in Konstanz had been excellent, but the critical mass was at a different order of magnitude at UMass.

- And although at first she had wanted to just take courses in semantics and work with Emmon and me (Terry was away her first year), she's glad that someone advised her to take the full first-year program, just in case. She did that, was pleasantly surprised and impressed by syntax and phonology as well, and decided she wanted to stay for the Ph.D.

Irene at UMass, 1977 – 1981, *cont'd.*

- She reports that what was a real eye-opener besides the amount of work going on in semantics was the discovery that there are impressive people and exciting work in syntax and phonology.

- She was impressed by her first year courses in syntax with Edwin Williams and in phonology with Alan Prince. And in Edwin's course, she realized that she needed to reconsider her negative opinions about MIT syntax.

- And in fact it turned out to be not only her – after she was already in graduate school and people like Pesetsky and Stowell were studying at MIT, and the Pisa lectures came out, people back in Germany like Arnim and Angelika started to get interested in Chomskian syntax again.

- So when formal semanticists like Emmon and me were impressed by the possibilities GPSG or categorial grammar or other monostratal theories, for Irene and the Konstanz group, that was nothing new – "Been there, done that."

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Irene at UMass, 1977 – 1981, *cont'd.*

- Because of her strong background, Irene was in my second-year course on Montague Grammar in her first year.

- She reports that what was new to her there was that as we studied Montague's work, we were always thinking about how to extend it, how to do better.

- We paid attention to "fancy pronoun problems", which she'd never encountered before – she hadn't been exposed to linguists puzzling about problems of anaphora. She had never thought before about pronouns, binding, coreference, 'pronouns of laziness'. That was the most new thing to her in semantics on her arrival.

- Konstanz had been more advanced in the study of context-dependency – cf. Angelika's dissertation. The 'bible' there was Cresswell's 1973 book *Logics and Languages*, not PTQ.

- Angelika was working on modals, Bäuerle was working on tense, context-dependency issues were central. Pronouns, anaphora, quantifiers were not high on the agenda in Konstanz.

Irene at UMass, 1977 – 1981, *cont'd.*

- So what was new and impressive to Irene in the US was all the thinking about anaphora, and learning syntax and phonology and getting some sense of the bigger picture of generative grammar.

- In the general Chomskian world, she saw good interaction across the subfields. Konstanz hadn't had 'fully rounded' linguistics.

- So it is not accidental that her dissertation was the first in formal semantics to incorporate a Chomskian kind of syntax rather than something like Montague grammar or GPSG or categorial grammar.

- What she observed around her was "either you were a syntactician who really didn't know what a quantifier was, really, or you were a semanticist and you had to do categorial syntax or GPSG or whatever, and it just didn't seem that the connections were that logical between these choices, and that was a point I wanted to make in my dissertation."

Irene at UMass, 1977 – 1981, *cont'd.*

- I think I had been stuck at the earlier stage: If you have some serious semantics, then you don't have to show things to be related by reducing them to a common deep structure; so you have less need for transformations at all.

- But as she saw it, having some serious semantics "makes the game harder" … "once you knew that you *could* do it this way or that way, there was still a question about which way you *should* do it."

- But before saying a few words about her dissertation, let me fill in a little more about who was there then, and who she interacted with.

- On the faculty, given that Terry Parsons was away her first year and had left altogether before she finished, she had less interaction with him than students who entered before her. She had his seminar with Emmon on tense and aspect, and his seminar that went with his book *Nonexistent Objects.*

- Her dissertation committee was me, Emmon, Edwin, and Ed Gettier. And Arnim and especially Angelika were very much involved.

Irene at UMass, 1977 – 1981, *cont'd.*

- Fellow students doing semantics: Only three had already finished before she arrived – Cooper, Siegel, and Carlson.

- Starting before her: Paul Hirschbühler (1974 – 1978), Rick Saenz (1975 - …) and Ken Ross (1975 – 1981), Mark Stein (1976 – 1981).

- Starting at the same time: Elisabet Engdahl (1977 - 1980) and Jonathan Mitchell (1977 – 1986).

- Starting after she did: Mats Rooth (1978 – 1985), Gennaro Chierchia (1979 - 1984).

- She spent a lot of time talking with Paul Hirschbühler as he was working on his dissertation on multiple-WH questions, and she also talked a lot with Elisabet Engdahl, who was working on 'functional questions' (*Which picture of himself did every applicant submit?*)

- Irene's paper at the first Konstanz workshop on formal semantics in 1978 was on concealed questions (Heim 1979).

- She liked the constructive engagement in our community -- critical but trying to make positive suggestions. Edwin a great model of that.

5. Indefinites and the introduction of dynamic semantics.

- The preface to Irene Heim's 1982 dissertation begins:

In November 1978, a workshop was held at the University of Massachu-setts whose title was "Indefinite Reference" and whose topic Barbara Partee described in a circular that started as follows:

One standard view among logicians is that indefinite noun phrases like 'a tall man' are not referring ex-pressions, but quantifier phrases, like 'every man', 'no man', and 'most men'. Yet in many respects, in-definite noun phrases seem to function in ordinary language much like definite noun phrases or proper names, particularly with respect to the use of pro-nouns in discourse. This may be simply a matter of sorting out semantics from pragmatics, but there is not to our knowledge any currently available theory that simultaneously characterizes the logical or truth-functional properties of indefinite noun phrases and accounts for their 'discourse-reference' properties....

Indefinites and dynamic semantics, *cont'd.*

- That workshop was part of our Sloan-funded Cognitive Science program. I brought together an interdisciplinary group to try to break through the impasse concerning indefinite NPs, which seemed neither straightforwardly referential nor straightforwardly quantificational. Participants included

- Terry Parsons, Philosophy, UMass

- Ellen Prince, Linguistics, Penn

- Keith Stenning, Psychology, Liverpool

- Gary Hendrix, AI and Computational Linguistics, SRI International

- Asa Kasher, Philosophy, Bar Ilan

and some others. Attendees included both Irene Heim and Hintikka's student Lauri Carlson, visiting from Finland.

- Both Irene and Lauri realized that the problem was interesting and that no one had a solution, and both started working on it.

Indefinites and dynamic semantics, *cont'd.*

- Right after the workshop, Irene wrote "Toward a unified semantics of definite and indefinite noun phrases," treating both as variables.

- But she became disaffected with that. But in summer 1980 she saw how differences could come from different presuppositions of definites and indefinites. Then the dissertation took almost 2 years.

- The questions raised in the workshop concerned indefinites and their puzzling nature, and did not include anything about definites. It was Irene's idea to work on the two classes of expressions together, to try to capture not only their differences, but how they are similar to each other and different from real quantificational NPs.

- Terry had presented a paper "Pronouns as paraphrases" at the workshop, with a procedure for constructing a definite description capturing the content of a pronoun with an indefinite antecedent; Irene criticized it, I thought convincingly, but some years after her dissertation, she espoused a somewhat similar "E-type" theory.

Indefinites and dynamic semantics, *cont'd.*

- A family of five problems:
- **Discourse anaphora.**

(1) a. John/the man/a man walked in. <u>He</u> looked tired.

    b. #Every man/ no man/ more than one man walked in. <u>He</u> looked   tired.

- **Different "discourse" behavior of logically equivalent sentences.** (This argues against a purely pragmatic account of the differences in (1a-b).)

(2) a. One of the ten marbles is not in the bag. It is probably under the sofa.

    b. Nine of the ten marbles are in the bag. ??It is probably under the sofa. (Example in Heim's 1982 dissertation, from Partee)

- **Indefinite introduces a "discourse referent" with limited lifespan.**

(3) a. John wants to catch a fish and eat it. (Karttunen)

    b. Maybe he would share it with me. (Modal subordination, Roberts)

    c. #It's probably under the boat now.

Indefinites and dynamic semantics, *cont'd.*

- **The two connected problems of "donkey sentences"**

(4) a. *Every man who owns <u>a donkey</u> beats <u>it</u>.*

   b. *If <u>a man</u> owns <u>a donkey</u>, <u>he</u> always beats <u>it</u>.*

- Problem 1: How to explain that *it* can be anaphoric to *a donkey* in these sentences?

- Problem 2: How to explain that *a donkey* seems like a *universally* quantified NP in these sentences? (And in (4b), same for *a man.*

- **The quantificational variability of indefinites.**

(5) An equation like this *always/often/sometimes/never* has more than one solution. -- paraphrases with 'all/many/some/no equations like this'.

- **Solution** (Heim/ Kamp): Indefinite NPs are not quantifier phrases.

- An indefinite "introduces a new file card/ a new discourse referent".

- Heim 1982 file change semantics, Kamp 1981 DRT are **dynamic** theories: replace truth conditions by "context change potential".

- Truth of a file: embeddability of the file ('partial model') in a model.

Indefinites and dynamic semantics, *cont'd.*

- Irene Heim and Hans Kamp came up with their theoretical innovations in response to this family of problems virtually simultaneously and independently.

- Heim's dissertation is a classic. One important thing she did is to treat indefinites and definites both, treating both as "variables" of type e with no independent quantificational force of their own.

- The difference between indefinites and definites consists in two things:
  - (i) for an indefinite, pick a new variable, for a definite pick an old one. (This is the core of the "anaphoric theory of definites").
  - (ii) the NP content of an indefinite enters into asserted content; the NP content of a definite is presupposed.

- Both Heim and Kamp had accounts of the "lifespan" of a discourse referent, with crucial differences between simple sentences or sequences thereof vs. quantified sentences.

Indefinites and dynamic semantics, *cont'd.*

- Both Heim and Kamp built on Stalnaker's theory of how assertions update the "common ground".

- Dynamic semantics: The interpretation of sentences not only depends on the context, it also affects the context.

- Heim: the semantic content of a sentence is its "context change potential" (CCP). And this change goes down to the smallest parts, so the contribution of each part is its contribution to the context change potential of the whole. Big theoretical change.

- Truth conditions are still there, derivatively. CCP primary.

- In Heim's "file change semantics", a "file" is true w.r.t. a model if it is embeddable in that model (disc. referents mapped onto entities in the model, etc). A formula is true if updating a true file with it makes a true file, false if updating a true file with it makes a false file.

- Where does the apparent existential quantifier in the interpretation of an indefinite come from? "File is true if *there is a way* to embed it."

Indefinites and dynamic semantics, *cont'd.*

- Heim did not build her approach into a whole research program, and soon afterward changed her views on anaphora with indefinite antecedents, though many still like the approach in her dissertation better than her later approach, or consider that both are needed.

- Kamp did pursue his Discourse Representation Theory (DRT) and has made it into a large-scale research program. Asher and others have extended it to include more pragmatics, more text-coherence relations.

- Kamp argued that DRT was not straightforwardly compositional and shouldn't be.

- Groenendijk and Stokhof responded with a fully compositional new kind of logic, Dynamic Predicate Logic, extended to Dynamic Montague Grammar. Chierchia extended it and applied it to a lot of difficult anaphora problems in his 1995 book.

- Reinhard Muskens showed how to reconcile Kamp with Groenendijk and Stokhof with his 1993 "Compositional DRT".

6. Heim's legacy

- Thumbnail followup:

- 1981-82: "Postdoc" at Stanford (following Engdahl) Dissertation actually turned in summer 1982 in time for PhD September 1982.

- Applied for position at MIT for 1982; they hired Higginbotham, she got a postdoc 82-83.

- Then Texas – which got better by the time she left (Ph.D. chair for Carl Weir 1986, Maria Bittner (co-chair with Kamp) 1988, Toshi Ogihara (co-chair with Kamp) 1989, and there were some good younger ones)

- Then hired at MIT in 1989. Great many PhD students there!

- Partial list: Utpal Lahiri 1991, Diana Cresti 1995, Orin Percus 1997, Uli Sauerland (co-chair) 1998, Danny Fox (co-chair) 1999, Martin Hackl 2000, Paul Elbourne 2002, Michela Ippolito 2002, Elena Guerzoni 2003, Jon Gajewski 2005, Valentine Hacquard 2006, Ezra Keshet 2008, Michael Erlewine (co-chair) 2014

Heim's legacy, *cont'd.*

- Legacy includes:

- Opening up of issues about context change, bringing Stalnaker's perspective into linguistic discussion. Foundational and substantive at the same time.

- Bringing formal semantics and Chomskian syntax together, leading to explosion of work on syntax-semantics interface and much-improved communication.

- I tried to do it in the early 70's, then gave up and turned to non-transformational approaches instead, mostly stayed agnostic about syntax in order to be compatible with colleagues.

- Led to formal semantics being more within linguistic theory, reflected in new conference SALT (1991), new journal Natural Language Semantics (1992).

- Substantive legacy in approaches to presupposition, dynamics of domain restriction, many other areas besides NP semantics and anaphora. We'll hear more this weekend!

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