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FORMAL SEMANTICS: ORIGINS, ISSUES, EARLY IMPACT

ABSTRACT: Formal semantics and pragmatics as they have developed since the late 1960's have been shaped by fruitful interdisciplinary collaboration among linguists, philosophers, and logicians, among others, and in turn have had noticeable effects on developments in syntax, philosophy of language, computational linguistics, and cognitive science.

In this paper I describe the environment in which formal semantics was born and took root, highlighting the differences in ways of thinking about natural language semantics in linguistics and in philosophy and logic. With Montague as a central but not solo player in the story, I reflect on crucial developments in the 1960's and 70's in linguistics and philosophy, and the growth of formal semantics and formal pragmatics from there. I discuss innovations, key players, and leading ideas that shaped the development of formal semantics and its relation to syntax, to pragmatics, and to the philosophy of language in its early years, and some central aspects of its early impact on those fields.

1. INTRODUCTION

1.1. "Semantics" can mean many things

"Semantics" can mean many different things, since there are many ways to be interested in "meaning". One recurring topic of debate in the twentieth century has concerned how much common ground can be found across logic, philosophy, and linguistics.

Formal semantics, a discipline founded on an optimistic answer to that question, has been shaped over the last forty-plus years by fruitful interdisciplinary collaboration among linguists, philosophers, and logicians. In this paper I'll reflect mainly on the development of formal semantics and to a lesser extent on formal pragmatics in linguistics and philosophy starting in the 1960's. I'll describe some of the innovations and "big ideas" that have shaped the development of formal semantics and its relation to syntax and to pragmatics, and draw connections with foundational issues in linguistic theory, philosophy, and cognitive science.

I'm not trained as a historian of linguistics or of philosophy[1]; what I know best comes from my experience as a graduate student of Chomsky's in syntax at M.I.T. (1961-65), then as a junior colleague of Montague's at UCLA starting in 1965, and then, after his untimely death in 1971, as one of a number of linguists and philosophers working to bring Montague's semantics and Chomskyan syntax together, an effort that Chomsky himself was deeply skeptical about.

"Semantics" traditionally meant quite different things to linguists and philosophers, not surprisingly, since different fields have different central concerns. Philosophers of language have long been concerned with truth and reference, with logic, with how compositionality works, with how sentence meanings are connected with objects of attitudes like belief, with the semantic analysis of philosophically important terms, with the nature and ontological status of "meanings". Linguists at least since the Chomskyan revolution have been concerned with human linguistic competence – what's "in the head" of the speaker of a language, and how it's acquired—and about the architecture of grammar, including the nature of the interface between syntax and semantics. And here I'm really only speaking of 'analytic philosophy' and 'formal linguistics', two relatively compatible and 'science-friendly' schools of thought within the broader fields of linguistics and philosophy.

Different research methodologies in different fields also lead to different research content. Phonology and morphology influenced the use of "semantic features" in early linguistic work. Field linguists and an-

thropologists have used componential analysis and structural methods to study kinship systems and other systematic patterns, usually within the lexicon, reinforcing the idea of analyzing the meanings of words as bundles of distinctive features. An emphasis on studying semantics at the lexical level can also be found in psychology, where the study of semantics often means the experimental study of concept discrimination and concept acquisition. With the growth of generative grammar, linguists began to think more about semantics above the lexical level. There syntax has strongly influenced linguists' notions of "logical form": 'structure' of meaning suggests 'tree diagrams' of some sort. Logicians, on the other hand, are accustomed to building formal systems with axioms and model theoretic interpretations. 'Structure' to a logician is more likely to suggest 'inferential patterns'.

These sorts of differences in thinking about semantics help to explain why it took some time for linguists and philosophers to appreciate each other's work, and even longer to be able to work together, and why even now, decades after the field of formal semantics was built up through the cooperative work of philosophers, logicians, and linguists, it is still by far not the only approach to semantics. Some prefer a different approach because they have a different set of questions or interests or preferred research methodologies. Some share the goals of formal semantics but prefer a different approach because they are not convinced that formal semantics can be a successful path to those goals.

1.2. *The roots of formal semantics*

Formal semantics has roots in several disciplines, most importantly logic, philosophy, and linguistics. The most important figure in its history was undoubtedly Richard Montague (1930-1971), whose seminal works in this area date from the late 1960's and the beginning of the 1970's. (There were of course many other important contributors, as I'll mention.)

The development of formal semantics over the past forty and more years has been a story of fruitful interdisciplinary collaboration among linguists, philosophers, logicians, psychologists, and others, and by now formal semantics can be pursued entirely within linguistics as well as in various interdisciplinary settings, including cognitive science, in-

formatics, and computational linguistics. In the U.S. formal semantics is mostly within linguistics departments now, but in parts of Europe (e.g. Amsterdam) it's strongly embedded in the context of logic and philosophy.

My main goal is to help contemporary students of formal semantics understand where various ideas and approaches came from, and why some things are the way they are now. I also want to convey some of the sense of excitement of the beginnings of formal semantics, including how surprising some of the philosophers' and logicians' ideas were to linguists at first, and vice versa. This may also help make it easier to read earlier papers, when the differing assumptions of philosophers and linguists were not explicit, but certainly affected argumentation. For that reason, I put major emphasis on the late 1960's, when Montague did his influential work and the early 1970's, when the early development of formal semantics through cooperation between linguists and philosophers began in earnest. In this paper I will not really try to go much beyond the middle of the 1970's, by which point work in formal semantics was starting to grow and diversify at a rapid pace.

First I will describe earlier developments in linguistics (Section 2) and in philosophy and logic (Section 3) that led up to the period in which Montague did his work. Section 4 is about Montague's work—its origins and its central ideas. Section 5 describes early interactions among linguists and philosophers and the beginnings of the development of Montague grammar and related approaches. The early impact of Montague's and related work on linguistics, and reactions to formal semantics, are discussed briefly in Section 6. Section 7 is a very brief glimpse at some of the further development of formal semantics and the beginnings of formal pragmatics, and just a few words about more recent developments, the current outlook, and some of the lasting impacts of formal semantics on linguistics and related fields.

2. SEMANTICS AND GENERATIVE GRAMMAR: FROM BEFORE SYNTACTIC STRUCTURES TO THE LINGUISTIC 'WARS'

2.1. *Before Syntactic Structures*

Linguistics has the interesting property of straddling the boundaries of the humanities, social sciences, and natural sciences. The Chom-

skyan perspective is definitely one of linguistics as a science, but that didn't start with Chomsky. In Europe, linguistics came from philology, to which it often still belongs or has close ties. And it was there in the nineteenth century that German historical philologists known as the *Junggrammatiker* made their breakthrough discoveries about the sound changes through which they could establish the evolutionary history of the Indo-European languages. Darwin in his *Origin of Species* "said that linguistics, as practiced by the leading exponents of comparative Indo-European philology, offers *the* paradigm of scientific method." (Harris & Taylor 1997, p. 187) And a little later in France, De Saussure developed a competing approach to structural linguistics with an emphasis on synchronic description, also concerned to put it on a scientific footing.

In the U.S., linguistics was often a part of anthropology, with fieldwork a main activity, and the writing of grammars of indigenous languages one of the main goals. In both Europe and the US, there was a self-conscious drive to view linguistics as a science in the 1930's, partly under the influence of the Vienna Circle; linguistics was part of the Vienna Circle's "unified science" movement, with the semiotician Charles Morris as one of its leaders. In the postwar period, it was evidently advantageous in terms of both prestige and access to funding for one's field to be considered a science; the Mathematical Social Sciences Board was founded in 1965 through the cooperation of the Social Sciences Research Council and the Center for Advanced Study in the Behavioral Sciences, with psychologists such as C. Duncan Luce, later a co-author of Chomsky's on formal language theory, among its leaders.

Part of the Chomskyan revolution was to view linguistics as a branch of psychology, and one of his early successes was his attack on behaviorism in psychology, making linguistics an important contributor to the rise of modern cognitive science.

Semantics tended to be rather neglected in early and mid-20th century American linguistics. There were probably several different kinds of reasons for this[2]. There had been rather little semantics in early American anthropological linguistics, since in doing linguistic fieldwork it seemed necessary to start with phonetics, then phonology, then morphology, then perhaps a little syntax, and the only semantics came in working on dictionaries, and perhaps in working out the semantic

features crucial for the structural analysis of particular lexical domains such as kinship terms. And the behaviorists viewed meaning as an unobservable aspect of language, not fit for scientific study, and that had some influence on the Bloomfieldians. Quine had strong skepticism about the concept of meaning, and had some influence on Chomsky.

At the same time there was great progress in semantics in logic and philosophy of language, as will be discussed in Section 3, but that was relatively unknown to most linguists.

Then in 1954, Yehoshua Bar-Hillel wrote an article in Language (Bar-Hillel 1954a) inviting cooperation between linguists and logicians, arguing that advances in both fields would seem to make the time ripe for an attempt to combine forces to work on syntax and semantics together.

But Chomsky immediately wrote a reply in Language (Chomsky 1955) arguing that the artificial languages invented by logicians were too unlike natural languages for any methods the logicians had developed to have any chance of being useful for developing linguistic theory[3].

While Bar-Hillel remained in close contact with Chomsky, and the two discussed issues in the formal theory of grammars, he did not make another public attempt to persuade linguists to work together with logicians. But he did continue to try to bring the two fields together: in 1967, he wrote to Montague, after receipt of one of Montague's pragmatics papers: "It will doubtless be a considerable contribution to the field, though I remain perfectly convinced that without taking into account the recent achievements in theoretical linguistics, your contribution will remain one-sided."[4]

2.2. *Syntactic Structures*

In *Syntactic Structures* (Chomsky 1957), Chomsky is quite ambivalent about semantics. He spends many pages arguing that semantic notions are of no use in constructing a grammar, and arguing that intuitions of grammaticalness are distinct from intuitions of meaningfulness. "Grammar is best formulated as a self-contained study independent of semantics. In particular, the notion of grammaticalness cannot be identified with meaningfulness." (p.106)

But at the same time he holds that one test of a good syntax is

that it should provide a good basis for a good semantics (if we had any idea how to study semantics). "In other words, we should like the syntactic framework of the language that is isolated and exhibited by the grammar to be able to support semantic description, and we shall naturally rate more highly a theory of formal structure that leads to grammars that meet this requirement more fully." (p.102)

And he argued that transformational grammar is a positive step in that direction, since it uncovers differences at the "transformational level" (what would later be reworked as "deep structure") that are obscured in the output (later "surface structure"). "The general problem of analyzing the process of "understanding" is thus reduced, in a sense, to the problem of explaining how kernel sentences are understood, these being considered the basic 'content elements' from which the usual, more complex sentences of real life are formed by transformational development." (p.92).

He argues, for instance (pp. 88–89), that the phrase (1a) has an ambiguity that cannot be captured at the level of phrase structure, where it has just a single description, but is transformationally derived from two different kernel sentences, (1b) and (1c).

(1)   a.   the shooting of the hunters
      b.   The hunters shoot.
      c.   They shoot the hunters.

He takes this as an example of a property that grammars should have: "This suggests a criterion of adequacy for grammars. We can test the adequacy of a given grammar by asking whether or not each case of constructional homonymity is a real case of ambiguity and each case of the proper kind of ambiguity is actually a case of constructional homonymity." (p.86)

But Chomsky also notes that transformations sometimes change meaning. "...we can describe circumstances in which a 'quantificational' sentence such as ⌈(2a)⌉ may be true, while the corresponding passive ⌈(2b)⌉ is false, under the normal interpretation of these sentences—e.g., if one person in the room knows only French and German, and another only Spanish and Italian. This indicates that not even the weakest semantic relation (factual equivalence) holds in general between active and passive." (pp. 100–101)

(2)   a.   Everyone in this room knows at least two languages.
      b.   At least two languages are known by everyone in this room.

In later years, those judgments about (2) came to be questioned; some argued that (2b) is ambiguous, some argued that both are. Chomsky himself noted problems with the judgments and their diagnosis when he discussed the same examples in (Chomsky 1965). Difficulties with such data continued for many years, sometimes with arguments in favor of competing theories built in part on competing judgments about what the data were that should be captured. No perfect methodologies for settling such debates have been found; but over time, linguists have become more aware of the need for care in eliciting judgments and have developed more subtle ways to get data than just asking about their own or their consultants' intuitions.

2.3.   Katz and Fodor's Semantic Component

Jerrold Katz and Jerry Fodor were the first to start working on adding a semantic component to generative grammar (Fodor 1961, Katz 1961, Katz & Fodor 1962, Katz & Fodor 1963). They were concerned with compositionality, which they generally called the Projection Problem: how to get the meaning of a sentence from meanings of its parts. Hodges (1998) identifies their 1963 paper as the first use of the term compositionality. "As a rule, the meaning of a word is a compositional function of the meanings of its parts, and we would like to be able to capture this compositionality" (p. 501 in the version reprinted in (Fodor & Katz 1964)).

> "Since the set of sentences is infinite and each sentence is a different concatenation of morphemes, the fact that a speaker can understand any sentence must mean that the way he understands sentences he has never previously encountered is compositional: on the basis of his knowledge of the grammatical properties and the meanings of the morphemes of the language, the rules the speaker knows enable him to determine the meaning of a novel sentence in terms of the manner in which the parts of the sentence are composed to form the whole. Correspondingly, then,

we can expect that a system of rules which solves the pro-
jection problem must reflect the compositional character
of the speaker's linguistic skill." (p. 482 in the version
reprinted in (Fodor and Katz 1964))

At that time, "Negation" and "Question Formation" were trans-
formations of affirmative declaratives. They were prime examples of
meaning-changing transformations.

So meaning depended on the entire transformational history. "P-
markers" (phrase structure) were extended to "T-markers", to which
semantic Projection rules applied. Katz and Fodor's idea of computing
the meaning on the basis of the whole T-marker can be seen as aim-
ing in the same direction as Montague's derivation trees: the steps in
the semantic interpretation reflect the steps in the syntactic derivation
(what Emmon Bach later christened the "Rule-by-rule" approach to the
relation between semantics and syntax.) I give a simple example to il-
lustrate (oversimplified) the derivation and interpretation of a negative
sentence.

The derivation of the sentence *The airplanes will not fly* proceeds
by first constructing the (positive) kernel sentence in (3a) with phrase-
structure rules, and then applying the optional negation transformation
T-NEG to (3a) to derive the final result (3b).

(3)   a.   [The airplanes [will [fly]]] ("deep structure" or "kernel
             sentence") ⇒T-NEG
        b.   [The airplanes [will not [fly]]] ("surface structure" or "de-
             rived sentence")

The T-marker for (3b) includes the P-marker for its deep structure
(3a) plus a graph showing what transformations have been applied in
its derivation. The structure of the underlying "kernel sentence" in (3a)
is interpreted by "Type 1 projection rules". Then a "Type 2 projection
rule" corresponding to the negation transformation applies to yield the
interpretation of the resulting sentence.

Katz and Fodor thus took compositionality seriously at the very out-
set of their work on semantics. But their semantic tools were very
primitive. Katz and Fodor worked with "semantic features", and their
semantic representations were "bundles of features"—suitable at best
for decompositions of one-place predicates.

Quine (1970) had a typically felicitous characterization of how
compositionality works from a logician's perspective: "Logic chases
truth up the tree of grammar" (p.35); Katz and Fodor's position might
be characterized: "Semantic projection rules chase semantic features
up the tree of grammar."

Later they started adding some bits of structure to try to handle
transitive verbs and their two arguments, but 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. They also tried to capture a notion of
analyticity, but only for copular sentences. The examples of what they
could capture didn't seem very exciting, and the accounts were some-
times open to easy counterexamples[5].

2.4. Philosophers' reactions to linguists' "semantic representations"

One early and influential critique of Katz and Fodor's approach (and
the approach of Katz and Postal to be discussed in section 2.5) was
Vermazen's (1967). The most famous reaction, citing and in broad
agreement with Vermazen's, was David Lewis's:

> "But we can know the Markerese translation of an English
> sentence without knowing the first thing about the mean-
> ing of the English sentence: namely, the conditions under
> which it would be true. Semantics with no treatment of
> truth conditions is not semantics." ..."Translation into
> Markerese is at best a substitute for real semantics, rely-
> ing either on our tacit competence (at some future date)
> as speakers of Markerese or on our ability to do real se-
> mantics at least for the one language Markerese." (Lewis
> 1970, p.1)

But linguists *did* presuppose tacit competence in Markerese; they
took it to be universal and innate, and many (e.g. Ray Jackendoff,
Jerry Fodor) still do take that or some kind of semantic representation

language to be universal and innate. To philosophers and logicians doing formal semantics, the language of Markerese looked empty, since it was uninterpreted.

To linguists, concern with *truth* looked puzzling. Linguists were trying to figure out mental representations that would underlie linguistic competence. "Actual truth" was (correctly) considered irrelevant, and truth conditions were not really understood or appreciated. When the linguistic relevance of truth *conditions* finally penetrated (later), the very nature of linguistic semantics changed—not just in terms of the tools used, but also in the questions asked and the criteria of adequacy for semantic analyses[6].

2.5. Katz and Postal: Deep Structure as Input to Semantics

In a theoretically important move, separable from the "Markerese" issue, and related to the problem of compositionality, Katz & Postal (1964) made the innovation of putting such morphemes as Neg and a Question morpheme Q into the Deep Structure, as in (4), arguing that there was independent syntactic motivation for doing so, and then the meaning could be determined on the basis of Deep Structure[7] alone.

(4) a. [Neg [Mary [has [visited Moscow]]]] $\Rightarrow_{T-NEG}$
      [Mary [has not [visited Moscow]]]

     b. [Q [Mary [has [visited Moscow]]]] $\Rightarrow_{T-Q}$
      [Has [Mary [visited Moscow]]]

This led to a beautiful architecture: Deep Structure is the input to semantics. Transformations map Deep Structure to Surface Structure. Surface Structure is the input to phonology.

This big change in architecture rested on the claim that transformations should be meaning-preserving. It was an interesting and provocative claim, 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) was elegant and attractive.

2.6. The Garden of Eden period

Chomsky's thinking about semantics evolved from *Syntactic Structures* (1957) to *Aspects of the Theory of Syntax* (Chomsky 1965). There he tentatively[8] accepted Katz and Postal's hypothesis of a systematic connection between syntax and semantics at the level of Deep Structure.

> "Thus the syntactic component consists of a base that generates deep structures and a transformational part that maps them into surface structures. The deep structure of a sentence is submitted to the semantic component for semantic interpretation, and its surface structure enters the phonological component and undergoes phonetic interpretation. The final effect of a grammar, then, [page break here] is to relate a semantic interpretation to a phonetic interpretation—that is, to state how a sentence is interpreted. This relation is mediated by the syntactic component of the grammar, which constitutes its sole "creative" part." (Chomsky 1965, pp. 135–136)

During the brief period when *Aspects* held sway, 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". Quite a few dissertations were written about the grammar of one language or another, all with Deep Structures similar to what Chomsky proposed for English in Aspects, and differing only in what transformations applied to make those languages look different from English on the surface. This was also the period when the "Universal Base Hypothesis", the conjecture that the grammars of all natural languages have the same base rules, was developed independently by McCawley, Lakoff, and Bach; see brief discussions in (Partee et al. 1990, p.556) and (Newmeyer 1980, pp. 148–150), and more in Peters & Ritchie 1973.)

In that period, roughly the mid-60's, before the linguistic wars broke out in full force, I think generative grammarians generally believed the Katz and Postal hypothesis. The idea that meaning was determined at this "deep" level was undoubtedly part of the appeal of the notion of Deep Structure beyond linguistics (cf. Leonard Bernstein's

Norton Lectures (Bernstein 1976)) and probably contributed to the aura surrounding the notion of "language as a window on the mind."

So around 1965, there was very widespread optimism about the Katz-Postal hypothesis that semantic interpretation is determined by deep structure, and the syntax-semantics interface was believed to be relatively straightforward (even without having any really good ideas about the nature of semantics.)

2.7.   Expulsion from Garden of Eden and the roots of the linguistic wars

What happened to upset that lovely view? Although of course there were multiple factors, I think it's fair to focus on one salient issue: linguists discovered quantifiers! (Bach 1968, Karttunen 1968, Karttunen 1969, Lakoff 1968, McCawley 1971) Transformations that preserved meaning (more or less) when applied to names clearly did not when applied to some quantifiers. Clear examples come from "Equi-NP Deletion", the transformation that applied to (5a) to give (5b).

(5)   a.   John wants John to win.
      b.   John wants to win.

When the identical NPs are names, the transformation seems to preserve meaning all right. But if applied to sentences with quantifiers, it would have the unwanted result of deriving (6b) from (6a).

(6)   a.   Everyone wants everyone to win.
      b.   Everyone wants to win.

Similar problems arise for the then-assumed Reflexivization transformation: should (7b) be derived from (7a)? And likewise for the "Conjunction-Reduction" transformation, which would transform (8a) into the non-synonymous (8b).

(7)   a.   Every candidate voted for every candidate.
      b.   Every candidate voted for himself.

(8)   a.   Every number is even or every number is odd.
      b.   Every number is even or odd.

(We'll return to these problems when we discuss early efforts to combine Montague Grammar with Transformational Grammar.)

2.8.   The linguistic wars

There were two classes of responses by linguists to the problematic relation between classic transformational derivations and semantics, the Generative Semantics response and the Interpretive Semantics response. Much has been written about the ensuing linguistic wars, so I will be very brief and schematic; see (Harris 1993, Huck & Goldsmith 1995, Newmeyer 1980, Seuren 1998).

The Generative semantics response (Lakoff, Ross, McCawley, Postal, early Dowty, Larry Horn, sometimes Bach): In order for deep structure to capture semantics, it needs to be deeper, more abstract, more like "logical form" (first-order-logic). The resulting syntax seemed implausible to some, though it should be noted that some maligned rules like the Generativists' "Quantifier Lowering" were later reproduced "upside down" by the "interpretivists" (cf. "Quantifier Raising" of May (1977)). But semantics was taken seriously by the generative semanticists, much more so than by Chomsky; they were trying to preserve the kind of elegant relation between the deepest level of structure and semantic interpretation that Chomsky had espoused, following Katz and Postal, in *Aspects*.

The Interpretive semantics response (Chomsky 1971, Jackendoff 1972): Keep syntax beautiful and 'independently motivated'. Give up the principle that an ambiguous sentence should always have two different deep structures, since there did not seem to be any independent syntactic evidence for syntactic ambiguity for a semantically ambiguous sentence like (9).

(9)   Every student answered one question correctly.

Different semantic modules may work at different levels; quantifier scope and anaphoric relations may be determined at surface structure. The resulting semantics often seemed architecturally ad hoc, although with many insights as well.

In 1971 I published a paper (Partee 1971) in which I analyzed some of the 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 at least for me the paper helped clarify the issues, and helped me be ready to appreciate the potential usefulness of Montague's work once I began to understand it. (The paper was written after I had had my initial exposure to Montague's work in a seminar in Fall 1968 but before I had understood it well enough to say anything about it.)

So with the battles of the late 60's and early 70's raging in linguistics, let's turn to philosophy and logic.

3. PHILOSOPHICAL AND LOGICAL BACKGROUND

3.1. Early philosophers

The relevant history in philosophy goes back at least to Aristotle, but for this short article I will skip Aristotle and the Stoics and the interesting medieval logicians and start with just brief mentions of Leibniz, Boole, De Morgan, and Peirce before turning to the central figures of the late nineteenth and early twentieth centuries.[9]

Leibniz (1646–1716) dreamed of a *characteristica univeralis*, based on an <u>*ars combinatoria*</u>, a system of symbolization that would have simple forms for simple concepts, and unambiguous logical forms displaying the logical structure of all complex expressions, together with a *calculus ratiocinator*, a complete system of deduction that would allow new knowledge to be derived from old. Leibniz's program aimed to encompass the three relationships between language and reality, language and thought, and language and knowledge. Leibniz also had a notion of possible worlds. But his work on these topics had little impact.

With the rise of mathematics in the 19th century, George Boole (1815–64) had an algebraic conception for a system governing the "Laws of Thought", a kind of calculus ratiocinator independent from the vagaries of natural language. (Boolean algebra turns out to have widespread application to natural language semantics, whether Boole would like that or not.) De Morgan in the mid-19th century extended

Boole's work on "the algebra of logic" to include operations on relations (composition of two relations, converse of a relation.) Peirce added further operations and is often credited with developing the theory of relations (Burris 2009).

3.2. Frege

The greatest foundational figure for formal semantics is Gottlob Frege (1848–1925). He is credited with a number of ideas that have been crucial for logic and for semantics. One of his central contributions is the idea that *function-argument* structure is the key to semantic compositionality. Without the idea that some expressions denote functions that can apply to the denotations of other expressions, it was a mystery how compositionality should work—what kinds of things could the meanings of phrases be such that they could be combined—by what kind of a calculus?—to give meanings of larger phrases?

To illustrate Frege's idea with a simple example, consider the sentence *John is happy*. Following Frege, we can say that the predicate is *happy* denotes the characteristic function of the set of happy entities; that function applies to the individual denoted by the name *John* to give 'true' or 'false', the denotation (extension) of the sentence. Schematically:

(10) $||\text{Happy}|| \, (||\text{John}||) = T$ (or F)

Another of Frege's great contributions was the logical structure of quantified sentences. That was part of the design of a "concept-script" (*Begriffschrift*), a "logically perfect language" to satisfy Leibniz's goals; he did not see himself as offering an analysis of natural language, but a tool to augment it, as the microscope augments the eye.

Frege is also credited with the Principle of Compositionality[10], a cornerstone of formal semantics, and a principle quite universally followed in the design of the formal languages of logic, with a few interesting exceptions[11].

> **The Principle of Compositionality:** The meaning of a complex expression is a function of the meanings of its parts and of the way they are syntactically combined.

And Frege introduced the distinction between *sense* and *reference*

(*Sinn* and *Bedeutung*), which later philosophers of language tried to formalize in various ways, e.g. as the distinction between *intension* and *extension*.

3.3. Russell, Carnap, Tarski

Russell (1872–1970) largely followed Frege, but to avoid paradox he introduced *logical types*, and used them to impose restrictions on well-formed function-argument expressions. He differed from Frege in regarding the concrete referents of term phrases, rather than their senses, as direct constituents of propositions. His contributions to logic and philosophy of language were many and varied, and his influence was great.

Carnap (1891–1970) in his early work used the theory of types for the 'logical construction of the world' (Carnap 1928) and 'the logical construction of language' (Carnap & Smeaton 1937). Carnap was a founding member of the Vienna Circle, a group who sought to unify the sciences within a common logico-linguistic framework based on type theory; their primary method was logical analysis, and one of their targets was the elimination of 'meaningless' metaphysics. 'Meaningful' sentences should be either analytic—true by virtue of their meaning and logic—or knowable on the basis of experience—'verifiable.' (Quine admired Carnap greatly, but found a deep circularity in the notions of 'analytic' and 'meaning' and related notions.)

Later Carnap read and appreciated Tarski's work, saw the need for previously excluded non-extensional language, and developed a *semantic* approach, where meaning = truth conditions (Carnap 1956). (Quine, for one, distrusted all non-extensional language.) Carnap introduced possible worlds as state-descriptions; see Section 3.5. His metalanguage remained extensional.

Later still, Carnap recognized the importance of adding pragmatics to his theorizing, with issues of gaining and communicating knowledge: not everything important about language could be expressed with pure logical syntax and semantics.

Tarski (1902–1983) developed *model theory* based in set theory and with it made major advances in providing a semantics for logical languages, including a semantical definition of truth, still with an extensional metalanguage.

3.4. The Ordinary Language—Formal Language war

Around the time of these advances, and perhaps in light of increasing attention to pragmatics and the real use of real languages, a major war began within philosophy of language, the "Ordinary Language" vs. "Formal Language" war[12].

The Ordinary Language Philosophers were a new generation who rejected the formal approach, and urged closer attention to the functions of ordinary language and its uses. Two central figures were the late Wittgenstein (1889–1951), and Strawson (1919–2006). In 'On referring' (Strawson 1950), Strawson said, "The actual unique reference made, if any, is a matter of the particular use in the particular context;..." ..."Neither Aristotelian nor Russellian rules give the exact logic of any expression of ordinary language; for ordinary language has no exact logic."

Russell replied in 'Mr. Strawson on referring' (Russell 1957): "I may say, to begin with, that I am totally unable to see any validity whatever in any of Mr. Strawson's arguments." ..."I agree, however, with Mr. Strawson's statement that ordinary language has no logic."

It is noteworthy that both sides in this 'war' (as well as Chomsky, as expressed in his reply to Bar-Hillel (Chomsky 1955)) were in agreement that logical methods of formal language analysis did not apply to natural languages.

3.5. Responses to the OL-FL war

In some respects, that war continues. But the interesting response of some formally oriented philosophers was to try to analyze ordinary language better, including its context-dependent features. The generation(s) that included Prior, Bar-Hillel, Reichenbach, and Montague, and later Montague's students, gradually became more optimistic about being able to formalize the crucial aspects of natural language. See, for instance, (Bar-Hillel 1954a, Bar-Hillel 1954b, Bar-Hillel 1963, Prior 1967, Reichenbach 1947).

Arthur Prior (1914–1969) made great progress on the analysis of tense, one central source of context-dependence in natural languages, which had been omitted from earlier logical languages. Reichenbach (1947) developed a theory of tense and aspect that later came to play a

major role in formal semantics and pragmatics. And Montague (1930–1971), a student of Tarski's, was an important contributor to these developments. His Higher Order Typed Intensional Logic unified tense logic and modal logic, extending Prior's work and building on Church's work on intensional logic and type theory (Church 1940, 1951), and more generally unified "formal pragmatics" with intensional logic.

Paul Grice was also an important contributor to resolving the war, since his work on conversational implicatures (Grice 1892) showed that having some good explanatory pragmatic principles at hand can allow the semantics to be much simpler, so that the apparent gap between the "logicians' meaning" of various little logical words and their ordinary-language meaning may be much less than had been supposed.

3.6. *Possible Worlds Semantics*

While the notion of possible worlds goes back at least to Leibniz, the foundations for contemporary possible worlds semantics were laid in the late 1950's and early 1960's by Rudolf Carnap, Stig Kanger, Jaakko Hintikka, Saul Kripke, and Richard Montague[13],[14]. Tarski (1935) introduced the related notion of alternative models in the semantic explication of logical entailment and validity. Carnap (1946, 1947) came close to possible worlds with his state-descriptions, which are sets containing atomic sentences or their negations, intended to "represent Leibniz's possible worlds or Wittgenstein's possible states of affairs" (Carnap 1956, p.9). Carnap systematized the intension-extension distinction, analyzing sameness of intension as sameness of extension in all state-descriptions[15], linking the semantics of the modal operators "necessarily" and "possibly" to the intensions of the expressions they apply to, and giving a successful semantics for quantified modal logic.

Because of the ways in which state-descriptions differ from later conceptions of possible worlds, Carnap's approach applied smoothly only to the purely logical modalities, not to such modal notions as physical necessity or moral obligation. But in a paper delivered orally in 1955 and published as Montague (1960)[16], Montague took Carnap's notion of logical necessity as truth in all models and extended it with relations between models with which he was able to show the parallels among logical necessity, physical necessity, and obligation (deontic necessity), and the parallels between these notions and universal quantification.

Kanger (1957a, 1957b) and Kripke (1959, 1963) distinguished the notion of possible worlds from possible models and added worlds into the models as part of the semantic interpretation of necessity and possibility. With this enrichment of the models, Kripke and Kanger were able to show how different axiomatizations of modal logic corresponded to different accessibility relations among the possible worlds and hence how different competing accounts of the properties of necessity and possibility could all represent different reasonable notions that might correspond to different kinds of necessity—logical, deontic, metaphysical, etc.

Hintikka (1962) showed the value of possible worlds for doxastic and epistemic logic with his well-known treatments of knowledge and belief. (Actually, he used "model sets" in his earliest work, possible worlds later.)

The extension of model theoretic techniques into modal logic led to a great expansion of work in logic and philosophy of language in quantified modal logic, tense logic, the logic of indexicals and demonstratives, adjectives and adverbs, propositional attitude verbs, conditional sentences, and intensionality.

Montague contributed greatly to the development of formal semantics with his development of intensional logic and his combination of pragmatics with intensional logic (Montague 1968, 1970a). The intensional logic he developed unified modal logic, tense logic, and the logic of the propositional attitudes.

4.   MONTAGUE'S WORK

Montague unified Carnap's work with Kripke and Kanger's; he treated both worlds and times as components of "indices", and intensions as functions from indices (not just possible worlds) to extensions. The strategy of "add more indices" was accepted from Dana Scott's "Advice on modal logic" (Scott 1970), an underground classic long before it was published.

Montague also generalized the intensional notions of property, proposition, individual concept, etc., into a fully typed intensional logic, extending the work of Carnap (1956), Church (1951), and Ka-

plan (1964), putting together the function–argument structure common to type theories since Russell with the treatment of intensions as functions to extensions.

In 'Pragmatics and Intensional Logic' (Montague 1970a), Montague distinguished between 'possible worlds' and 'possible contexts'; contexts were introduced to treat the indexical character of such words as *now, I,* and *here* (this latter development represents joint ideas of Montague, Dana Scott, and Hans Kamp). In 'On the Nature of Certain Philosophical Entities' (Montague 1969), he applied his logic to the analysis of a range of philosophically important notions (like *event, obligation*); this was all before he started working directly on the analysis of natural language.

That work, like most of what had preceded it, still followed the tradition of not formalizing the relation between natural language constructions and their logico–semantic analyses or 'reconstructions': the philosopher–analyst served as a bilingual speaker of both English and the formal language used for analysis, and the goal was not to analyze natural language, but to develop a better formal language. Montague's work on the formal treatment of natural languages came only with his last three papers, "English as Formal Language" (EFL) (Montague 1970c), "Universal Grammar" (UG) (Montague 1970b), and the last and most famous, "The Proper Treatment of Quantification in Ordinary English" (PTQ) (Montague 1973).

4.1. Why did Montague turn to natural language work?

No one seems to know for sure why exactly Montague decided to turn his attention to the task of constructing a formal framework for the syntactic and semantic description of language. His change of direction came as a surprise to some of his colleagues; Solomon Feferman, for instance, had been working with Montague on a book on the method of arithmetization of metamathematics and some of its applications, incorporating the results of both of their dissertations (both under Tarski). A great deal was written, but before it could be completed, their paths diverged, Montague's principally in the direction of his "linguistic" work. To Feferman, Montague's work on formal semantics of natural languages came "out of the blue" (Solomon Feferman, p.c. January 10, 2011).

I have gotten clues from several sources, though, which I report in this section.

4.1.1. A note on the Kalish and Montague textbook.

The first edition of Kalish and Montague's (1964) logic textbook (published in 1964, but used in classes much earlier) contains the following passage: "In the realm of free translations, we countenance looseness ... To remove this source of looseness would require systematic exploration of the English language, indeed of what might be called the 'logic of ordinary English', and would be either extremely laborious or impossible. In any case, the authors of the present book would not find it rewarding." (p.10)

On page 10 of the second edition (Kalish et al. 1980), the passage is altered[17]: "In the realm of free translations, ... would be extremely laborious or perhaps impossible. In any case, we do not consider such an exploration appropriate material for the present book (however, see Montague (1974) and Partee (1976))."

So Montague's attitude evidently underwent a change in the late 60's. I have been wondering what's behind the original remark, and what's behind the changes.

It's clear that Kalish and Montague took more than ordinary pains to give students explicit guidance in the process of translation from English to first order logic: rather than the usual informal explanations and examples, they produced an algorithm for step by step conversion of sentences of (a subset of) English to and from formulas of first order logic. The algorithm was quite exact for a very regimented sublanguage of English; there were some guides but not an algorithm for converting between this 'strict' translation into English and the more idiomatic 'free' translation referred to in the p.10 passages.

I thought I had been told years ago that Montague then reasoned that if such translation could be formalized, it must also be possible to formalize the syntax and semantics of English directly. But Hans Kamp and Nino Cocchiarella (both students of Montague's) have given me other opinions about what may be behind the story.

4.1.2.   Kamp and Cocchiarella on Montague's Motivations.

From Hans Kamp (e-mail, October 1, 2009, abridging slightly):

> Richard's interests in giving a systematic account of mean-
> ing for natural languages must go back a good deal far-
> ther. One important point is that the text was available at
> UCLA for years as script before it appeared as a book and
> that both Kalish and Montague used it on a regular basis in
> their courses.

> The quoted passage from p. 10 is, I believe, highly sig-
> nificant. Richard emphasized to me repeatedly that there
> was something odd about the way the book presents the
> subject: Everything about the formal languages of logic is
> presented with precision, but when the student is asked
> to apply the formal languages in the exercises, an appeal
> is made not only to the student's grasp of the formal def-
> initions but also to his intuitive understanding of English.
> Montague was acutely aware this odd 'gap': What IS it that
> enables us to do those translations and to check whether
> somebody has got them right?

> While I guess that an awareness of this gap must have been
> present in Richard for quite some time before the appear-
> ance of the book, it may well be that an idea about what
> could be done to fill it ripened only during the second half
> of the sixties. Perhaps a contributing factor to this 'delay'
> was a difference in opinion between him and Kalish. Mon-
> tague apparently wanted to include the model theory for
> first order logic in the book (and also in the courses based
> on it), but Kalish appears to have disagreed, thinking that
> that would make things too hard for the beginning logic
> student. So Montague gave in and model theory was left
> out. If it hadn't been, then perhaps his ideas about the
> model-theoretic semantics of English might have come to
> fruition a little earlier.

> But whatever the exact time when Richard's work on the
> semantics of NL got properly under way, the relation to

the passage of p. 10 is I think pretty clear: Defining good
translation functions between NL and symbolic logic is very
hard, and it is also a task that is difficult to define, because
it isn't clear exactly what the criteria should be; in partic-
ular, it may not be all that clear (or generally acceptable)
whether preservation of truth conditions should be the one
and only goal of an adequate translation.

> Developing a model-theoretic semantics for NL is a some-
> what different enterprise. Here the focus is clearly, natu-
> rally and inevitably on conditions of truth and reference;
> and in and of itself developing a model-theoretic seman-
> tics is not the same thing as defining a translation func-
> tion from NL to Predicate Logic (not even when the models
> used in the model-theoretic semantics are models for Pred-
> icate Logic). However, a model-theoretic account of NL
> meaning can be used as a criterion (either as the only one
> or as one among others) for adequate translation. And of
> course, as became plain in Richard's later papers on NL se-
> mantics (PTQ and Universal Grammar), a translation func-
> tion could also be useful as a way of articulating a model-
> theoretic treatment.

> I think it was during the last time I saw Richard (Christ-
> mas/New year '69/'70) that he mentioned a new edition
> of the Logic Introduction. I do remember his saying that he
> thought the book would gain from having model theory in-
> cluded in it. But I cannot remember whether he said some
> agreement with Kalish had been reached. In any case,
> the second edition that finally appeared must have been
> Kalish's responsibility. [end of excerpt from Hans Kamp
> e-mail October 1, 2009]

From Nino Cocchiarella, e-mail Dec 2010:

> [Montague's] early work on pragmatics and intensional
> logic had not yet [in the mid 60's] affected [his] basic
> philosophical view: namely, that all philosophical analy-
> ses can be carried out within a definitional extension of set

theory, which explains why in "English as a Formal Language" Montague uses set theory to construct the syntax and semantics of a fragment of English in a way that resembles the construction of the syntax and semantics of a first-order modal predicate calculus.

But Montague did not remain satisfied with set theory as a *lingua philosophica*, nor with unprincipled 'paraphrasing' between natural language and logical language, and in the end he proposed instead the construction of an intensional logic as a new theoretical framework within which to carry out philosophical analyses. . . .

Once Montague moved on to an intensional logic we have a distinctive new tone about English and natural language in his papers . . . .

4.1.3. A New Clue about Montague's Motivations.

While working in the Montague archives at the UCLA library in January 2011, I found a new clue about Montague's motivations, not inconsistent with either Kamp's or Cocchiarella's opinions, but adding two quite different factors.

Attached to a handout of an early talk version of "English as a Formal Language", July 31, 1968, UBC, Vancouver, is a page in Montague's handwriting of remarks he evidently intended to make in introducing his talk, but didn't include in the handout itself.[18] There Montague wrote, "This talk is the result of 2 annoyances: The distinction some philosophers, esp. in England, draw between "formal" and "informal" languages; [and] The great sound and fury that nowadays issues from MIT about a "mathematical linguistics" or "the new grammar"—a clamor not, to the best of my knowledge, accompanied by commensurate accomplishments. I therefore sat down one day and proceeded to do something that I previously regarded, and continue to regard, as both rather easy and not very important—that is, to analyze ordinary language*. I shall, of course, present only a small fragment of English, but I think a rather revealing one." Montague had added a note to insert in the place he marked with an asterisk:
"Other creditable work: Traditional grammar, Ajdukiewicz, Bohnert

and Backer[19], JAW Kamp."

Thus two of his important motivations appear to be negative ones. He wanted to refute the presupposition shared by philosophers on both sides of the Ordinary Language—Formal Language divide (and by Chomsky, as we have noted) that formal languages and natural languages are so different that logicians' formal methods cannot be applied to natural languages. And while he is known to have had a positive attitude toward the professed goals of Chomsky's linguistic program, he was not impressed with the results, and was certainly not interested in a project that involved only syntax.

It is a bit deflating for those of us who have spent much of our careers working in formal semantics to hear that Montague considered the task of analyzing natural language "rather easy and not very important". Although I have not found any statements by Montague suggesting that he changed his mind about that assessment, I would of course like to think that he did, and there is at least some circumstantial evidence of two sorts. For one thing, he devoted most of his research to that topic for the next two or three years (which turned out to be the remaining years of his life, although he couldn't have anticipated that), and he indicated his intention to write further papers and a book. And secondly, there are handwritten pages in his files from 1970 when he was working on PTQ that show failed attempts to treat quite a number of phenomena that never made it into PTQ. For example, he had intended to include a much larger class of quantifiers than the three (*a, the, every*) that ended up being treated in PTQ. But he abandoned the attempt to include a treatment of plural expressions in PTQ, which eliminated most quantifiers and eliminated term phrases conjoined with *and*, leaving only those three singular determiners and term phrases conjoined with *or*. He also made some attempts at *any* and *no*, but also left them out. There is evidence of his having worked on other constructions as well, such as passives, without finding a satisfactory treatment[20].

4.2. Central ideas in Montague's work

The first result of Montague's work on natural language was that provocatively titled paper "English as a Formal Language" (Montague 1970b), which begins with the famous sentence, "I reject the con-

tention that an important theoretical difference exists between formal and natural languages." (p. 188 in Montague (1974)) As noted by Bach (1989), the term "theoretical" here must be understood from a logician's perspective and not from a linguist's.

What Montague was denying was the logicians' and philosophers' common belief that natural languages were too unruly to be formalizable; what he was proposing, here and in his "Universal Grammar", was a framework for describing syntax and semantics and the relation between them that he considered compatible with existing practice for formal languages and an improvement on existing practice for the description of natural language.

The Fregean principle of compositionality was central to Montague's theory and remains central in formal semantics.

The Principle of Compositionality: The meaning of a complex expression is a function of the meanings of its parts and of the way they are syntactically combined.

For Montague, the compositionality requirement was given an algebraic formulation. For Montague, syntax is an algebra of 'forms', semantics is an algebra of 'meanings', and there must be a homomorphism mapping the syntactic algebra into the (polynomial closure of the) semantic algebra. Compositionality is the homomorphism requirement. The nature of the elements of both the syntactic and the semantic algebras is left open; what is constrained by compositionality is the relation of the semantics to the syntax.

Details of Montague's own analyses of the semantics of English have in many cases been superseded, but in overall impact, PTQ was as profound for semantics as Chomsky's *Syntactic Structures* was for syntax. Emmon Bach (1989) summed up their cumulative innovations thus: Chomsky's Thesis was that English can be described as a formal system; Montague's Thesis was that English can be described as an *interpreted* formal system.

Martin Stokhof (2006) describes the PTQ model of Montague grammar in some detail, isolating "two core principles that are responsible for its remarkable and lasting influence":

A. Semantics is syntax-driven, syntax is semantically moti-

vated.
B. Semantics is model-theoretic.

Montague did not invent model-theoretic semantics; but it was through his work that the model-theoretic approach became more widely known and adopted among linguists, with far-reaching changes to the field of linguistic semantics. One of the central ideas, not novel with Montague, is that truth-conditions and entailment relations are basic. These are minimal data that have to be accounted for to reach "observational adequacy" in semantics. That principle, inherited from the traditions of logic and model theory, is at the heart of Montague's semantics and is one of the defining principles of formal semantics.

Cresswell (1978) put this in the form of his "Most Certain Principle": we may not know what meanings are, but we know that if two sentences are such that we can imagine a situation in which one of them is true and the other false, then they do not have the same meaning. As Cresswell showed, many decisions about semantic analysis, both in general architecture and in particular instances, can be seen to follow from that principle. It may be hard to remember or realize how surprising and controversial an idea it was to linguists in the early 1970's to think about truth conditions rather than just ambiguity, semantic anomaly, and synonymy.

And because Montague's general framework specifies constraints on the relation between semantics and syntax without specifying what exactly the content of the semantics and the syntax should be like, the framework leaves room for evolution of notions of how meaning should be modeled. Montague's work in his various papers exemplifies quite a range of different particular choices that the framework permits for both semantics and syntax. In EFL he gives a direct model-theoretic semantics for English; in UG and PTQ he has an intermediate step of compositional translation from English into the language of Intensional Logic, with only the Intensional Logic given a direct model-theoretic semantics. The syntax in EFL is given as a rather brute-force (though in many respects insightful) recursive definition of well-formed expressions of all the syntactic categories. The syntax in PTQ is also a recursive definition, but uses a modified form of categorial grammar. David Lewis's more transformational-grammar-like syntax in Lewis (1970) also uses categorial grammar, in the base component;

Lewis and Montague were colleagues at UCLA during this period and shared some of their ideas on these topics. Both Montague and Lewis cite Ajdukiewicz (1960) in connection with categorial grammar, and Montague acknowledged Haskell Curry's work as well in oral presentations.[21]

There is much more to tell about Montague's work, but much has already been written, so I will leave this section brief. I do want to emphasize that while Montague certainly deserves his pre-eminent position in the history of formal semantics, he did not work single–handedly or in a vacuum. In addition to building on the work of others, as partially sketched above, his papers include acknowledgements to suggestions from David Lewis, David Kaplan, Dana Scott, Rudolf Carnap, Alonzo Church, Yehoshua Bar-Hillel, J.F. Staal, Terence Parsons, and the present author; his students Hans Kamp, Dan Gallin, Perry Smith, Harry Deutsch, and Michael Bennett; and others.

Other works that appear to have influenced his thinking, on problems or on solutions or both, show up in the bibliographies of his papers and/or in assigned reading in his seminars on the philosophy of language in 1967 and later. These include, among others, Quine (1960), Goodman (1951) and Geach (1962, 1967) for puzzles of intensionality; Frege (1892), Davidson (1964, 1967, 1970), Kripke (1963), and various works of Tarski's.

And there were of course other important early contributors to the development of formal semantics as well; see Abbott (1999), Partee (1996), Partee & Hendriks (1997), Stokhof (2006)).

5. INTERACTIONS BETWEEN LINGUISTS AND PHILOSOPHERS IN THE LATE 1960'S AND EARLY 1970'S

Montague was doing his work on natural language at the height of the "linguistic wars" between generative and interpretive semantics, though Montague and the semanticists in linguistics had no awareness of one another. PTQ (Montague 1973) gave recursive definitions of well-formed expressions and of their interpretations, illustrating what Bach christened the "rule-by-rule" approach to syntax-semantics correspondence. That was quite different from both generative and interpretive semantics, which looked for some "level" or "levels" of syntactic

description to interpret.

There were already some interactions between linguists and philosophers by that time. Linguists who knew something about some philosophers' work and drew on it in their work included Emmon Bach, Ed Keenan and James McCawley; philosophers and logicians who knew something about what linguists were doing and took an interest in it included Gil Harman, Donald Davidson, David Lewis, Terence Parsons, Julius Moravcsik, Richmond Thomason, J.F. Staal, and Yehoshua Bar-Hillel. And there were particular works that somehow came to the attention of those on the other side of the linguistics-philosophy divide; to give just one example, there was much interest among linguists in the puzzles of "donkey pronouns" posed in Geach (1962).

5.1. Linguistic-philosophy interactions—east and west

Within the U.S., there was an interesting difference in the logic-philosophy environment on the East Coast, dominated by Quine, and on the West Coast, a hotbed of Tarski-trained logicians working on possible worlds semantics and higher-order logic.

From 1967 to 1969, Donald Davidson and Gil Harman were both at Princeton, interacting intensely, both optimistic about the potential fruitfulness of linguistics-philosophy interactions. They were optimistic about the prospects of generative semantics, with its underlying structures somewhat resembling the structures of first-order logic. They influenced each other's work, and together they produced some exciting conferences and influential edited collections, which I will describe below.

At the same time David Lewis and Montague were both at UCLA, also interacting; David introduced me to Montague and I first sat in on a seminar of Montague's at UCLA (with David and my student Frank Heny) in the Fall of 1968. I had a lot of very naïve questions at the beginning (including "what are these lambdas?"), and David was the one I could ask them to; he always answered patiently and well.

Also in 1968, the first version of Terry Parsons' "A Semantics for English" (1968) (precursor to Parsons 1972) was circulated; we became acquainted when he paid some visits to UCLA, and later helped influence each other to move to the University of Massachusetts at the same time in 1972.

5.2. *Linguistic-philosophy interactions—vignettes*

In this section I want to illustrate the early linguistics-philosophy interactions with a few vignettes, of four early conferences and a summer school. I won't say very much about the ideas in this part, but will try to convey some of the feeling of what I sense in retrospect was a rather fast evolution over just a few years from a period when linguists and philosophers were beginning to be open to the idea of trying to interact but found it difficult to a period when linguistics-philosophy interactions began to be quite common and quite fruitful.

In August 1969 Davidson and Harman organized an Interdisciplinary Conference on the Semantics of Natural Language at the Center for Advanced Study in the Behavioral Sciences in Stanford, California, where Davidson was a Fellow for the year. The participants were the linguists Emmon Bach, Charles Fillmore, George Lakoff, James McCawley, and Barbara Partee, and the philosophers Donald Davidson, Peter Geach, Gilbert Harman, David Kaplan, W.V. Quine, J.A. Staal, and Bruce Vermazen. Papers were presented by Quine, Fillmore, Geach, McCawley, Davidson, and Bach (including the dueling papers, Geach's "A Program for Syntax" and McCawley's "A Program for Logic"). Montague was not invited, which I thought was a pity. I found the conference stimulating and interesting, but I suppose it's true that no one changed anyone's way of thinking on any important issues. Abbott (1999) notes that Quine states in his abridged autobiography that the conference was "a fiasco at bridge-building" (Quine 1986, p.38). Nevertheless, a wonderful huge edited volume came out of that conference, with papers by many who were not at the conference, including Montague (1970a) and Kripke (1972).

In May of 1970, there was a small two-day conference of linguists and philosophers at UCLA, organized by Barbara Partee, Martin Tweedale, and Talmy Givón. The conference was 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. The four philosophy talks were by Julius Moravcsik, Richard Montague ("Universal Grammar"), John Vickers, and Martin Tweedale; the four linguistics talks were by George Bedell, George Lakoff ("Linguistics and Natural Logic"), Robin Lakoff, and Barbara Partee. (Mine was a forgettable and inconclusive talk called "Does de

Morgan's Law operate in English?". I remember puzzling over whether *neither-nor* constructions should be syntactically derived from something containing negation and *and*, but I couldn't find any analysis that would make me both syntactically and semantically happy.) I don't have a full list of the other participants, but I know that they included Lauri Karttunen and Bob Wall, and that the students attending included Michael Bennett and Larry Horn. A particularly memorable moment came when an argument erupted between Lakoff and Montague about whether it was crazy to derive prenominal adjectives from relative clauses or crazy not to. Montague had just mentioned a simple rule that combines an adjective with a noun. Lakoff interrupted: "Don't you know that adjectives are derived from relative clauses?" Montague: "Why would *anyone* think THAT?" I jumped up and quickly explained to each of them where the other's position was coming from, and during the coffee break got the closest to a compliment I ever got from Montague—"Barbara, I think that you are the only linguist who it is not the case that I can't talk to."[22]

The small closed conference at which Montague presented PTQ, organized by Moravcsik, Hintikka and Suppes, was held at Stanford in two parts. At part 1 in September, all the participants presented their papers, and then circulated them to the other participants. Participants included (among others) David Kaplan, Ed Klima, Ken Wexler, Joyce Friedman, Stanley Peters, Dov Gabbay, Joan Bresnan, and me. When "part 2" was held two months later, in November, we were all to make comments on as many of the other participants' papers as we wished. I decided to put all my efforts into commenting on Montague's paper. I commented on Montague's syntax, comparing it with transformational grammar. I recall David Kaplan saying that by listening 'inversely', he was able to understand something about how transformational grammars worked. And Montague didn't object to my description of what he was doing—that was reassuring. That conference resulted in an edited book (Hintikka et al. 1973), including the papers and the commentaries on them.

Just a few months after that conference, on March 7, 1971, Montague was murdered; for a summary of what is known about his death, see (Feferman & Feferman 2004, 331–333). That tragic event marked a major turning point in the development of "Montague grammar" and

formal semantics: Montague was no longer there to play a leading role, and it was left to those who had become or would become interested in his kind of work to develop it as best we could. This may be one of the reasons for the often-remarked spirit of cooperation that has marked semantics for many decades.

In the summer of 1971 there was another major event organized by Davidson and Harman, a Summer Institute in Philosophy and Linguistics at the University of California, Irvine, under the auspices of the Council for Philosophical Studies. That was a memorable and influential event—for me personally in part because it was the occasion where I figured out how to make use of lambdas to model 'syntactic deletion' and made my first steps toward combining Montague Grammar and Transformational Grammar—, but more generally for the intense immersion in a philosophy of language environment that it afforded to its hundred or so participants. 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[23], 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; 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 later 1974 Linguistic Institute at UMass also brought in quite a number of philosophers of language, and continued the lively and productive interaction.)

And I want to mention an important conference in the Spring of 1973 that brought a partly different set of people together with good interaction and some long-term results. This was a Formal Seman-

tics conference organized by Ed Keenan at Cambridge University—the first international conference on formal semantics. It included papers by David Lewis, Barbara Partee, R.D. Hull, Altham & Tennant, John Lyons, Pieter Seuren, Östen Dahl, Colin Biggs, Hans Kamp, Renate Bartsch, Carl Heidrich, Arnim von Stechow, Timothy Potts, George Lakoff, Stephen Isard, Petr Sgall, Theo Vennemann, Yorick Wilks, Joe Emonds, Maurice Gross, Ed Keenan, Haj Ross, and a few others. The edited collection *Formal Semantics of Natural Language* (Keenan 1975) was published in 1975. (The term 'formal semantics' which may have been new to linguists then, soon gained ground among like-minded semanticists who weren't all strictly Montagovian, Keenan being a prime example.)

5.3. *The introduction of Montague's work to linguists*

The earliest introduction of Montague's work to linguists came via Partee (1973b, 1973c, 1975) and Thomason, who published Montague's collected works with a long introductory chapter in Montague (1974). Partee and Thomason argued that Montague's work might allow the syntactic structures generated to be relatively conservative ("syntactically motivated") and with relatively minimal departure from direct generation of surface structure, while offering a principled way to address many of the semantic concerns that motivated some of the best work in generative semantics.

5.4. *The introduction of Montague's work to linguists*

Let me describe an obstacle I faced when I started trying to put Montague Grammar and Transformational Grammar together, whose solution is related to a leading idea that came into linguistics from philosophy and logic in this period, namely the (Fregean) idea that for a language rich enough to include variable-binding, the clause-level building blocks cannot all be closed sentences (as the "kernel sentences" in early TG were), and Montague's innovative use of abstraction as the central device involved in the semantics of variable-binding constructions. The obstacle: what to do about deletion rules? In classical TG, (11a) was derived from something like (11b) by "Equi-NP Deletion".

(11)   a.   Mary was eager to win.
        b. $[_S$ Mary was eager for $[_S$ Mary to win$]]$

But given the principle of compositionality, and given the way MG works by building up the meanings of constituents from the meanings of their subconstituents, there is nothing that could correspond to "deleting" a piece of a meaning of an already composed subpart. Recall the consequences of the analysis in (11b) for a sentence like (12a). The presumed deep structure (12b) would clearly give the wrong meaning.

(12)   a.   Everyone was eager to win.
        b. $[_S$everyone was eager for $[_S$ everyone Tns win$]]$

The MG-TG resolution that was suggested in Partee (1973b, 1975) rested on the realization that what we want as "underlying" subject in the embedded sentence is a bindable variable. I followed Montague's line and bound it by lambda abstraction to make a VP type, as in (13a), assuming that the complement of the adjective *eager* is a VP. (Others have proposed an S type for the infinitive, with the variable bound by the lambda abstract associated with the higher quantifier, as in (13b).) (In this very simple example, the VP in (13a) could just be base-generated and interpreted directly; my "Derived VP rule" was motivated by VPs like to *see herself* or *to be elected*, which I proposed should be derived transformationally from open sentences like she_0 sees her_0self and he_1 elects her_0.)

(13)   a. $[[$ to win $]] = \lambda x [$ win $(x)]$
        b.   alternatively: everyone'($\lambda x[$ x was eager for $[$x to win$]])$

That solution struck me as an illustration of the importance of the Fregean principle that recursion must be allowed to work on *open* sentences[24]. The syntax and semantics of logical languages had been done that way since Frege—that is crucial to the semantics of quantified sentences in first-order logic. But as Landman (p.c.) emphasizes, "the fact that Montague separates quantification and variable binding, using the λ-operator for the latter, is a conceptual change, away from first order logic and the Frege-Tarski quantifiers, a change that has made Montague Grammar possible and successful to the present day."

But in any case, trying to do recursion on closed sentences ("kernel sentences") was what made transformational rules cast in terms of "identical NPs" break down when quantifiers were discovered, and led to what I described as the Expulsion from the Garden.

In Chomskyan syntax, a corresponding change was eventually made. The first step, probably influenced by Tanya Reinhart's work (Reinhart 1976, 1983), involved replacing the "identical NP" by the special null element PRO, interpreted as a bound variable. A considerably later step, probably influenced by Heim (1982) introduced functional heads that could be interpreted as abstraction operators rather than assuming that indexed quantifiers themselves were responsible for binding. Other syntactic theories, like GPSG, HPSG, and LFG, and modern versions of Categorial Grammar, were developed after the quantifier and binding issues had become well known, so they were designed from the start not to run into those problems.

There were other obstacles to combining Transformational Grammar and Montague Grammar, but that can serve as a good example of an important problem in principle, whose resolution requires rethinking parts of the architecture of one theory or the other. But the goal of combining the then-current version of TG with MG lost some of its urgency as various linguists began to realize that with a powerful semantics to do some of the work, some kinds of transformations—possibly all—might be eliminated: (Dowty 1978b, Gazdar 1982). But in any case, the lesson that variable binding must be provided for if semantics is to be compositional has been well learned by now.

6.   THE EARLY IMPACT OF MONTAGUE GRAMMAR

It turned out that Chomsky was deeply skeptical of formal semantics and of the idea of compositionality in any form; see Chomsky (1975). I have never been able to satisfactorily explain his skepticism; it has seemed to me that it was partly a reaction to a perceived attack on the autonomy of syntax, even though syntax is descriptively autonomous in Montague grammar. But syntax is not "explanatorily autonomous" in Montague grammar, or in any formal semantics, and I do not see any rational basis for believing that it should be. Another possible reason could be the anti-psychologistic stance of Frege and Montague;

semantics is distinguished from knowledge of semantics, and semantics itself is in the first instance concerned with truth-conditions (not actual truth, as is sometimes mistakenly asserted) and entailment relations, not with internal representations.

There are certainly deep puzzles for an account of the semantic competence of the native speaker if semantics is model-theoretic and the model structures are anything like what Montague and others have posited (Partee 1979); I do not believe these puzzles have been solved, but I also do not believe they stand in the way of progress on substantive issues in the analysis of the syntax and semantics of natural language.

One related and frequently cited criticism of Montague's semantics and essentially all versions of formal semantics stems from the logical equivalence problem, also discussed in Partee (1979): if the meaning of a sentence is its truth-conditions, then all logically equivalent sentences are predicted to be synonymous, which seems to be patently false (but not to Stalnaker: see the series of his papers collected in Stalnaker (1999)). Logically equivalent sentences may be intersubstitutable *salva veritate* in all ordinary extensional or intensional contexts, but they do not seem to be so in propositional attitude contexts. (For some of the best philosophical discussion of this issue, which may not be as black-and-white as it looks, see Stalnaker (1984)).)

Here too, I recognize this as a deep problem (I have taken a few stabs at it, in Partee (1973a, 1979, 1982), without notable success), but I regard it as a genuinely difficult problem which no theory I know of has a good solution for, and I do not believe that the lack of a solution to that problem casts any serious doubt on semantic analyses of ordinary extensional and intensional constructions.[25] Others apparently take it as a reason not to pursue formal semantics at all: "Note that the Montague type of semantics was already shown clearly to be a false theory in 1977, since it determines that all necessary truths have the same meaning and that all necessary falsehoods have the same meaning [see Katz & Katz (1977)]." (Paul Postal, from an interview. (Huck & Goldsmith (1995), p.137)) For more sophisticated argument to the same conclusion, see Soames (1985).

But there are alternatives to Stalnaker's "heroic" response; an insightful discussion of responses and kinds of solutions was presented early on in (Lewis 1970). See (King 2011) for a survey of the problem and of the "structured propositions" line of response (not the only family of responses, but one with roots in Carnap's notion of "intensional isomorphism" and with several well-developed variants.) The problem and these alternative solutions are not discussed in most textbooks on formal semantics, in part because the problems only arise in practice in "hyper-intensional" contexts such as *belief*-sentences, and textbooks generally begin (and often end) with the study of extensional constructions, ignoring the complications of dealing with time, possible worlds, indexicality, etc.; advanced courses within linguistics departments are more likely to deal with topics such as tense and aspect, plurality, modality and conditionals, cross-linguistic comparative study of core topics, etc., than to focus on propositional attitudes. The topic is more likely to be discussed in philosophy courses if discussed at all. Probably it should be discussed more widely, since it seems that many people have picked up strong opinions on the topic without studying the arguments on both sides.

In any case, formal semantics did spread and became "mainstream semantics" in the US and Europe in spite of Chomsky's and some others' skepticism. It has never been a monolithic enterprise, and there are many different varieties, but for the most part not crystallized into competing "schools". One point of divergence that emerged in the mid-1980's starting, I believe, with Heim (1982) as an example on the one side and Gazdar et al. (1985) on the other: some, like Gazdar, proposed non-transformational grammars, generally aiming for something close to direct (surface) compositionality, while others, like Heim, work with something like a Chomskyan "LF" as an intermediate (syntactic) level: the mapping to LF resembles upside-down Generative Semantics. But semanticists can often manage to discuss analyses across such differences, translating back and forth between frameworks.

7.   FURTHER DEVELOPMENTS

7.1.   *Formal semantics and the beginnings of formal pragmatics*

The height of initial intense interaction on semantics between linguists and philosophers had passed by 1980, followed by the rise of cognitive science, including semantics as an interdisciplinary concern, 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 1980's also saw the beginnings of semantic typology, formal pragmatics, and computational semantics, each of which would need a chapter of its own.

One major theme starting in the 1980's and continuing up to the present is the increasing prominence of dynamic aspects of meaning and the development of dynamic theories to deal with them. This has been accompanied by the development of formal theories of pragmatics and increasingly integrated theories of formal semantics and pragmatics.

By the middle of the 1980's the increasing recognition of formal semantics as part of the core curriculum in linguistics was seen in the publication of textbooks (Chierchia & McConnell-Ginet 1990, Dowty 1978a, Dowty et al. 1981, Gamut 1982, Gamut 1991, Link 1979) and the growing number of departments with more than one semanticist; by now quite a few have several. And many linguists now are syntax-and-semantics specialists, a welcome development.

By the beginning of the 1990's, formal semantics was a fully established field within linguistics, and students were not conscious that the core fields hadn't always been 'phonology, syntax, semantics'. In the 1980's, there was less interaction between linguists and philosophers in the U.S., in part because interest in the philosophy of language had declined as interest in philosophy of mind increased. But since the 1990's, it's on the rise again, and now there are a number of universities where semanticists and philosophers of language are encouraged to get some training in both fields.

7.2. *Later developments and current outlook*

One could see some divergence between Europe and the US in the 1990's in the study of formal semantics. The Institute for Logic, Language, and Computation (ILLC) was founded in Amsterdam in the late 1980's, with the related journal *JOLLI* and the ESSLLI summer schools: these were all marked by equal weight on language, logic, and computation.

In the US, the journal *Natural Language Semantics* was launched in 1992 by Heim and Kratzer, to integrate formal semantics more closely

into linguistic theory, and to connect semantics with syntactic theory, unlike the older *Linguistics and Philosophy*, which has always stressed that articles should be readable by both linguists and philosophers. And (Heim and Kratzer 1998) is a fully post-Montague textbook in formal semantics.

But I believe that there is less separation between European and American (and Asian) formal semantics now than ten or fifteen years ago. The recently resurgent *Journal of Semantics*, increasingly oriented towards formal semantics over the last couple of decades, is a good case in point: its associate editors, advisory board, and editorial boards are all quite evenly balanced between North America and Europe, and its published articles come from all over. Faculty and graduate students also move ever more freely across continents, increasing the cross-fertilization of ideas.

I should note a criticism that comes from Lakoff and other "Cognitive Linguists": formal semanticists don't work on metaphor, because formal semantics is inadequate for dealing with metaphor, and deals only with 'easy' parts of natural language. There is probably a grain of truth to this, and it is undoubtedly connected with the relative narrowness of treatments of lexical meaning within formal semantics. (Formal semanticists have a great deal to say about the semantics of "logical words", and about aspects of the semantics of open-class words that impinge directly on their contribution to compositional meaning, but very little to say about what distinguishes the meanings of open-class words whose more 'formal' properties are alike.) I don't believe that formal semantics will ever account for 'all of meaning'. But I believe that it does very well at accounting for the truth-conditional core of literal meaning, which is not handled in any explicit way within Cognitive Linguistics. And we're getting better at solving problems, and there is progress on semantic issues in language typology, language history, language acquisition, pragmatics and discourse, computational linguistic applications, and more. And as the field has made progress, new questions have opened up. I have really not said anything about the work of the last thirty years, and it is that work by which the fruitfulness of the field can best be judged.

7.3. *In closing: A retrospective look at a retrospective look*

In 1980 I gave a talk in Amsterdam called "The First Decade of Montague Grammar". I recently rediscovered my handout from that talk, and if I leave out the (few) examples I included, it's concise enough to reproduce here. I still largely agree with it.

I. Montague's contributions to linguistics

(1) The applicability of the techniques of model-theoretic semantics to natural language. (EFL, 1970)

(2) The tools of higher-order typed intensional logic (including lambdas)

(3) A general theory of syntax and semantics (UG, 1970), with a strong form of Fregean compositionality; rigorously explicit about syntax, semantics, and the mapping between them

(4) Truth-conditions and entailment as the basic criteria for "observational adequacy" in semantics

(5) A new interpretation of categorial grammar which gave linguists an entirely new perspective on its value (PTQ, 1973)

(6) Basic grammatical relations as function-argument structures

(7) The method of "fragments"

II. Dangers

(1) Montague's Intensional Logic on a pedestal

(2) Bias towards English and related languages

(3) Assumption of fixed set of possible worlds

(4) Acceptance of intensions as Fregean senses

III. Big Advances

(1) Natural language syntax is not crazy

(2) Less burden on syntactic component—same meaning does not require same deep structure (Thomason 1976); new possibility of English as a context-free language (Gazdar 1982)

(3) Quantification and pronouns, tense and aspect, comparatives, questions, temporal and locative adverbs, relative clauses and other WH-constructions, . . . [by now this list would be MUCH longer]

(4) Formal pragmatics: demonstratives, indexicals, interaction of context and content, counterfactual conditionals

(5) Opening up questions of universality—languages with no articles, no count/mass distinction, no singular/plural distinction, no quantifiers without '"classifiers"—different logic?

IV. Attacks

(1) Propositional attitudes: logically equivalent sentences are given the same intension; seems wrong to many

(2) All the non-logical, non-metaphysical modalities [but with the pioneering work of Kratzer (1977), this problem became much more tractable]

V. Central foundational issue: Integration of model-theoretic semantics with a theory of human semantic competence

(1) Epistemological question: What is it to know a language, if language is as Montague describes it? And what is it to understand a sentence?

(2) Then we don't know our language, since we can't always recognize entailment and can't always determine that logically true sentences are true

(3) Two responses

(a) Dummett, intuitionists, procedural semanticists, "psychologistic" linguists: a theory of meaning must be a theory of human semantic competence, so Montague et al are wrong

(b) Putnam, David Lewis, Kripke (?): distinguish theory of reference from theory of understanding; what we know (together with various external facts) determines what the truth-conditions of our sentences *are*, but doesn't by itself provide an algorithm for recognizing them Analogy: What is it to "know" first-order logic?

(4) Why it's not *just* an epistemological question:

   (a) Meanings of words are determined in part by what sentences we hold true
       ("socialism", "pragmatics", "time", "motion")

   (b) Problems of propositional attitude sentences seem linked to limitations of finite brain and to interrelatedness of meaning and beliefs

   (c) Assumption of a single set of all possible worlds seems unrealistic

   (d) Possible picture [this has never been pursued by me or by anyone else, as far as I know]: "Object language" L1; semantics: finite partial models, extended to infinite models by finite descriptions in metalanguage L2; semantics of L2: finite partial models, extended by descriptions in L3; . . .

The very last conjecture was invented for the occasion and really has no place in the historical account. There is less focus on the problems of propositional attitude sentences now, I think, but not because they have been solved. And the list of domains in which big advances have been made is now much longer, and the focus on English and related languages is now beginning to be broadened to a wider typological perspective. But overall, I believe the highlights of the history of the first ten years that I identified in 1980 are a reasonable summary of the significant features of the early history of formal semantics, and many of them would still belong in a summary of the main features of the first forty years.

Notes

[1] But I'm trying to 'become' a bit of a historian: I'm starting work on a book on the history of formal semantics, going beyond what I know first-hand. I am still in the early stages of research and interviewing; I expect that the project will take several years. Earlier work of mine on the history of formal semantics includes (Partee 1996, Partee 2004, Partee 2005, Partee 2009, Partee & Hendriks 1997). Some topics that are here discussed much too briefly (or not at all) are discussed more fully in those papers; some will be treated more fully in the book.

[2] This paragraph is partly speculative; I still have more to learn about this period.

[3] When David Kaplan heard about Chomsky's reply to Bar-Hillel, he said it reminded him of Quine's vehement rejection of Kripke's work on modal logic (David Kaplan, p.c. January 2011).

[4] From a letter from Bar-Hillel to Montague, 20 Nov 1967 (in the Montague archives, UCLA).

[5] 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 at that time neither I nor any linguist I knew had any idea what a semantic theory should look like.

[6] Later Katz became a Platonist about meanings, but with a semantics still couched in terms of 'semantic markers' (Katz 1981); formal semanticists found his later theories

similarly inadequate (Pelletier 1982).

[7]The actual terminology, and indeed the actual architecture, of deep structure and surface structure didn't come until *Aspects* (Chomsky (1965)), when generalized transformations that combined kernel sentences into complex sentences were eliminated, and the structures underlying all the constituent kernel sentences were combined into a single underlying phrase-structure tree, the deep structure.

[8]I recall hearing Chomsky say things around that period like "We don't really understand much about semantics, but what Katz and Postal are doing seems promising." In the book, he definitely accepts the hypothesis with only a few caveats in footnotes. He expresses quite nuanced worries about the apparently non-synonymous active-passive pair (2a–b). He offers some "independent motivation" for making the Passive transformation an obligatory rule triggered by the presence of an abstract "Manner adverbial" *by+Passive* in the deep structure, but notes that that doesn't solve the semantic problem, since the semantic effect of Passive transformation is by no means so uniform as that of the Negation transformation or the Yes-No Question. He considers the possibility that in some abstract sense, both members of the pair (2a–b) are ambiguous (which is probably the majority view now), and some other principles may conspire to block one or the other reading at Surface Structure.

[9]For help with this section I am indebted to more conversations and sources than I can remember; my sources include at least (Cocchiarella 1997, Soames 2010, Stanley 2008) and conversations with Dagfinn Føllesdal, Joseph Almog, David Kaplan, and several others, in addition to my own rather eclectic education in philosophy.

[10]Not without some controversy; see Janssen (1983). And see Hodges (2001) for a nice statement of the relation between compositionality and contextuality in discussing whether Frege really did espouse compositionality. And see Pelletier (2001) for a third evaluation, considering Frege committed neither to compositionality nor contextuality.

[11]It has been observed in a number of works (I learned it from Tarski (p.c.)) that the usual semantics for the quantifiers of first-order logic in terms of satisfaction and assignments is not strictly compositional.

[12]An expanded version of this section would of course have to say something about Austin and Ryle.

[13]This section draws on the first three pages of Partee (1988), with updates at several points.

[14]Thanks to Fred Landman for alerting me to Montague's early role in these developments.

[15]Hintikka (1973) says that Carnap did not take the retrospectively natural further step of analyzing intensions as functions from possible state-descriptions to extensions, as Montague later did. But according to Joseph Almog (p.c. January 2011) Carnap did do that, and can be considered the inventor of model theoretic semantics for modal logic.

[16]In a footnote Montague indicated that since his paper "contains no results of any great technical interest", he had not originally planned to publish it but in light of the publication of Kanger's and Kripke's papers, "together with the possibility of stimulating further research", he reconsidered that decision.

[17]Thanks to Nick Drozd (p.c. 2009) for alerting me to this quotation and its revision.

[18]Montague's handwritten manuscripts and notes often include some shorthand; I hope I am deciphering these remarks correctly. I am sure enough about the overall content, but I might have a few words wrong. I will continue to work on learning his shorthand, which should be possible.

[19]Montague in EFL notes that his treatment of quantification "bears some resemblance to the rather differently motivated treatment in Bohnert & Backer (1967)" (p.189 in Montague (1974)).

[20]My plans for the book include studying these handwritten pages to try to figure out as much as I can about what he was trying to add and what difficulties he was finding; it will be interesting to compare his attempts with subsequent research on the constructions he considered.

[21]I found the mention of Curry in my notes from a colloquium presentation by Montague at UCLA on October 25, 1968 (on EFL rather than PTQ - he was thinking in function-argument terms and acknowledging both Ajdukiewicz and Curry before he started using categorial grammar notation for his grammatical categories.)

[22]Montague's choice of words was so memorable that my own memory thirty years later turned out to agree exactly with what Larry Horn—already working on negation—had copied down in his notebook.

[23]Paul Dekker (p.c.) gently asked me, when this description appeared in Partee (2005), why I mention the only woman in that list, a well-known linguist, as so-and-so's wife. I think Sally herself would say that she was invited only because of her philosopher husband. In those days the scarcity of women in philosophy was all too familiar. At that Institute I was the only woman among the faculty (and I was there as a linguist) and I don't recall there being more than one or two women among the hundred or so "students".

[24]Thanks to Paul Postal (p.c.) for reminding me that Jim McCawley was one of the first linguists to argue for the importance of doing recursion on open sentences, at least as early as McCawley (1968). And thanks to Fred Landman (p.c.) for pointing out the difference between "doing recursion on open sentences", necessary if binding is done directly by quantifiers as in Frege's pioneering work, and having binding done by lambda abstraction. It was Montague's innovative use of lambda abstraction as the active variable-binding operator in PTQ that enabled a unified treatment of variable binding in connection with quantification, relative clauses, and interrogatives. As Polly Jacobson (Jacobson (1999) and earlier works) has shown, it may not be strictly necessary for the grammar to generate open sentences; but abstraction still appears to be crucial.

[25]In the last of the mentioned papers, I argued that some of the phenomena that make the semantics of belief-sentences so difficult may be related to a real open-endedness and partial indeterminacy in the semantics of our language that means that the idea of a complete semantics for a human language is a phantasm. But this is speculation; I have no theory to go with it.

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