To appreciate the development of pragmatic theory in linguistics, one must recall several classic papers of the late 1960’s and early ‘70’s, during a period of development of Generative Semantics. The attempt was made to explain pragmatic phenomena within a grammatical theory. John R. Ross’s famous 1970 paper “On Declarative Sentences” put J.L. Austin’s performative verbs into underlying structure. Jerry Sadock’s 1969 “Hypersentences” and his 1970 “Whimperatives,” Sadock’s book (1974) *Toward a Linguistic Theory of Speech Acts*, and the papers preceding it extended the performativ analysis to indirect speech acts. George Lakoff’s 1971 papers “On Generative Semantics” and “Presupposition and Relative Well-formedness” and his 1972 paper “Linguistics and Natural Logic” used Perlmutter and Postal’s “transderivational constraints” to account for presuppositions without embedding them in the logical structures of sentences. The famous Gordon and Lakoff paper of 1971 “Conversational Postulates” tried to account for Searle’s 1975 analysis of indirect speech-acts by trans-derivational conversational postulates, which utilized context-dependent entailments. Essentially this was an effort to reduce pragmatics to context-relative semantics. By 1975 both Sadock, in “The Soft Interpretive Underbelly of Generative Semantics,” and Georgia Green had noted, in criticism of Lakoff, that the use of global syntactic rules that referred to conversational entailments of deep structures undermined the basic assumptions of Generative Semantics.

But by 1969/1974 Bruce Fraser and others had undermined Ross’s arguments for the performative hypothesis. Gerald Gazdar’s (1976/1979) doctoral thesis undermined Lakoff’s conversational postulate approach via entailments, as did Jerry Morgan’s 1977 “Conversational Postulates Revisited.” And Ruth Kempson’s 1975 book on presupposition undermined Robin Lakoff’s 1971 paper “If’s, And’s, and But’s about

Lakoff’s effort to semanticize pragmatics was of great interest, and it was an issue where I myself played an early though minor role. Lakoff had published a paper in “Foundations of Language” in 1972 on “Performative Antinomies”, like ‘Don’t obey this order!’ This treatment was modified in 1973, for Ed Keenan’s Cambridge conference, in Lakoff’s paper “Pragmatics in Natural Logic.” But the original paper was a paradigm example of the reduction of pragmatic facts to semantics. Lakoff tried to show that sentences like ‘Don’t obey this order!’ literally contain as content a semantic paradox, like ‘This sentence is false’. What made the exercise of interest to me was that the reduction obviously failed. The paper as published contained a basic logical fallacy, which I pointed out in a squib I wrote on the paper a week or so after it was published in the autumn of 1972. I sent the criticism to Lakoff for his reaction, and then I spent three hours on the telephone with him in December 1972, going through my criticism of his argument line by line. Lakoff’s “Pragmatics in Natural Logic” paper at the April 1973 Cambridge conference discusses (1975, pp. 266ff) theretofore undiscussed data known as “declarative antinomies,” the data and theory of which Lakoff took from my unpublished critical 1972 essay. What is notable is that there is no reduction of the performative antinomies to semantic paradoxes in the Cambridge version of the paper. That project had failed. The effort to semanticize pragmatic “paradoxes” was shown to be unsuccessful.

But during the period of Generative Semantics of the late 1960’s and early 1970’s, linguists had become aware of the work done by philosophers of language of the period: Waismann, J.L. Austin, John Searle, Peter Strawson, H. Paul Grice, Quine, Davidson, Dummett, Ryle and his students. They had also become aware of the logical tradition in logical empiricism: Frege, Russell, Wittgenstein, Carnap, Reichenbach, Tarski, Dana Scott, Richard Montague, John Lemmon, and Arthur Prior, which had influenced Generative Semantics.

With the decline of Generative Semantics, the influence of Oxford “ordinary language” philosophy remained. In 1967 Grice had given the William James lectures at Harvard on “Logic and Converation.” Manuscript copies of the lectures circulated, and were duplicated in their entirety for George Lakoff’s course at the Linguistic Society of America Summer Institute at the University of Michigan, Ann Arbor, in the summer of 1973. For the first time many linguists were able to see the whole of the William James Lectures. In the spring of 1973, there had been a major linguistics and philosophy conference on Performatives, Implicature, and Presuppositions at the University of Texas, Austin.

It was at that 1973 conference that George Lakoff introduced me to Stephen Levinson, then still a graduate student at the University of California, Berkeley, where Lakoff was now teaching, along with Grice. I had just left Princeton one year before, to take up my first teaching position in a Philosophy Department in southern California. I was 27 years old; Levinson was three years younger. When I was introduced to him, he was wearing those then-trendy little “granny glasses,” and his hair was long, down nearly to his shoulders, sun-bleached by all the outdoor swimming he did at Berkeley. He was
wearing a purple and pink, horizontally striped T-shirt, and a pair of faded, pink bell-bottom trousers. I was in a my East Coast, Ivy League university academic uniform: white shirt, regimental tie, a blue “blazer” jacket, and dark – probably gray – trousers. We were an incongruous sight together. But I discovered that he had an elegant mind: quick, acute, imaginative. Several days of the conference were spent in our own long discussions about performatives, implicature, and presupposition. In March of 1972 Paul Grice had given me a copy of the William James Lectures, when he had visited Princeton. I had read it with interest, but without enthusiasm. Grice’s rescue of classical logic as a language of semantic representation, in opposition to Strawson’s and others’ views, by the use of a refinement of his 1961 notion of implicature impressed me, and I understood why Generative Semanticists had found it so seductive an idea. But I had wanted to formulate his theory of implicature in a more rigorous, more precise way, without knowing how to do so.

At the end of the conference, I was sitting next to George Lakoff on the coach to the airport, and he asked me whether I should like to be a participant in a Workshop on the Formal Pragmatics of Natural Language at the University of Michigan in the summer of 1973, just a few months hence. I said that I would come to Ann Arbor only if he invited Levinson to attend as a participant as well. He agreed, and so several months later, Levinson and I went to Ann Arbor, where for the first time I met Jerry Sadock, Larry Horn, Lauri Karttunen, Jerry Morgan, Georgia Green, and untold others. I lectured on Frege’s semantic, pragmatic, and speech-act accounts of presupposition, all in “Sinn und Bedeutung” – Michael Dummett, who had taught me in Princeton as a Visiting Professor and then given me a Tutorial on his Princeton lectures on Realism as we sat weekly in the Fellows Garden of All Souls in June, July, and August of 1971, had just published his book on Frege’s Philosophy of Language, and I spent part of the summer in Ann Arbor reading it, thinking about van Fraassen’s supervaluation theory of semantical presupposition, and working intensively with Levinson, thinking through Grice’s William James Lectures.

My sudden realization that ‘not’ in definite description sentences was not scope-ambiguous – the negative part of the Atlas-Kempson Thesis – came upon me in a curious way. I was walking with Levinson at dusk in the quadrangle of the University of Michigan Law School in July 1973. I was trying to explain to him why van Fraassen’s construction of supervaluations could not give an explanatory account of presupposition. I had explained that the semantical theory had required van Fraassen to posit two negations, a choice or predicate negation and an exclusion or sentence negation. But the theory had some peculiar logical consequences in the unconstrained way in which supervaluational validity and entailment were defined. I remember saying to Levinson, “These consequences ultimately derive from the ambiguity premise, and they make no sense. The only solution is to reject the ambiguity premise, but that is as solid a doctrine as we have. Yet, it can’t be. Negation cannot be ambiguous! It just can’t be. So, what is it?” Three weeks later Jerry Sadock was presenting his and Arnold Zwicky’s paper at the Summer Meeting of the Linguistic Society of America. It was in the cavernous hall of the Modern Language Building. I walked into the hall in the middle of Sadock’s 1973 paper on “Ambiguity Tests and How to Fail Them,” late as ever, sat down, and listened.
At the end, I thought to myself, “That bears on my negation problem, but he did not discuss the ambiguity of negation in the paper! I have to get a copy of the paper from him and work it out.” Several months later Sadock sent me a copy of the paper. I put it on the shelf untouched. Several months after that, in May of 1974, I began my long vacation in Pennsylvania, outside of Philadelphia. I read the first, introductory paragraph to Sadock and Zwicky’s paper, and I thought to myself, “That’s it. If ‘not’ is not ambiguous, it’s semantically non-specific. Let’s try the tests.” Ten minutes later I had satisfied myself that ‘not’ in definite description sentences failed Sadock’s ambiguity tests. And then the power of Grice’s notion of generalized conversational implicatural inference hit me full on. The solution had to be in the utterance-meanings, in one of two ways. Either the utterance-meanings were produced by a Griceanish inference, not from classical logical forms as Grice thought, e.g. sentential exclusion negation, but from a non-specific, semantical representation of a sentence-type whose meaning was not that of either a choice or an exclusion negation proposition, or the utterance-meanings were produced by a classical Gricean inference from the sentential exclusion negation logical form, which on classical Gricean grounds one would have to “posit” as the semantic content of “what is said” in the asserted sentence – Grice’s version of the “minimal proposition.” That was the positive part of the Atlas-Kempson Thesis. I then wrote a short essay a few weeks later, “Presupposition, Ambiguity, and Generality: a Coda to the Russell-Strawson Debate on Referring,” (Claremont, California: 1974), an unpublished manuscript that Larry Horn later quoted (p. 135) and discussed (pp. 135-6) and cited (p. 207) in his “Some Aspects of Negation,” in Volume 4: Syntax of Joseph Greenberg’s Universals of Human Language, Stanford, 1978. I published a brief discussion of the non-specificity of negation in my 1975 paper on “Frege’s Polymorphous Concept of Presupposition and Its Role in a Theory of Meaning,” in Ryzard Zuber’s Paris journal Semantikos, Volume 1, pp. 29-44, which was my Ann Arbor lecture of 1973, and finally an expanded version, in 1977, “Negation, Ambiguity, and Presupposition,” Linguistics and Philosophy, Volume 1, pp. 321-36. But there is more to the story.

The next summer, the summer of 1975, I was spending the long vacation in Amherst, Massachusetts. In June I was looking at my just-published Semantikos paper, in which I was defending my view that there were presuppositional and non-presuppositional understandings – not senses -- of a negative sentence, and in which I wrote (n. 23, p. 42), “I defend this view elsewhere. It rests on the observation that such negative sentences do not have ambiguities caused by different scopes of ‘not’. Instead they are “general,” “non-specific,” or (in G. Lakoff’s sense) “vague” [by which Lakoff meant ‘non-specific’] between a presuppositional and non-presuppositional understanding.”

In July of 1975, Ruth Kempson’s Presupposition and the Delimitation of Semantics, Cambridge University Press, the book form of her 1973 PhD dissertation, was published. When I saw the announcement of its publication, since I was actively thinking about presupposition, I ordered the book. In late July or August I read it. On p.16, to my astonishment and pleasure, Ruth Kempson, referring to a syntactic test that had been discussed by George Lakoff and Jerry Sadock and distinguishing between ambiguity and non-specificity, which she, following Lakoff, called ‘vagueness’, stated that “a negative
sentence has one underlying structure (given that it is not ambiguous for independent reasons) and is interpreted as vague in its interpretation by a semantic rule which predicts a disjunct set of possible readings.” It was the bit about disjuncts that gave me indigestion – the disjunct idea occurs also, but not seriously, in Deirdre Wilson’s thesis, and only in passing -- but otherwise, I thought, there are only two people in the world who believe this: Jay Atlas and Ruth Kempson. On p. 17 Ruth continued, “The inherent vagueness of negative sentences has not been widely recognized among linguists.” Now there is a case of British understatement. She continued, “In fact attempts have been made to predict a fully specified scope of negation, and these attempts have led to a proposed revision of the standard (1965) theory’s claim that the input to the semantic component is the set of deep-structure phrase markers. There are three chief protagonists to this issue: -- G.N. Lakoff (1970, Irregularity in Syntax), Jackendoff (1969, “An Interpretive Theory of Negation”), and Chomsky (1971, “Deep Structure, Surface Structure, and Semantic Interpretation”), of whom two (Lakoff and Jackendoff) base their arguments on the mistaken assumption that negative sentences are fully determinate in their meaning and scope-specification, and that they are therefore frequently ambiguous. The point at issue is this: are deep-structure phrase markers a sufficient input for the semantic component to be able to predict the interpretations of sentences, or should this claim be dropped in favour of a weaker claim that information from both deep structure and surface structure is necessary in order to predict the meanings of sentences?” Kempson concludes the first chapter parting company with the Generative Semanticists, arguing that semantical selection restrictions are not evidence for syntactic deep structure and further arguing that rules of semantic interpretation for negative sentences are not isomorphic to syntactic rules.

Kempson’s criticism of Robin Lakoff’s paper on ‘and’ required an extensive discussion of Grice’s Maxim of Relation, the imperative to Be Relevant. No doubt this, along with Deirdre Wilson’s published thesis, also in 1975, began that British obsession with Relevance that has blossomed into Relevance Theory.

In the United States, Larry Horn’s 1972 UCLA PhD Thesis “On the Semantic Properties of Logical Operators in English” was supervised by Barbara Hall Partee. Horn’s thesis further convinced American linguists that Grice’s William James Lectures were linguistically important. Gerald Gazdar’s 1976 thesis and the 1979 book Pragmatics: Implicature, Presupposition, and Logical Form, by its formal approach to the projection problem for presupposition, suggested that there was more order in the phenomena than one might have first expected.

In May of 1978 I gave a lecture in the Department of Linguistics and Phonetics, University College, London, “Grice’s Pragmatics: Some Foundational Questions,” in which I talked about the relationship between semantical non-specificity and Gricean implicature. (A version was published in 1979.) Sitting in the second row were Deirdre Wilson and Ruth Kempson, imbibing more of the heady drafts of underdeterminacy and its consequences for semantics and pragmatics. In the summer 1978 Gerald Gazdar gave a series of lectures, based on his thesis, at the Summer Institute of the Linguistics Society of America, at the University of Illinois, Champaign-Urbana. I was supposed to meet
Levinson there, but I was two weeks late. A Trinity Hall medical student Charles R.V. Tomson, who had sat his Moral Sciences exams that spring, mostly in Linguistics as a student of Levinson’s, was visiting me in California. (He is now a distinguished renal specialist.) When I showed up I went to Gerald’s lectures. After the first one I heard, I went back to the Student Union hotel, where it was air-conditioned, to lunch with Levinson. The temperature was over 90* F, and the corn-stalks were parched in the fields. Levinson asked, “What did you think of Gerald?” Atlas: “Impressive, but wrong-headed. There are more principled ways for a Gricean to achieve his results for cleft sentences.” And picking up a paper serviette and my pen, I began outlining what became Atlas and Levinson, “It-CLEFTS, INFORMATIVENESS, AND LOGICAL FORM,” 1981, in Peter Cole’s collection, Radical Pragmatics. Levinson looked at the serviette and said, “We should write this up.” A: “It’s too hot to write anything. But if we go upstairs to my air-conditioned room, I will dictate it to you.” So up we went. I lay down, Levinson took out his pen, and horizontally I dictated the first, short version of Atlas and Levinson 1981. A: “We shall have to modify Grice’s Maxims and so on.” After I dictated about 4000 words, we stopped. A: “Do you think Gerald will be convinced?” L: “Let’s ask him.”

A year later, in July 1979, Levinson visited me in California for a month. He brought a letter from Jerry Sadock, who had read the dictated paper. Sadock had astutely noted a difficulty with the proposed logical forms for clefts, and he had other criticisms. After reading the letter, I said to Levinson, “Sadock is completely right. This has to be entirely re-thought.” So Levinson and I thought about it further, and in the spring of 1980 I sat down and wrote the version that is in the 1981 Cole volume, working from Levinson’s notes and my own. The paper is now regarded as one of the standard criticisms of semantic presupposition, and its revisions of Grice’s maxims have led directly to the views of Gricean inference discussed in Levinsons’ 2000 book Presumptive Meanings: a theory of generalized conversational implicature, with its trinitarian account of Gricean inference, and to Horn’s 1984 “Toward a New Taxonomy for Pragmatic Inference: Q- and R-based Implicature,” which creates a dual-maxim, gnostic account of Gricean inference. Sperber and Wilson’s Relevance Theory appeared two years later, in 1986, with a monotheistic, i.e. monistic, account.

One should have expected that eventually the pragmatic atheists would weigh in and provide the null maxim account. And they have; they are semantic Indexicalists, about which I shall speak later in the day.

To summarize: Generative Semantics, and reactions to it, led to Gricean Pragmatics. In America we have Sadock and Horn and Levinson to thank. In the United Kingdom we have Levinson, Gazdar, Geoffrey Leech (about whom I have not said enough), Kempson, and Wilson to thank.

And I thank you all.