Review of Syntax & Semantics 9: Pragmatics,
Peter Cole, New York: Academic Press, 1978.
To appear in Journal of Pragmatics.

Gerald Gazdar and Ewan Klein
School of Social Sciences
University of Sussex

June 1979

This collection contains the following papers: "On the origins of referential opacity" by Peter Cole; "Negative scope and rules of conversation: evidence from an OV language" by Alice Davison; "Speaker references, descriptions and anaphora" by Keith Donnellan; "Negation in language: pragmatics, function, ontology" by Talmy Givon; "Further notes on logic and conversation" by H. Paul Grice; "Remarks on Neg-Raising" by Laurence Horn; "DTHAT" by David Kaplan; "Conversational implicature and the lexicon" by James McCawley; "Two types of convention in indirect speech acts" by Jerry Morgan; "On testing for conversational implicature" by Jerrold Sadock; "Synonymy judgements as syntactic evidence" by Susan Schmerling; and "Assertion" by Robert Stalnaker. With two exceptions, the papers fall naturally into three distinct groups: those by Davison, Givon and Horn deal with negation; those by Grice, McCawley, Morgan, and Sadock concern implicature; those by Cole, Donnellan, and Kaplan discuss issues arising out of the referential/attributive dichotomy; whilst those by Schmerling and Stalnaker don't fit comfortably into any of these three categories.

Davison's paper discusses the ways speakers of Hindi-Urdu avoid the ambiguities that one might expect the rather inflexible syntax of negation in that language to give rise to. She shows that various aspects of the syntax and lexicon of the language are exploited by reference to Gricean principles of cooperative conversation in order to make the intended reading apparent.

Givon's paper begins by surveying a wide range of facts drawn from diverse languages indicating that the range of environments in which negation can occur is quite restricted, and that even within those environments its attested frequency is relatively low.[1] He then develops a pragmatic explanation for these facts in terms of the Gestalt notion of figure/ground. Despite gratuitous references to Newtonian physics and Taoist philosophy (p109), his explanation seems both plausible and enlightening. Which is more than can be said for his logic. Thus he talks about "truth values" (p71) when he means "truth conditions", he confuses entailment with material implication (p83), claims that $p \rightarrow q$ and $\neg p \rightarrow q$ are contradictory (p83) which they aren't, and claims that $\neg(\neg p \rightarrow \neg q)$ follows from $p \rightarrow q$ (p83) which it doesn't. Luckily these solecisms merely distract from, rather than detract from, the main thrust of his argument. And they are more than redeemed by some timely methodological remarks (p90-91) he makes about the smugness inherent in some linguists' use of the notion of "markedness". These remarks have a particular relevance to the current scene in view of the widespread tendency of one school of syntacticians to "reanalyse" counterexamples to conditions on rules as "highly marked constructions" (objecting to this ploy is known as "naive falsificationism").

Horn's 90 page paper constitutes the most thorough exploration of a single grammatical rule since Postal's epic 1974 book on Raising. And it deserves the same kind of attention that Postal's work has received for it is without question the definitive paper on one of the least tractable problems in syntax/semantics/pragmatics. Horn collects together all the facts, considers all the arguments, and cites all the previous work on the topic, no matter how obscurely published. Rather than attempting to do justice to this paper, which alone warrants purchase of the book, we will restrict ourselves here to noting the existence of a perplexing class of example, of a type not discussed in Horn's paper:

(1)   He did not say what he did not wish, which
       was to be involved in any way with Certain Quarters.

This attested example, which is from Ambler (1972:217), appears to means the same as (2), an observation which is supported both by the context in which it occurs in the novel, and by the presence of negative-polarity any in the relative clause:

(2)   He did not say what he wished, which was not
       to be involved in any way with Certain Quarters.

The example is perplexing because the negative element in (1) must have been raised out of a wh island if (1) derives from (2) via a syntactic rule of Neg-Raising (pace Horn's remarks on p153).

Grice's paper begins by reviewing the criteria he gave in his 1975 paper for detecting cases of conversational implicature, and illustrates the issues involved by considering whether or should be analysed as having one sense or two. He then proposes a principle which he calls Modified Occam's Razor: Senses are not to be multiplied beyond necessity. This principle is the implicit or explicit motivation for much recent work in pragmatics. The paper concludes with three short, and largely independent, sections on stress, irony, and truth.

Sadock devotes his whole paper to a critique of the criteria with which Grice's paper begins. Grice himself admits to a doubt whether the criteria he adumbrates constitute a "knock-down test" (p115), but Sadock concludes that we have "no way of knowing for sure whether an implicature is conversational" (p296). Much of what Sadock has to say about conversational implicature is well worth saying, but the discussion is flawed by his adoption of Karttunen & Peters' obfuscatory and question-begging decision to refer to phenomena previously referred to as "presupposition" by the term "conventional implicature". Thus he claims (p295) that Karttunen & Peters (1975) have shown that conventional implicatures have to be entailed by the common ground if the expressions that induce them are to be used appropriately. But this isn't true if conventional implicatures are identified with presuppositions. Gazdar (1979:106) gives the following example involving a definite description:

(3)   I'm sorry I'm late, my car broke down.

Clearly, this can be uttered appropriately in a situation whose common ground does not entail that I own a car. Karttunen & Peters implicitly acknowledge the force of such examples in more recent work (1979) in which they only require conventional implicatures to be uncontroversial with respect to the common ground. Another difficulty concerns cancellability: Sadock assumes that conventional implicatures cannot be cancelled and infers from this that cancellability is "the best of the tests" (p292) for distinguishing conversational from conventional implicatures. The trouble here is that if presuppositions are (a subset of) conventional implicatures, then the noncancellability assumption, though endorsed by

Karttunen & Peters (1975, 1979), is false, as Gazdar (1979) and Soames (forthcoming) have demonstrated at length. So cancellability will not help one to distinguish presuppositions from conversational implicatures. And it may be that it should not, for what is question-begging about the Karttunen & Peters terminological revision is precisely its built-in assumption that presuppositions are CONVENTIONAL aspects of meaning. But this is not obviously true: after all, the fact that <u>regret</u> presupposes its complement, but <u>hope</u> does not, may be explainable in terms of the interaction of their conventional truth-conditional meanings with Gricean maxims (see Gazdar (forthcoming) for specific proposals). So presuppositions (aka "conventional implicatures") may in fact turn out to be a specie of conversational implicature.

Morgan, in an interesting and important paper, develops a notion of "conventions of usage". The idea is that there may be a number of distinct ways of conveying some meaning but that, over time, it becomes conventional to use one of these ways rather than any of the others. When the way chosen involves a conversational implicature, this implicature becomes, in a sense, conventionalized in that hearers no longer have to calculate the intended meaning via the maxims (although they still COULD do so) but rather can merely consult their knowledge of the usage conventions of the language. Morgan argues, very plausibly, that <u>Can you pass the salt</u> involves exactly this kind of "short-circuited implicature" (p274). There's an analogy to be drawn here between Morgan's notion of a "short-circuited" inference component, and Becker's (1975) notion of a phrasal lexicon, which is a short-circuited syntactic component: a lexicon which contains not just words but also frequently used phrases in order to speed up language production and perception.

McCawley's paper is an attempt to use conversational implicature to explain the apparent differences in meaning between lexical causatives (e.g. <u>kill</u>) and the paraphrases (e.g. <u>cause to become not alive</u>) assumed by the lexical decomposition approach to the analysis of word-meaning. The argument runs along the following lines: (i) lexical causatives are syntactically less marked than periphrastic causative constructions, (ii) direct causation is epistemologically less marked than indirect causation, consequently (iii) use of the marked syntactic form will conversationally implicate indirect causation, and (iv) use of the unmarked syntactic form will conversationally implicate direct causation. The premises of this argument have the post hoc flavor almost always associated with markedness claims. For example, if the facts had been otherwise, it would have seemed just as plausible a priori to have claimed that periphrastic causatives are syntactically less marked than lexical causatives since the former are constructed by reference to general syntactic rules whereas the latter are idiosyncratic lexical entries. It is also unclear to us how one would define the notion of "directness of causation" other than ostensively by references to the very meaning differences that it is employed to explain.[2]

Actually, it is not at all clear that McCawley is arguing for (iv) since he remarks at one point in the paper that "lexical causatives are assumed to have meanings restricted to direct causation" (p249). And this assumption is supported by the observation that the putative conversational implicature claimed in (iv) cannot be cancelled. But if the directness is a part of the MEANING of lexical causatives, but not a part of the meaning of the paraphrases, then the major point of the critics of lexical decomposition appears to have been conceded, and the invocation of conversational implicature to explain the meaning differences is otiose. McCawley's conclusion "that analyses involving lexical decomposition are both harder to confirm and harder to refute than has so far been thought the case" (p258) leads us to conclude that such analyses are probably best consigned to the history of the discipline.[3]

Like McCawley, Schmerling is also concerned with providing a nonsemantic explanation for an apparent meaning difference, in her case the lack of synonymy between examples (4) and (5):

(4)   I allowed the doctor to examine John.
(5)   I allowed John to be examined by the doctor.

But unlike McCawley, and the other contributors so far considered, she argues (p309) that Gricean "principles of conversation as such have no direct bearing on the judgments" of (non)synonymy that she discusses. Instead, the burden of explanation falls upon a perceptual strategy that she formulates thus: "Informants assign the most complex interpretation possible to a stimulus sentence" (p304). Crucial to this claim is the definition of complexity. Schmerling appears to be assuming that A is more complex than B if and only if XAY entails XBY but not conversely, as the following remarks indicate: "A sentence with <u>allow1</u> ENTAILS a sentence with <u>allow2</u> but not conversely. Another way of stating this is that an <u>allow1</u> interpretation is more complex than an <u>allow2</u> interpretation" (p309). But if the definition of complexity is as we have given it, then there exist examples which falsify her perceptual hypothesis.

(6)   a.   There's a dog in the garden.
        b.   There's a male canine in the garden.
        c.   There's a canine in the garden.

(7)   a.   The door was closed.
        b.   Some entity caused the door to
                become not open.
        c.   The door was not open.

The a sentences are ambiguous between the b and c interpretations. The b readings entail, but are not entailed by, the respective c readings. Therefore the b readings are more complex. Therefore informants who are presented with the a sentences as stimuli will naturally assign them b, rather than c, interpretations. However, informal experimentation strongly suggests that this clearcut prediction of Schmerling's hypothesis is, in fact, false. Our subjects unanimously prefer the c readings to the b readings.

The referential/attributive distinction (henceforth RAD) can be approached in basically two ways: one can either claim that there is something special about the meaning (or use) of definite descriptions, or else try to explain the distinction in terms of some more general linguistic phenomenon such as scope, indexicality, or Gricean implicature. On methodological grounds, the second approach seems preferable. Nevertheless, the first strategy is also defensible, and it is the one adopted by Donnellan in his paper. He is extremely cautious, however, about specifying just what is so special about definite descriptions, and devotes most of his effort to showing that the distinction, even if not amounting to a semantic ambiguity, has "semantic significance". This arises, he claims, from the fact that anaphoric pronouns can refer to the INTENDED, rather than the semantic, referent of an antecedent definite NP. Consider, for instance, the following dialogue (= Donnellan's (6a) plus his (8)):

(8)   Mr. Smith: The fat old humbug we met yesterday
                has just been made a full professor.
        Mrs. Smith: I don't think he's fat; he's just
                large boned. And as for his being a
                humbug, he seemed quite genuine and
                above board.

Clearly, the occurrence of _he_ in Mrs. Smith's rejoinder cannot refer to the semantic referent of _the fat old humbug we met yesterday_. Nevertheless, as Kripke (1977) points out, this data does not militate against treating the RAD as a purely pragmatic phenomenon. For there is abundant evidence to suggest that the reference of pronouns is determined on the basis of contextual factors, rather than strictly semantic ones. For example, we can construct an example somewhat like Donnellan's in which the antecedent of the pronoun has no semantic content whatsoever. Suppose Mr. Smith visits a colleague's office, expecting to find a distinguished visitor there. If Mr. Smith has forgotton the visitor's name, a dialogue such as the following might take place:

(9)   Mr. Smith: Oh, isn't thingummy here?
        Colleague: No, he had to catch a train home.

We remain unconvinced by Donnellan's claim that the RAD involves a semantic distinction, but his paper is well worth reading, not least for his interesting observations on 'indefinite' definite descriptions.

The paper by Kaplan has been circulating in xerox form for a number of years now, and its appearance in print at last is welcome. Kaplan adopts the second of the two approaches to the RAD which we indicated above, in that he explains the referential use of definite descriptions in terms of indexicality. According to the classical analysis of definite descriptions, an NP _the_ N will denote, at any world w, the unique individual (if there is one) who uniquely instantiates the property expressed by N at w. However, Kaplan claims that a description can also be used as a form of pointing, to DEMONSTRATE some individual in the context of utterance. When a description _the^N_, occurring in a sentence S, is used demonstratively at a context c, the sense of S will not be a function of the sense of _the^N_ (as it would be if the description were used nondemonstratively), but a function of the individual picked out by _the^N_ relative to c. How are we to understand this notion of context? According to Kaplan at least part of a context is a possible world, i.e. the world in which an utterance of a given sentence S occurs. For convenience, let us suppose that c is just a possible world. When a description _the^N_ is used demonstratively, in a context c, therefore, it will pick out that individual that satisfies the predicate N in c. Usually, when we say that a sentence S is true at a context c and world w, we assume that w, the world relative to which S is evaluated, is the same as c, the world in which S is produced. Given this assumption, the demonstrative use of _the^N_ will not affect the truth of S at c and w, since an individual will satisfy the description relative to c iff it satisfies it relative to w. In other words, in contrast to Donnellan, Kaplan construes the RAD in such a way that it has no affect on the truth conditions of sentences in which the relevant NPs occur only in extensional contexts. However, the proposition expressed by S (where we take a proposition to be a function from possible worlds to truth values) will be affected by the RAD. For if _the^N_ is used demonstratively, its value will depend only on c, and hence will not vary from world to world. Consequently, if _the^N_ is used demonstratively, it becomes a rigid designator, in the sense of Kripke (1972). Although Kaplan's analysis is extremely elegant and casts an illuminating light on the nature of demonstratives, some doubt remains as to whether he has succeeded in explaining Donnellan's distinction. To begin with, Kaplan seems to accord insufficient weight to the role of the speaker's intentions. Kaplan concedes that there is a discrepancy here between his analysis and Donnellan's original discussion. Given the arguments advanced by Kripke (1977) and Kaplan himself against taking the semantic value of a referentially used descriptio n to be the intended referent, one might conclude that this is a minor failing. Yet we are still left with the conclusion that there is some pragmatic dimension to the RAD which is left uncaptured by Kaplan's demonstrative/nondemonstrative distinction. Second, apart from certain idiomatic and specialized uses, it seems that demonstrative pronouns can only be used appropriately when the demonstratum is perceivable by the addressee (cf. Hawkins (1978)). It is admittedly true that the paradigm examples of referential descriptions are cases where the description could be replaced by a demonstrative. Nevertheless, referential descriptions are not governed by the same appropriacy constraint as overt demonstratives, and this weakens Kaplan's analysis. Last, in order to have a general explanation of the RAD, we need to have clear, independent evidence that certain classes of expression can be used either indexically or nonindexically, i.e. that there are expressions whose semantic value at a reference point c, w is a function either of c or of w. So far, such evidence is lacking.

In his paper, Cole accepts a simplified version of Kaplan's proposal according ˙ to which the RAD just amounts to a rigid/nonrigid distinction. He goes on to propose that this provides the basis for explaining Quine's transparent/opaque distinction. His claim can be stated as follows:

(10)   Let S be a sentence containing a definite
        noun phrase NP such that S is embedded
        beneath a verb of propositional attitude
        V. Then NP will be interpreted transparently
        with respect to V if NP is referential in S,
        and NP will be interpreted opaquely with
        respect to V if NP is attributive in S.

Although Cole does not make his position altogether clear, it seems reasonable to conclude, in addition, that if the NP _the restaurant on Broadway between Grant and Stockton_ in (11) (= Cole's (10)) is interpreted transparently, then it must be referential not only with respect to the complement S, but also with respect to (11) as a whole:

(11)   Tom believes that the restaurant on Broadway
        between Grant and Stockton serves great dim sum.

Yet it is not in general true that transparent NPs are always used referentially (with respect to the matrix sentence). Consider example (12) (from Klein (1978:140)):

(12)   All my first year students believe that the
        currently most fashionable pop musician
        is vastly superior to his predecessors.

(12) could be uttered by a speaker who has no idea as to who is the most fashionable rock musician, but is quite sure that all his students know. Thus the NP in question can be transparent with respect to believe, but attributive for the speaker. Cole claims that his account of the transparent/opaque distinction, as indicated in (10), yields a better approach to the failure of Leibniz's Law in opaque contexts than that afforded by a ·relative scope analysis. However, his arguments here do not seem quite correct. The claim made by (10) is identical to that made by a relative scope analysis complemented by a fairly standard possible worlds semantics, given the conditions that (a) we consider only one level of embedding, and (b) we accept Cole's equation of referential/nonreferential with rigid/nonrigid. For, on a scope analysis, placing a NP outside the scope of V merely ensures that NP is interpreted rigidly with respect to V. Since Cole accepts intensional objects in his semantics, any claim made within his framework can be duplicated within a relative scope analysis. Of course, the relative scope analysis will predict that the number of readings associated with a given NP will rise as a function of the number of opacity-creating operators beneath which NP is embedded. By contrast, Cole's proposal would appear to predict that the number of

readings stays fixed at two. Although it is difficult to base firm arguments on this kind of data -- see Abbott (1976:27-30) for some useful methodological cautions -- Cole accepts that the relative scope analysis makes the correct predictions here, and tries to show that the data can be accomodated within his framework. His proposal is ingenious, involving data from 'manner of speech verbs' of the sort discussed by Partee (1973), and deserves to be studied in more detail than we have space for here.

Stalnaker's paper is very much in the tradition of his earlier work.[4] As in his (1972) article "Pragmatics", he is concerned with developing a rigorous basis for pragmatic theory, and with showing that an adequate pragmatics can "take some of the weight off semantic and syntactic theory" (p331). As the title suggests, his main goal is to give a characterization of assertion, and his main claim is that there is a reciprocal relation between assertions and contexts: the context of use determines, to a certain extent, what is asserted by a given utterance, and the effect of an assertion is to change the context in a systematic way.[5] Let us briefly sketch this latter process. Each participant in a conversation has a set of presuppositions, and these in turn determine a set of possible worlds -- the worlds compatible with those presuppositions. The set of worlds compatible with the speaker's presuppositions is called the CONTEXT SET by Stalnaker; this is "the set of possible worlds recognized by the speaker to be the 'live options' relevant to the conversation". In fact, each participant in the conversation will have his own context set. For simplicity, let us suppose that these all coincide (in Stalnaker's terminology, that the context is NONDEFECTIVE). Then according to Stalnaker, the essential effect of an assertion is to reduce the context set, by eliminating all those worlds which are incompatible with the proposition expressed.

Stalnaker develops this idea in more detail, and then formulates three pragmatic principles, somewhat along the lines of Gricean maxims, which would govern the behaviour of a rational agent in an ideal communication situation. It is then argued that these principles can be used to explain how participants make sense of utterances in contexts. Stalnaker's paper is packed with ideas, sometimes presented in a rather condensed form, and it would be useful to see his claims developed at greater length and presented within a more detailed formal framework. Let us give a brief illustration. Consider the following example sentences from Stalnaker:

(13)   a.   That is either Zsa Zsa Gabor or Elizabeth Anscombe.
         b.   It is now three o'clock.

These have the interesting property that if they are true at all, relative to a given context, then they are necessarily true. For example, if (13a) is uttered in a context in which that demonstrates either of the two women mentioned, then it will be true at every possible world (on the assumption that both demonstratives and proper names are rigid designators). Stalnaker makes the important observation that such sentences can nevertheless be informative because we typically do not what context we are in. That is, while our utterances take place in some given world, we do not know exactly which world. Consequently, we do not always know what our demonstratives demonstrate (this same point is made by Kaplan in his paper). Stalnaker makes a specific proposal about how participants in a conversation deal with such sentences which, for reasons of space, we will not deal with here. However, he also extends his explanation to the sentences (14), which like those in (13) will either be necessarily true or necessarily false:

(14)   a.   Hesperus is identical with Phosphorus.
         b.   An ophthalmologist is an eye doctor.

This may well be the correct account, but it depends on there being a context-dependent aspect to the interpretation of proper names, and indeed of ophthalmologist and eye doctor. While this is not implausible,[6] it cannot be taken for granted, and a few words of amplification seem to be called for. As a last point, it should be noted that there is a series of potentially confusing misprints at the end of Stalnaker's article: in the second paragraph of p330, 'true' should be changed to 'false', and both occurrences of 'false' should be changed to 'true'.

As the length of our review indicates, we regard this as an important book: the editor and publishers deserve to be congratulated for assembling such a high quality collection of mutually relevant and independently interesting papers on central topics in pragmatics.

FOOTNOTES

1. Dahl (1979) provides a useful supplement to this section of Givon's paper.

2. Cf. Shibatani (1973:62-63) on bloom vs. grow.

3. See Horn (1978) for further discussion of McCawley's paper.

4. See Rogers (1978) for further discussion of Stalnaker's paper.

5. See Hamblin (1971) for a precursor of Stalnaker's theory.

6. Cf. Thomason (1976).

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