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Geoffrey J. Huck & John A. Goldsmith, *Ideology and linguistic theory: Noam Chomsky and the Deep Structure debates*. London & New York: Routledge, 1995. Pp. x + 186.

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The Deep Structure debates took place when many of today's graduate students were not yet born. Huck & Goldsmith (H&G)'s goal is to set the empirical record straight, erasing various misconceptions which they believe are found in most accounts of the dispute. To this end, they examine both the central empirical arguments and the rhetoric and argumentation of the dispute. They also have an interesting discussion of what happened to the proponents of Generative Semantics after the debates wound down. An appendix contains interviews with four central figures in the debate (Jackendoff, Lakoff, Ross and Postal), which are fascinating, and well worth the price of the book.

H&G choose four sets of empirical arguments and examine them in order to make points about the nature of the argumentation which they feel have not been adequately made elsewhere. The first and best known is Chomsky's (1970) claim in 'Remarks on nominalization' that derived nominals should not be derived transformationally from verbs. Chomsky's central claim was that derivational morphology should not be done with transformations, and a corollary was that transformations should not be allowed to change category. H&G show that the arguments against using transformations for derivational regularities were not utterly conclusive, but had problems of their own and simply set aside the question of how derivational regularities were to be captured. They go on to explain that the issue of category-changing transformations actually had little bearing on the central tenets of generative semantics: perhaps lexical items are inserted at some time after the deepest underlying level. Furthermore, they point out that it is possible to use syntax-like derivations to account for the regularities found in word formation and still be an interpretivist, as work by Lieber, Hale and Keyser, and M. Baker shows.

The second set of arguments involved Chomsky's advocacy of the traditional philosophical view that logical formulae must be quite distinct from natural language sentences. Because of this view, he insisted that semantic representations were quite unsuited as underlying syntactic structures. Furthermore, he was skeptical that the meaning of a linguistic expression could be given an exact representation, isolated from belief and knowledge about the world. McCawley was interested in expanding the mechanisms of logic in constrained ways, so that they would be able to represent meanings of linguistic expressions and then be derived into Surface Structures, but Chomsky was never satisfied with McCawley's attempts to demonstrate that such an enterprise could be possible. H&G show the extent

to which Chomsky and McCawley were at cross-purposes in their debate about this issue. They also point out elsewhere that work on the nature of the level of Logical Form from the late 1970's to the present has in some ways vindicated many of McCawley's claims (although not the central Generative Semanticist claim that semantic representations can be transformed into surface structures).

The third argument was that the Interpretive approach was better because these questions about the nature of semantic representations need not be settled or speculated about in order to proceed with syntactic analysis. Of course, as H&G point out clearly, this view did nothing to solve questions of the relationship between syntax and semantics, it simply set them aside, and it was based upon a rather strong hypothesis about the nature of semantics, namely that semantics does not come into play at all in syntactic analysis.

Finally, Chomsky argued that Generative Semantics, with its global rules and transderivational constraints, effectively allowed an arbitrary relation between form and meaning. I wish that this section had been more detailed. It seems to be assumed that readers know what global rules and transderivational constraints are, but I think most linguists who came of age after the early 1970's have heard the mantra that these devices are too unrestrictive but do not know enough about them to judge the seriousness of the charge. H&G point out that the unrestrictiveness in the Interpretivist accounts of the relevant data got shoved into the poorly-elaborated realm of semantics, so the Interpretivist syntax was more restrictive, but only because the data calling for relaxing the restrictions was effectively left unaccounted for. It would have been helpful to have enough information for the contemporary linguist to be able to compare global rules and transderivational constraints with current proposals for comparing derivations with respect to economy conditions.

In the third chapter, H&G try gently to take linguists to task for their sometimes inflammatory rhetoric, and show how the hyperbolic nature of some of the claims made this debate more acrimonious. This chapter reminded me of the sessions that my female colleagues and I would have in graduate school where we would only half jokingly take each other's papers and globally replace what we thought of as 'girl language' with what we characterized as 'boy language'. 'Therefore, it seems to me that' would become 'Thus I have shown', 'I will give some evidence that' would become 'I will prove that', 'supporting data' would become 'conclusive empirical evidence' and 'my suggestion' would become 'my theory'. I always wondered whether those (of whatever gender) who wrote that way really believed their inflated claims, or were just adopting a certain style. H&G claim that this style was much more prevalent among Interpretivists than among Generativists, which doesn't seem like news to me.

The empirical and rhetorical cases that H&G examine are meant as examples, and they do not rehash all of the arguments detailed in other

works such as Newmeyer (1986) and Harris (1993). They establish, I think successfully, that the Generative Semanticists took reasonable premises and drew from them interesting conclusions which were perhaps questioned but not disproved. They also show clearly that much of the dialogue on these issues was at such cross-purposes that it may be a stretch to call it a debate. These two considerations provide the support for their central point, which is that the demise of Generative Semantics cannot be attributed to fatal empirical flaws.

For me, this central point was a straw man. I never thought that Generative Semantics had been disproved. Rather, I thought it was made to look less promising than the Interpretive approach as a research strategy, for a combination of empirical and sociological reasons. H&G do give a number of quotes from standard sources (Newmeyer 1990, Jackendoff 1983, van Riemsdijk & Williams 1986) to verify the existence of what they call the 'standard view', but even one of these 'standard view' authors says elsewhere, 'It is tempting to think that it was the weight of interpretivist counterattack that led to the demise of generative semantics. While it played an important role, it was not the deciding factor' (Newmeyer 1986: 132). So it is not clear to me that the position H&G argue against is very widely held.

Lurking throughout this book is a confusion that seems also to have been present during the Generative-Interpretive battle: a distinction fails to be made between an interesting empirical result and a result that has the force to cause someone to abandon a system of beliefs in favor of another. In case after case, H&G assert that the arguments against Generative Semantics were not conclusive. But a claim does not need to be conclusively refuted in order to fail to be convincing enough. I do think that H&G understand this, since they quote Thomas Kuhn in a footnote,

But paradigm debates are not really about relative problem-solving ability, though for good reasons they are usually couched in those terms. Instead the issue is which paradigm should in the future guide research on problems many of which neither competitor can yet claim to resolve completely. A decision between alternate ways of practicing science is called for, and in the circumstances that decision must be based less on past achievement than on future promise. (Kuhn 1970: 157)

Although H&G place this footnote on material near the end of the book, Kuhn's point is precisely what kept occurring to me throughout. Generative Semanticists came up with some interesting evidence in favor of lexical decomposition and the interdependence of semantics and syntax, and they tried to show how semantic representations could be transformationally derived into surface structures. But they failed to develop their program in a way that convinced others of its future promise.

In fact, they failed to convince even themselves of its future promise. Newmeyer (1986: 133) maintains that '... the fact is that generative semantics

destroyed itself. Its internal dynamic led to a state of affairs in which it could no longer be taken seriously by anyone interested in the scientific study of human language'. H&G's account shows Newmeyer to be on the mark. Although they would probably argue that it ought to have been taken more seriously, they do show quite clearly that the Generative Semanticists themselves abandoned the enterprise, each for different reasons. The fourth chapter and the interviews reveal that by the mid 1970's, the main thing the Generative Semanticists had in common was a belief that things were wrong with the work of Chomsky and his followers.

The interviews give a vivid sense of how different the personalities of these four are, which makes it easy to see how conflicts could arise. They also show clearly how the Generative Semantics movement disassembled of its own accord in the mid 1970's. More than the body of the book, they give positive examples of how the participants' different disagreements with Chomsky's program resulted in the development of interesting research in alternative frameworks.

H&G try hard at the beginning to establish a common core of beliefs for Generative Semantics and also to extricate the core of beliefs that can be said to hold of Chomsky's program as it has developed from 1955 to the present, and to set this discussion up in a way that will highlight the commonalities. I was looking forward to a book which would illuminate those areas of Generative Semantics which could still hold true and be important, or which would show how congruences between Generative and Interpretive approaches might help to sort out questions that arise in current theories where D-Structure is eliminated, lexical decomposition is explored and semantic or pragmatic properties such as focus, definiteness and topichood seem increasingly to bear on syntactic analyses. I suppose I cannot fault H&G for failing to write the book that I hoped to read, but my hopes were for a new enlightening perspective on the issue, and I was disappointed. I think much of the problem may lie in the authors' great efforts to be evenhanded, which undermined their apparent desire to show that Generative Semanticists had been treated unfairly. The ostensive goal of this book is to show that Generative Semantics was not proven to be false, and as I've said above I find this to be a bit of a straw man. At this point twenty years after the fact, a useful book about the issues would need to address more closely which specific glib dismissals resulted in a loss for our current state of knowledge. This issue is addressed only in the interviews, where in particular Lakoff and Jackendoff discuss the direction their research wound up taking in terms that illuminate which aspects of the Generative Semantics challenge proved valuable to each of them. The subliminal goal of this book, as evidenced by the picture on the book jacket of Chomsky gazing over the red words 'Ideology and linguistic theory' is to decry the weapons used to win this war, presumably as a warning to those unaware that these weapons continue to be deployed. This goal is subliminal in the sense that the authors

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try not to be accusatory, but the evidence seems to be that the Interpretivists were much freer with the verbal nukes (although the Generative Semanticists were probably as free with the hyperbolic claims). H&G's assumption seems to be that rhetoric is really a factor that influences some people's paradigm choices, but I think this is an open question. Personally, I find arrogant rhetoric to be more distracting than persuasive, but I'd be interested to find out whether it has some unconscious effect on me. If rhetoric does not significantly influence paradigm choice, then it's not clear that an examination of it is useful. If it does have a significant influence, then I think H&G should have drawn stronger conclusions about how rhetoric might be used to enhance rather than discourage dialogue among linguists with diverse viewpoints.

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(Received 22 September 1997)

Johan Rooryck & Laurie Zaring, *Phrase structure and the lexicon*. Dordrecht: Kluwer, 1996. Pp. vii + 298.

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The task that Rooryck & Zaring undertake, namely to study the relation between the lexicon and phrase-structure, is both ambitious and well worth the trouble, particularly because it comes from a rich tradition (the 'linguistics wars' were fought over that turf) and the issues are still as alive as ever. In part to pursue this goal, the editors of the present volume co-organized a series of lectures, some of which made it into the book. This has the advantage of diversity, but also the disadvantage of an somewhat unfocused, uneven result.