Thirty Million Theories of Grammar

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THIRTY MILLION THEORIES OF GRAMMAR

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INTRODUCTION

'You can't order linguini with clam sauce.
If you want clam sauce, you gotta order spaghetti.'
(Unidentified waitress, San Diego, California, February 1970.)

This volume consists of four articles by me–two previously published in journals and two previously available only in working papers volumes–that are devoted to some significant extent to critiques of 'interpretive' and 'lexicalist' approaches to syntax and semantics, that is, of work by Chomsky since 'Remarks on nominalization' (1970) and by other linguists, especially Chomsky's students, which is more or less within the frameworks within which Chomsky has worked since the late 1960s.

I emphasize, however, that these articles are not concerned with a supposed single issue often spoken of as 'generative semantics versus interpretive semantics'. The terms 'generative semantics' and 'interpretive semantics' are names not of two contrasting positions on a single issue, nor of two poles on a continuum of positions, but of two packages of positions on a fairly large number of issues, each package corresponding to the views held (actually or in popular caricature) by representative members of two communities of linguists in about 1970 (George Lakoff, Haj Ross, Paul Postal, and I being representative members of the 'generative semantic' community, and Noam Chomsky, Ray Jackendoff, and Joseph Emonds being representative members of the 'interpretive semantic' community).

Neither of these communities was completely homogeneous, no member of either community retained exactly the same set of views for very long, the loci of the disputes between the two communities changed rapidly, often in mid-article, and the relationships among the views that at any moment were packaged together as 'generative semantics' or as 'interpretive semantics' were generally far more tenuous than representative members of either community led people (including themselves) to believe. One of my chief goals in writing the articles collected here has been to take apart the various packages and to demonstrate where possible the independence of the views that comprise the package.

The title of this volume is a conservative estimate of the number of viable combinations of answers to the questions that I take up here.[1] I arrived at the figure of thirty million by computing 2^{25} and rounding downwards: I deal here with easily forty issues, each of which admits at least two possible positions, and I doubt that weeding out the inconsistent, incoherent, or blatantly false combinations of positions would reduce the number of combinations by a factor of more than a few powers of 2. Beyond the probable understatement in the number, the title involves the inaccuracy of applying the term 'theory' to simply a set of positions on issues, when the term is most often used (though not so often defined) to refer not to just a set of propositions but to an ontology combined with a conception of what propositions are meaningful and what their relationship to possible facts is. A number of the issues discussed in the articles below provide distinctions among 'theories' in this narrower sense. In any case, whether what I provide below is indirectly a survey of thirty million theories or of only three hundred, the number of significantly different sets of positions that can be taken on the issues discussed here is considerably greater than the number of names that have been given to conceptions of how meaning and form are organized and are related to one another and to context. It is the issues and not the named sets of views that form the subject matter of this book.

According to the vulgarized version of Sir Karl Popper's 'Falsificationist' philosophy of science that is popular among linguists, each of those issues is in principle resolvable by matters of fact that will falsify one or other of the competing views.[2] Many of the issues are indeed widely believed to have been resolved in this way. The trouble with this conception of the resolution of scientific controversies is that there is no reliable way of telling what a falsification falsifies. False factual consequences are never deduced from just a theory but only from a large number of premises, many of them hidden,[3] of which some are parts of the theory that the investigator is testing, some are parts of other theories whose subject matter the given test impinges on (for example, if you test an astronomical theory by making predictions about what one will see through a telescope pointed at a particular place in the heavens, you must rely on theories of optics, of the medium through which the light is passing, and of visual perception), and others of which relate to the correctness of the assumed statement of the facts (for example, the proposition that your telescope really has the magnification that you think it does). A falsification demonstrates that at least one of the premises from which the false factual proposition has been deduced is false, but it gives no clue as to which one(s) the blame should be pinned on. Thus falsifications do not conclusively eliminate the theoretical propositions that they are designed to test: they only provide estimates of the price that one must pay in order to retain those propositions. The philosophies of science developed by Lakatos (1970) and Feyerabend (1975) acknowledge that any theoretical idea can be maintained at some price. Much of this book is devoted to what Feyerabend calls 'counter-induction': the search for ideas that can provide environments in which allegedly refuted ideas are viable (that is, in which the price of accepting them becomes affordable), in the way that Galileo's theory of motion provided an environment in which the proposition that the earth rotates on its axis was viable.

This book begins with a review article on Chomsky's *Studies on Semantics in Generative Grammar* (*SSGG*) which was written in 1973 and first published in *Studies in English Linguistics, 3*, 209-311 (1975). Chomsky's *SSGG* consists of his influential article 'Remarks on nominalization', in which he first advanced the 'X-bar' conception of syntactic categories and his 'lexicalist' account of nominalizations, and two articles devoted mainly to attacks on generative semantics as he conceived of it. In my review I point out that whether nominalizations must be entered in the lexicon of English is independent of whether a nominalization transformation is required, that the 'transformationalist' analysis that Chomsky attacks is a straw man, and that most of the facts which he takes as evidence against that analysis in fact provide support for an alternative 'transformationalist' analysis that is much closer to that offered by Lees 1960.[4] Since the papers in *SSGG* were written fairly early in the development of Chomsky's non-transformational approach to grammar, and since not even Chomsky, let alone I, could predict what significance some of the ideas he expressed or hinted at in *SSGG* would have in subsequent work, I have added to this review article a particularly generous supply of annotations (enclosed in square brackets to keep them distinct from the original notes) so as to relate my discussion of *SSGG* to many issues that have subsequently acquired importance.

The next article, 'How to get an Interpretive Theory of Anaphora to Work', appeared in *Linguistic Inquiry,* 7, 319–41 (1976) under the title 'Notes on Jackendoff's theory of anaphora'. I have restored the title under which I circulated a preliminary version of it, on the grounds that the earlier title is more informative: the article is concerned not just with evaluating Jackendoff's specific system of semantic interpretation rules but with determining what facets of his approach can be retained in an analysis that accounts for a number of phenomena for which his rules make false predictions. As in the discussion of anaphora in my review of *SSGG*, I make a point of keeping separate the issues of what underlies personal pronouns and other anaphoric devices, what determines the form they take, and what role coreference can play in the action of linguistic rules. The 'classical' transformational treatment of anaphora (for example, Ross 1967a), in which all anaphoric devices are derived from constituents identical to and coreferential with their antecedents and take their form from syntactic features of the underlying constituent, involves answers to all three of these questions. Arguments for 'interpretive' treatments of anaphora have often mistakenly taken facts relating to one of the questions as necessarily having a bearing on all three, perhaps because of a belief that propriety demands that one be either 'consistently interpretivist' or 'consistently transformationalist' and avoid any 'mixed position'. In my current view, 'mixed' positions are mixed only in relationship to packages of answers that, largely through historical accident, have taken on the status of landmarks.

The third article, 'Language Universals in Linguistic Argumentation' was my forum lecture at the 1978 Linguistic Institute, held at the University of Illinois at Champaign-Urbana; it received semi-publication in that university's working papers series (*Studies in the Linguistic Sciences*, *8*, no. 2, 205–19, 1979). It deals with the role that the notion 'language universal' has played in the argumentation of transformational grammarians, especially with arguments in which conclusions are justified on the basis of the claim that they allow one to maintain language universals that alternative analyses would conflict with. I find the bulk of such arguments worthless, since the putative universals generally are merely features accidentally shared by analyses that the investigator for some reason happens to like. The investigator's preferred type of analysis is always available at a price, and in advancing the putative universal he is only expressing his commitment to pay that price and to bully his fellow linguists into paying it too. However, there are also worthwhile arguments based on considerations of language universals, especially those in which an analysis is supported by verifying the pattern of interlinguistic variation that it predicts, on the assumption that linguistic rules purport to identify the possible loci of linguistic variation.

The final paper in the volume, 'The Nonexistence of Syntactic Categories', is an extensively revised and expanded version of a paper read at the Second Annual Michigan State Linguistic Metatheory Conference (May 6–7, 1977) and circulated in the volume of conference papers. In it, I dispute a large number of assumptions about syntactic categories, which generative semanticists and interpretive semanticists have shared, and show that it is only in virtue of these assumptions (especially the assumption that syntactic categories remain constant throughout derivations) that there is any conflict between the claims of generative semanticists that there are extremely few syntactic categories, and the claims of interpretive semanticists that there are a large number of categories. I develop an alternative approach in which the notion of syntactic category as such is rejected in favor of the recognition of a set of factors, some of which are not syntactic in nature, that can play roles in various kinds of syntactic phenomena. The resulting conception of syntax is shown to provide the basis for a picture of language acquisition that is far less mysterious than the picture generally assumed in transformational grammar.

While I will disagree below with many ideas that adherents of Chomsky's (revised) extended standard theory ((R)EST) hold dear, I emphasize that I am by no means hostile to all developments within (R)EST. For example, I regard the following as fairly well established:

1. Points relating to complementizers and COMP position:
   (a) complementizers are sisters of their clauses;
   (b) in WH-movement in English, items are moved into 'COMP position', that is, into a position that could otherwise be occupied by a complementizer.
2. Points relating to syntactic categories:
   (a) a fairly large number of syntactic category distinctions must be drawn;
   (b) the syntactic category of an item is a complex of components, of which one is the lexical category of the head of the item;
   (c) there is a category distinction between lexical items and phrasal constituents of which they are heads, for example, N vs. $\bar{N}$, A vs. $\bar{A}$.
3. Points relating to the cycle and cyclic domains:
   (a) NPs, as well as Ss and/or $\bar{S}$s, are cyclic domains;[5]
   (b) there is a principle of strict cyclicity.

My differences with (R)EST in these areas relate to specific issues that go beyond those listed above. For example, with regard to the first category listed above, I consider the case for successive-cyclic WH-movement to be extremely weak and the problems created by successive-cyclic WH-movement to outweigh its alleged benefits, and I reject the putative language universal (Bresnan 1970) that WH-movement always substitutes the moved item for a complementizer.[6] With regard to the second category, I have several reasons for rejecting the conception of 'base rules' with which X-bar syntax is usually combined, as well as its double and triple bars (for me, NP is not N plus bars and $\bar{N}$ and $\bar{V}$ can nest *ad libitum*, for example [$_{\bar{N}}$ [$_{\bar{N}}$ [$_{\bar{N}}$ *book on Copernicus*] *by Kuhn*] *that you recommended*] or [$_{\bar{V}}$ [$_{\bar{V}}$ [$_{\bar{V}}$ *work*] *hard*] *all day*]), and I reject many of the putative categories that have figured in X-bar syntax (for example, M, Aux, QP). With regard to the third category listed above, I lean towards the position that ALL constituents are cyclic domains,[7] I recognize as cyclic domains many constituents that do not exist in REST analyses (see, for example, the explanation of why passive *be* follows all other auxiliary verbs given in McCawley 1981a), and I accept a different version of strict cyclicity than that which Chomsky has generally assumed (for example, for me postcyclic transformations can apply to embedded clauses).

Except for 'The Nonexistence of Syntactic Categories', which I rewrote completely for this volume, and §2.2 of 'How to Get an Interpretive Theory of Anaphora to Work', which I have replaced by a newly written section that avoids a major error that I made in the original, the versions of my papers that are included here are lightly edited but heavily annotated. With those two exceptions, changes in the texts of the previously published papers are confined to stylistic improvements (including the omission of some superfluous notes and the addition of a couple of extra examples); however, I have added numerous new notes in which I make retractions and clarifications or comment on subsequent work. The new notes, as well as the rewritten section of Chapter 2, are enclosed in square brackets to make them easily distinguishable from the original material.

To keep the number of added notes from becoming astronomical, I have not included retractions and clarifications in all the places where they are appropriate, particularly in the earliest work in this volume, my review article on *Studies on semantics in generative grammar*. I will accordingly list here some points on which either I have changed my mind since the early 1970s or my thinking has become more consistent since then, in lieu of still more added notes that would be quite repetitive.

First, I now regard my earlier use of 'V' as a symbol for 'predicate' (as in (13) of §4.6.1 of Chapter 1) as extremely misleading and now restrict 'V' to the lexical category 'verb' (as opposed to noun, adjective, preposition, and perhaps some other things); I now use '0' for 'predicate' without determinate lexical category, as in (10) of §2 of Chapter 4.

Second, in many places I was much less concerned with justifying details of constituent structure than I now am and accordingly failed to bring in considerations that now lead me to set up such constituent structures as $[_{NP}$ *the* $[_{\bar{N}}$ $[_{\bar{N}}$ *discovery of Uranus*$]$ *by Herschel*$]]$.

Third, I have become much more consistent than I had been in taking linguistic structures not to be strings but topological objects such as trees and thus in taking the question of underlying constituent structure to have more substance and importance than the question of underlying constituent order. I accordingly reject notational schemes (such as the standard schemes for formulating transformations) in which the often totally irrelevant factors of constituent order and adjacency are made necessary parts of the formulations of all transformations; in fact I doubt that adjacency is ever relevant to syntactic phenomena other than those that are in part also morphological phenomena, for example, cliticization.[8]

Fourth, as is suggested in the third point, I now regard the case that I offered (1970c) for deep VSO word order in English as very weak because of my gratuitous assumption that there IS a deep constituent order, my reliance in some of the arguments on the notational system for transformations that even in 1970 I regarded as pernicious, and my failure to identify the role of grammatical relations in some of the phenomena that I discussed; I now consider the VSO order that appears in structures that I propose in Chapter 1 to be simply a makeshift way of indicating the grammatical relations between predicates and arguments (see in this connection note 11 to Chapter 4).

Fifth, another idea that I assume more consistently now than in some of my earlier work is that rules of grammar are derivational constraints rather than operations, that is I take the rules of a grammar to be conditions on what can occur in various stages of derivations and on how different stages of derivations may or must differ from each other. I accordingly reject the metaphor of the grammar as a sentence factory (or as a blueprint for an imaginary sentence factory) and am quite happy to consider possibilities that from the point of view of that metaphor are quite outlandish, for example, a conception of grammar in which there are 'phrase structure rules' specifying what are possible surface constituent structures but no rules specifying what are possible deep constituent structures. The arrow that figures in my formulations of rules for how consecutive derivational stages may differ serves only for the purpose of orientation, like the arrow on a map that points north, and it does not imply that what follows the arrow owes its existence to what precedes it. (The arrow on the map tells you that Detroit is north of Toledo, not that you have to go through Toledo to get to Detroit.)

Finally, much of what I say in this volume reflects my rejection of the notion of a language as being a set of sentences and the notion of 'grammaticality' as a property of sentences in and of themselves. Accordingly, I am interested in identifying factors that affect the interpretation and acceptability of sentences but have no interest at all in classifying those factors as grammatical or extra-grammatical. For me the question of whether an odd-sounding sentence is 'grammatical' or not is a question not about the language but about the linguist who asks the question, in the sense that his answer tells me something about his conception of linguistics but nothing about the language (cf. McCawley 1976a). I hang my head in shame at seeing how many times I have spoken of sentences as being 'grammatical' or 'ungrammatical' in the review of *SSGG*; in those passages, the reader should take 'ungrammatical' as simply an informal English equivalent for the asterisk, which I use to indicate that the sentence (with an intended interpretation that I hope will always be obvious) possesses the kind of anomaly that I happen to be talking about at that moment.

To the acknowledgements with which the individual papers in this volume are provided, I wish only to add expressions of appreciation to Ray Jackendoff, whose course at the 1980 Linguistic Institute at the University of New Mexico assisted me considerably in identifying and exploring quite a few issues that I take up in the newly added notes, and to Geoff Pullum, who gave me valuable suggestions for the improvement of this introduction, and to voice my gratitude to the many students at the University of Chicago who, through their incisive questions and comments in classes where I have discussed the topics with which I deal here, have helped me to achieve a much better understanding of those topics than I otherwise could have attained.

Notes

1. Since certain highly literate friends of mine have misinterpreted the title as a reproach to interpretive semanticists for failure to narrow down the set of possible theories sufficiently, I should emphasize that no such suggestion lurks behind my choice of a title. I am rather reproaching interpretive semanticists and generative semanticists alike for failing to recognize issues that ought to have been matters of controversy but so far have not been.
2. See Lakatos (1970: 180–4) for a clear account of the differences between Popper's actual views and those which are often mistakenly attributed to him.
3. See Musgrave (1976: 200–2) for the role of the hidden assumption that water is not a combustion product in what for several years was widely held to be a conclusive refutation of oxygen chemistry and confirmation of phlogiston chemistry. Hidden assumptions, as in this case, are often false propositions which are so obviously true that no one bothers to mention them.

4. See, however, note 17 to Chapter 1, where I argue that the surface constituent structure of nominalizations conflicts with the implications both of Chomsky's non-transformational analysis and of the 'updated Lees' transformational analysis.

5. '$\bar{S}$' here is to be understood *de re* rather than *de dicto*: I regard the constituents that are labeled $\bar{S}$ by X-bar syntacticians as cyclic domains, though I am neutral with regard to whether a category distinction between S and $\bar{S}$ need be drawn.

6. Epée (1976) points out that in Duala-dependent questions, a WH-moved expression is put after rather than in place of the complementizer (which in some cases is a morpheme that introduces yes–no questions and in others is the Duala analog of the *that* complementizer) and Wachowicz (1974) observes that in Polish two or more interrogative expressions can occur in a single 'COMP position'. Since in Polish as in English, constituents of dependent questions cannot be relativized or questioned, this shows that Chomsky's (1973: 244–7) putative explanation of the English fact must be rejected: if the reason why such expressions as *the book which Al asked who wrote* are impossible is that successive-cyclic WH-movement would allow them to be derived only via an intermediate stage involving a doubly-filled COMP position, then languages such as Polish that allow multiple WH expressions in COMP position should allow relativization out of dependent questions. Rudin (1981) gives a similar argument based on Bulgarian facts.

7. This position is argued for in Williams (1974) and is in effect assumed in Montague grammar, as I argue in McCawley (1977b). See Pullum (1976: 97–100) for criticism of Williams's proposal; I regard Pullum's objections as posing a more serious problem for Williams's claim that only 'root' transformations can be postcyclic than for his proposal that all constituents are cyclic domains.

8. On this point, see also Pullum (1980b).

1 REVIEW ARTICLE ON NOAM A. CHOMSKY, *STUDIES ON SEMANTICS IN GENERATIVE GRAMMAR* *

1

This volume, henceforth *SSGG*, which reprints three papers[1] written by Chomsky between 1967 and 1970, is concerned with developments in transformational grammar since the appearance of *Aspects of the theory of syntax* in 1965. It takes up a large number of issues on which Chomsky's position has either changed or become more specific since then and contains much criticism of other lines of development in transformational grammar, especially that one which has become known as generative semantics.

A reader of *SSGG* who has read nothing later than *Aspects* may be amazed at the extent to which Chomsky's ideas have changed, but I think it is inevitable that any serious proponent of the *Aspects* theory would rapidly come to give up one or other of the major tenets of that theory. While *Aspects* accepted a distinction between syntactic rules and semantic interpretation rules (henceforth SIRs), it also accepted tenets that made it hard to maintain such a distinction: that only the deepest stage of syntactic derivations was relevant to meaning, and that 'syntax' was to be interpreted so broadly that, for example, selectional restrictions were matters of syntax rather than (or perhaps, in addition to) semantics. In the years immediately following the emergence of the *Aspects* theory, deep structures rapidly got deeper (and closer to what could be taken as constituting semantic structure), until a point was reached where it was reasonable to question the assumption that syntax and semantics are distinct. 'Generative semanticists' such as Postal, Lakoff, Ross, and myself found the syntax/semantics dichotomy the most dispensable of our premises and proceeded to reject it. 'Interpretive semanticists' such as Chomsky and Jackendoff, on the other hand, clung to the distinction between syntax and semantics and rejected some of the premises of the arguments that led to deep structures that approximated semantic structures, notably the assumptions that deep structure determines meaning and that selectional restrictions are a matter of syntax.

Since this dilemma became apparent (about 1967) and linguists began to choose horns, further areas of divergence have arisen. For example, while there was general agreement about the notion of 'grammaticality' in 1967, generative semanticists have come to dispute the notion that one can speak coherently of a string of words (or even a surface phrase-marker) as being grammatical or ungrammatical or having a degree of grammaticality and now hold that a surface structure can be 'grammatical' only relative to the meaning that it is supposed to convey and the (linguistic and extra-linguistic) context in which it is used. Thus, strictly speaking, generative semanticists are not engaged in 'generative grammar'. Chomsky, on the other hand, has greatly expanded the range of sentences which he would call 'grammatical' but semantically unacceptable and thus, while maintaining a notion of grammaticality of sentences, applies it very differently than he did in 1965. The fact that the differences between these two lines of development have been increasing as time passes makes it difficult to be fair in reviewing anything even a couple of years old in which an interpretive semanticist criticizes generative semantics or vice versa: changes in the assumptions on both sides have been rapid, often not explicitly acknowledged, sometimes perhaps unconscious, which renders it impossible to be very sure what X assumed in his 1968 criticism of Y's 1967 paper or what it would have been reasonable for X to assume in 1968 that Y had assumed in 1967. I will not try very hard to be fair, since (for the reason just mentioned) fairness would require going into tiresome detail about ephemeral and insignificant points of history. I will concentrate rather on making clear the issues touched on in this volume or raised by it which, on the basis of all the hindsight now available to me, seem the most important and which are most germane to current controversies. However, I have cast the review in the form of a fairly detailed commentary since I think that it will thereby serve best the interests of readers who wish to give *SSGG* a careful and intensive reading such as it deserves.

2

2.1.

The first paper, 'Remarks on nominalization' (pp. 11-61, henceforth 'Nominalization'), is devoted to arguments that nominalizations[2] do not involve an embedded S but rather have deep structures that differ in only minor ways from their surface structures. Chomsky's proposals cover only action and property nominalizations (*their refusal of my offer*; *John's honesty*); he mentions other kinds of nominalizations such as agent and object nominalizations (*the discoverer of radium*; *Dostoevskii's writings*) only in the process of criticizing proposed