INTRODUCTION

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1 The papers collected in this volume have three things in common: that they originate within the transformational-generative tradition[1] of syntactic and semantic analysis, that they have inherent and/or historical importance, and that they have hitherto been part of an underground literature: the many papers that have been circulated in office-duplicator or in project reports but have not been given normal publication in journals or books. (One of the papers has appeared in Russian translation and another in German translation in normal publications, but their English originals have remained underground until now.)

Most of the papers have been widely cited in the above-ground literature, and I make some attempt to suggest the impact that they have had on above-ground controversies in the introductory notes that I have supplied to each paper. The form in which each paper appears is substantially that in which the paper was originally circulated, except that I have allowed myself and the respective authors to make minor stylistic and terminological improvements, to replace ill-chosen examples by examples that make the same point better, to delete or summarize sections that are of minor interest, and to add clearly marked annotations in which the authors

may make retractions or discuss subsequent related work (original footnotes are keyed by numbers, added annotations by letters). In addition, the bibliographies of the individual papers have been collated into a single list of references, and citations in the papers have been keyed to that list (since unpublished papers often make reference to papers that at the time of writing were also unpublished but later were published, the reader should not be perplexed at finding references to "Ross, 1969a" in a paper written in 1967; 1969 is the date Ross's paper was published, and "Ross, 1969a" replaces the author's citation of an at that time unpublished paper). The papers in this volume are arranged in the order in which they were written.

2 The reader of this volume probably has two questions about it: why were the papers in it not published in the 1960s and early 1970s, when they were written, and why should anyone want to read them now?

No uniform answer to the first question can be given. In most instances, the failure of the paper to appear above-ground until now can be attributed to sloth on the part of the author; most of the authors could probably have found the time to rewrite and retype their papers and submit them to a journal, but they simply never got around to it. Or, by the time they did get around to seriously thinking of revising their papers for publication, their ideas had changed so considerably that they preferred to write entirely new papers, rather than revise their earlier efforts (which is not to imply that the authors then set about writing new treatments of the same themes--they often exhibited no less sloth with regard to their new intentions than with regard to the old ones). A contributing factor in the failure of some of the authors to publish their papers is probably the in-group mentality that was all too common among transformational grammarians in the 1960s. My impression is that most of the transformational linguists who were at M.I.T. or Harvard in the 1960s (which is where most of the authors represented in this volume spent part of that decade) felt that anyone capable of appreciating their work probably read the M.I.T. Quarterly Progress Report and the Harvard Computation Laboratory reports anyway, so what need was there to publish anywhere else? One of the papers in this volume, George Lakoff's "Pronouns and Reference", is an unfinished work that would have been of book length had it ever been finished. Other papers were intentionally written in a form inappropriate for normal publication, particularly Postal's "Linguistic Anarchy Notes" and the series of informal notes that it inspired

(Kuroda's "Linguistic Harmony Notes", and Morgan's "Cryptic Notes" and "WAGS"); Benwick, Fay, and Knight's "Camelot" was prepared by three students attending the 1968 Linguistic Institute as a summary of lectures given in courses there by Ross, George Lakoff, and myself.

3 There are a number of important linguistic issues that receive a more thorough and incisive treatment in papers included here than in anything hitherto published above ground. One striking example of this is George Lakoff's "Pronouns and Reference" (paper 16), which demonstrates that neither the once popular view that all pronouns are derived from copies of their antecedents, nor the now fairly popular view that all pronouns are present as such in base structures is a tenable position. He shows that while all anaphoric devices are subject to the constraint (Ross, 1967b, Langacker, 1969) that the anaphoric device may not both precede and command its antecedent (or rather, to a corrected version of that constraint, which Lakoff develops in his paper), there are both anaphoric devices that must be derived from copies of the antecedent and anaphoric devices that cannot be derived from copies of the antecedent, and hence the Ross-Langacker constraint must be an overall constraint on anaphora-antecedent pairs, rather than part of either a pronominalization transformation or a rule for interpreting base-generated anaphoric devices. Anderson's paper on the notion "base component" (number 7) argues that the conception of "base component" presented in Chomsky's "Aspects" conflates two matters that must be kept separate: the assignment of syntactic constituents to syntactic categories (which, Anderson argues, is neither language particular nor restricted to the level of deep structure) and the specification of what constituent order and what combinations of elements the language allows in deep structure. Anderson's approach has far-reaching possibilities that have never been explored in the literature. For example, it allows syntactic categories that do not figure in the deep structures of a language to appear in surface structure: if transformations create a combination of elements that does not appear in deep structure (say, if a transformation introduces prepositions, adjoining them to certain NPs), the universal category definitions could yield derived structures in which a category appears that did not appear at deeper stages of derivations (say, the category "prepositional phrase", if "prepositional phrase" is defined universally as consisting of preposition and NP). Anderson's paper is a welcome antidote to the prevalent prejudice against transformations that 'build structure' (see,

e.g., Akmajian and Heny, 1975, pp. 148-149 for a recent illustration of this attitude). 'Structure building' is outlandish only if one takes the base component of the grammar to be the source of all instances of syntactic categories.

4 Rather than continuing to tabulate the original and important ideas presented in these papers, a task that is better done in the introductory notes to the individual papers, to the extent that it needs to be done at all, I would do better to devote the bulk of this introduction to the matter of the historical significance of the papers, a subject not easily covered there, since it calls for discussion of the historical setting in which the papers were written.

Only the first four papers in this volume antedate the publication of Chomsky's "Aspects of the Theory of Syntax", and only the first three antedate the writing of "Aspects" and the oral dissemination of its conclusions. The underground transformational literature prior to "Aspects" is in fact rather sparse, even sparser than the above-ground literature. This state of affairs results from the fact that not only was the community of transformational grammarians quite small until well into the 1960s, but the rate at which ideas were born and died was also rather low. Three not unrelated changes took place fairly rapidly around 1964-1965: (i) the number of transformational grammarians increased from a miniscule figure to a quite sizeable one, (ii) transformational grammar turned from an avant garde movement in American linguistics into a clear contender for the title of "the establishment", and (iii) the character of writings by transformational grammarians changed to more research oriented from largely polemical, i.e., more concerned with identifying defects in the then dominant descriptivist or post-Bloomfieldian approach to linguistics and demonstrating the superiority of a transformational approach[2] than with developing and refining transformational grammar, which remained quite programmatic. The first two papers in this volume, both by Robert B. Lees, are high-quality examples of the early transformational polemic tradition, a genre that first achieved prominence with the publication of Lees' influential review (1957) of Chomsky's "Syntactic Structures". Such examples of that genre as Postal (1964) contrast strikingly with Postal's "Linguistic Anarchy Notes" (item 11 in this volume), where Postal's principal concern was to exhibit factual domains in syntax and semantics into which the then existing varieties of transformational grammar provided no insight.

The first two of these changes amount to a scientific revolution, and the third change signals the beginning of a

period of normal science, in the sense of Kuhn (1962). In speaking of a scientific revolution here, I have ignored entirely the question of whether transformational grammar was in fact an advance over the descriptivist approach. For the reason mentioned in footnote 2, it is not easy to give a nontrivial answer to the question (as opposed to the trivial affirmative answer that assumes the 1965 transformational grammarian's goals and priorities or the equally trivial negative answer that assumes the 1950s descriptivist's goals and priorities). I do not hold that scientific revolutions are always for the better; neither, apparently, does Chomsky, in view of his higher regard for seventeenth than for nineteenth century linguistics. As I have remarked elsewhere (McCawley, 1976a), "Scientific communities get the scientific revolutions that they deserve"; scientific communities in which bizarre and trivial concerns prevail are those that are most vulnerable to revolutions in which a worthless approach gains wide acceptance.[3] My feeling is that this time the American linguistic community was lucky and got a better revolution than it deserved, though by no means an ideal one.

However, the question of how good a thing per se it was for that particular revolution to take place is not of direct relevance to this historical sketch. Of much more relevance is the question of what climate the revolution created, given its timing in relation to other things going on in the field. The publication of "Aspects of the Theory of Syntax" took place during the period when the above three changes were taking place. Accordingly, the claims and the model of analysis of "Aspects" were the hottest thing in transformational grammar at the time when large numbers of persons started doing straight research within a transformational framework. It is thus not surprising that when the rate at which transformational literature was produced increased enormously in the mid 1960s, most of this literature took the "Aspects" theory as a frame of reference, whether in applying that theory to new factual areas or in proposing revisions in it. "Aspects" thus is at least visible in the background in most of the papers collected in this volume, many of which are devoted to critical examination of its tenets (or of the claims about the structure of English that have figured in the "Aspects" tradition of syntactic analysis--the tradition of analysis must be distinguished from the theory itself, since the theory allows for alternative analyses that are wildly at variance with the analytical tradition associated with "Aspects") and the formulation of alternatives. My impression is that the amount of work done in and on the "Aspects" theory in the mid and late 1960s was

far greater than one would expect, even granting the great increase in the number of transformational grammarians and the fact that transformational grammarians no longer had any reason for expending on antidescriptivist polemics energies that could be spent better on putting their theoretical house in order. Of course, an n-fold increase in the number of scholars in a field will often result in far more than an *n*-fold increase in the amount of research that goes on; a critical mass must be achieved before a scientific community becomes intellectually lively. And on the other hand, unless work is going on in several geographically separate centers, the idea pool (which is to a scientific community as a gene pool is to a biological species) is unlikely to become diverse enough for the implications and possibilities of the prevalent ideas of the community to be explored thoroughly. Of course, the increase in numbers of transformational grammarians in the mid 1960s dispersed the transformational linguistic community over the United States (and soon, much of the world), whereas it had previously been for all practical purposes confined to Building 20 at M.I.T.

But even so, the creative outburst that took place within the "Aspects" theory is astonishing. I see two reasons for this orgy of activity, besides those already touched on. First, the development of the "Aspects" theory [via the intermediate stage of Katz and Postal's (1964) theory] from the "Syntactic Structures" theory had involved a fair amount of identification and re-evaluation of premises (albeit nowhere near the amount that could have been carried out), and in the transformational community there thus prevailed an attitude that was relatively favorable to the critical examination of the foundations of transformational linguistic theory, which meant that the class of questions that transformational grammarians felt happy about asking was much greater than it had been a few years earlier. Second, a number of characteristics of the "Aspects" theory made it a far more stimulating framework, and far more attractive to persons outside of or only marginally within linguistics, than the "Syntactic Structures" theory had been.

One such characteristic is the obvious one, that "Aspects" brought semantics out of the closet. Here was finally a theory of grammar that not only incorporated semantics (albeit very programatically) but indeed claimed that semantics was systematically related to syntax and made the construction of syntactic analyses a matter of much more than just accounting for the distributions of morphemes.

A second appealing characteristic of the "Aspects" theory was its much greater systematicity, in comparison with

both the "Syntactic Structures" theory and all nontransformational forerunners with which I am familiar. To my mind, the principal appeal of transformational grammar is that it allows the linguist to cope with syntactic complexity. By contrast, complex sentences threw the theory of Harris (1946) for a loop. Compare the elaborate formulas that Harris (1946; pp. 173-174) gave for a couple of special instances of English relative clauses (and was in principle unable to convert into a fully general account) with the general rule that is available under any variety of transformational grammar, by which a relative pronoun (more generally, a 'relative expression' such as *for whose mother* or *under the influence of whom*) 'originates' in the position of a corresponding nonrelative expression and is moved to the front of the relative clause by a transformation. Note the way that a transformational treatment allows one to separate out the various factors that affect what relative clauses are possible: conditions on what is possible in nonrelative clauses, the process of moving the relative expression to the front of the relative clause, and the option of omitting the relative pronoun. However, the "Aspects" theory gives one more assistance in coping with complexity than the "Syntactic Structures" theory did. Whereas the "Syntactic Structures" theory did not make clear how the various rules involved in constructing a complex sentence could interact with each other, the "Aspects" theory provided a simple and elegant picture of how the rules interacted; each sentence had a deep structure in which sentences could appear within sentences in a way specified by the base rules, and the interaction of the rules was governed by the form of the deep structure (rules applied to lower Ss before any rules applied to higher Ss) and by a fixed ordering of the transformations, which governed the sequence in which they could apply to any given S of the deep structure.[4]

Third, the "Aspects" theory allowed the notion of syntactic category to be separated from various factors that affect what co-occurs with what (particularly selectional restrictions and notions of strict subcategorization such as the notion of transitive) and thus made it possible to do transformational syntax with fidelity to the facts, without having to set up an inventory of morpheme classes of the type that had figured in Harris' work and was implicit in early transformational grammar.

The first characteristic increased the inherent interest of doing transformational syntax, as well as making it relatively easy to come up with analyses that stood a chance of being right; characteristic two made it relatively easy to

determine what the grosser implications of a given analysis were; and characteristic three made it relatively easy to formulate transformational analyses in extremely general terms without any loss of precision, and to start dealing seriously with syntactic universals.

The "Aspects" theory accordingly was tested more thoroughly and more critically than any of its predecessors (transformational or otherwise), and much of the literature, both above-ground and underground, in the latter half of the 1960s got deeply enough into the implications of the "Aspects" theory to identify claims and assumptions of the theory that could be taken issue with or to find gaps in the theory and proposed corresponding revisions, both major and minor, in the theory. In some cases, what started out as a minor revision in the "Aspects" theory had far-reaching and at the outset unexpected implications. For example, George Lakoff's 1965 dissertation (published as G. Lakoff, 1970a) started out as an attempt to make precise the notion of exception that had been used informally by transformational grammarians; however, Lakoff was then able to show that the theoretical machinery that was needed to specify how rules apply in the presence of "exceptional" lexical items allowed a drastic reduction in the set of syntactic categories [e.g., it allowed "verb" and "predicate adjective" to be taken as belonging to the same syntactic category, a line of argument that led naturally to the proposal of Bach (1968) that there are only two 'elementary' types of syntactic units: predicates and referential indices] and in the process allowed deep structures to resemble semantic structures to a much greater degree than the deep structures presented in "Aspects" did.[5]

A number of papers in this volume criticize specific details of the "Aspects" theory. Collectively, they help remedy an anomaly in the literature, namely, the remarkable lack of reviews of "Aspects".[6] Anderson's paper on the base component further develops the proposal of McCawley (1968a) (originally worked out in 1965-1966), that the base component of a transformational grammar should be regarded as consisting not of rewriting rules but of node admissibility conditions, which specify directly what tree configurations are possible in deep structures, rather than requiring the deep structure tree to be constructed from a derivation that consists of strings. Chomsky is, in fact, unusual among transformational grammarians for the extent to which he prefers to talk about strings rather than about trees. It is generally trees rather than strings that are of relevance to a transformational grammarians concerns, and Anderson's paper provides an excellent example of how formulating rules in terms of trees

rather than of strings provides a new perspective and reduces to an appropriate level of triviality certain problems (such as that of tree pruning), which otherwise call for arbitrary and unsatisfying accretions to grammatical theory.

Several of the papers are concerned with an issue that has been a major scene of controversy since 1967, namely, the division between syntax and semantics. At the time "Aspects" was published, the question of identifying deep structure with semantic structure was not raised, since the syntactic analyses that had been proposed up to that time involved underlying structures that, while they often reflected semantic structure more than did surface structure, still were a long way removed from semantic structure.[7] Moreover, the version of semantic structure that was best known to transformational grammarians at that time, that of Katz and Fodor (1963), involved semantic structures that were at least typographically quite remote from what were then being proposed as underlying syntactic structures and thus helped perpetuate the belief that syntactic structure and semantic structure were of radically different nature. It should be emphasized, though, that Katz and Fodor failed to raise seriously the question of the formal structure of semantic representations, and the typographical format that they chose allowed for far less structural complexity in semantic structures than was actually necessary, as Katz himself (e.g., Katz, 1966) soon recognized. A large body of syntactic analyses soon developed that in the various details that were at issue agreed with the presumably logical structures:

(i) Quantifiers were argued to be predicates of higher clauses (G. Lakoff, 1965, further developed in G. Lakoff, 1970c, and McCawley, 1972a) with the scope of the quantifier being the constituent it was predicated of.

(ii) Causative verbs such as transitive *break* and inchoative verbs such as intransitive *break* were argued to derive from structures containing 'abstract' predicates CAUSE and BECOME respectively, with a complement sentence that contained the corresponding stative adjective; e.g., *Floyd broke the glass* would have a deep structure constituent *the glass (be) broken* (G. Lakoff, 1966; R. Lakoff, 1968).

(iii) Negation was analyzed as an intransitive predicate with a sentential subject (McCawley, 1968c; G. Lakoff, 1970d).

(iv) Auxiliary verbs were argued to be deep-structure main verbs having the clause to which they applied as deep structure complement (Ross, 1969b, further developed in

McCawley, 1971a).

(v) The deep structure distinction among declarative, imperative, and interrogative sentence was argued to reside in a higher clause (such as *I order you to S*) that specified the speech act performed by uttering the sentence (Ross, 1970; McCawley, 1968b).[8]

(vi) Various adverbial expressions were analyzed as involving higher predicates, with the surface main clause as complement of that predicate (G. Lakoff, 1965), even in cases such as that of instrument adverbs (G. Lakoff, 1968a) where the higher predicate (*use*) did not appear as such in the surface form of the sentence (i.e., the underlying *use* was realized as the preposition *with*).

Moreover, the sorts of arguments that had been offered in justification of analyses that diverged less radically from surface structure could generally be found to support these analyses, particularly arguments based on shared selectional restrictions in the structures that were held to be related and on missed generalizations: respects in which the proposed analyses allowed uniform rules to cover cases that would have to be listed separately in alternative analyses. Of course, many of these analyses have been contested, in some cases on quite reasonable grounds; however, the existence of this large body of analyses and of the quite sizeable mass of arguments in their support made it much more reasonable to raise in 1967-1968 [9] a question that would have been outlandish in 1965, namely the question of whether anything was to be gained by distinguishing between syntax and semantics, as those were understood in the "Aspects" theory, i.e., whether transformations did not simply amount to rules for specifying how semantic structures are related to surface structures, and base rules did not simply amount to rules specifying what semantic structures were possible.

The paper that first raised these questions seriously is Lakoff and Ross's "Is Deep Structure Necessary?" (paper number 9 of this volume), which can be said to mark the beginning of the variety of transformational grammar known as generative semantics. What characterizes generative semantics is not so much the well-known claim that there is no distinction between transformations and semantic interpretation rules as the abandonment of the assumption that there is such a distinction. For a generative semanticist, a grammar is a specification of what the relationship between semantic structures and surface structures is, and the proposition that a distinction between syntactic rules and semantic rules must be drawn is a claim requiring justification

on the basis of facts about semantic structure and its relation to surface structure. Accordingly, for generative semanticists, semantic data play fully as important a role as do syntactic data, if a distinction between semantic data and syntactic data can even be drawn. The question of whether an analysis can be justified on the basis of syntactic data was an important question to an adherent of the "Syntactic Structures" theory and remains an important question to an adherent of the extended standard theory, which figures in Chomsky's recent works, whereas to a generative semanticist the question is either of minor interest or is rejected outright as meaningless (on the grounds that all linguistic data are strictly speaking "semantic").

In accordance with this change of attitude, a large proportion of the generative semantic literature has been concerned with investigating semantic structure (e.g., McCawley, 1970b, 1973b; Morgan, 1969b, 1973) and with determining what the rules relating semantic structure to surface structure must be, given semantic structures that are taken to have been justified (e.g., McCawley, 1968c, 1971b; Levi, 1975). Such concerns are illustrated in this volume by Morgan's notes on *again* (the first part of item 17) and on *know* and *forget* (the second part of item 17), Larkin's study of *will* and *must* (paper 20), and Neubauer's note on idiolectal differences in the meaning of *pretend* (paper 21), as well as in the more extended studies of pronouns and reference by Lakoff (paper 16) and by Karttunen (number 19). In addition, there is an extensive literature, represented in this volume by Morgan's note on *again,* that argues that the semantic constituents of a semantically complex lexical item can stand in syntactic relations to overtly occurring elements and hence that syntax cannot be segregated from semantic analysis of lexical items (e.g., G. Lakoff, 1969, 1971; McCawley, 1968c, 1971b).

The matter of pronouns and reference deserves some comment here. The earliest transformational treatment of pronouns (Lees and Klima, 1963) ignored matters of reference entirely and derived all personal and reflexive pronouns (except those personal pronouns that have no antecedent) from copies of their antecedents via transformations that optionally pronominalized a NP that was identical to some other NP and did not express formally in their rules the fact that when that option is exercised, the two NPs are interpreted as coreferential, and when the option is not exercised, they are generally interpreted as non-coreferential.[10] In "Aspects", Chomsky modified the Lees-Klima analysis so as to make pronominalization and reflexivization contingent on coreferentiality. He proposed

that every noun in deep structure be supplied with a "referential index", that identity of referential indices correspond to identity of purported reference, and that identity conditions in the formulations of at least some transformations[11] be interpreted as demanding identity not only of constituent structure and of lexical items, but also of referential indices. However, Chomsky's proposal appears only in one short paragraph ("Aspects", pp. 145-146) and none of the deep structures that are presented explicitly in "Aspects" gives any indication of referential indices, which suggests that Chomsky was admitting referential indices to his syntactic theory only grudgingly and only because they were forced on him by his adherence at that time to the proposition that deep structure completely determined meaning.[12]

In 1966, Ross, Langacker, G.H. Matthews, and Maurice Gross independently discovered an important constraint on the relationship of pronouns to their antecedents: The pronoun may precede the antecedent only if it does not command the antecedent (e.g., *Before* he_i *went to bed,* $John_i$ *prayed to Zoroaster* is possible, but not **He*$_i$ *prayed to Zoroaster before* $John_i$ *went to bed;* see Ross, 1967b; Langacker, 1969). Ross formulated this constraint as a condition on a pronominalization transformation and argued that that transformation must be in the cycle because of the way that it interacts with the cyclic transformation of Equi-NP-Deletion in such sentences as *The realization that* he_i *was unpopular disturbed* $John_i$ (versus **The realization that* $John_i$ *was unpopular disturbed* him_i). A dilemma soon arose with the discovery by William A. Woods of sentences such as *The pilot*$_i$ *that shot at* it_j *hit the MIG*$_j$ *that chased* him_i, in which there are two pronouns, each contained in the antecedent of the other. As Emmon Bach and Stanley Peters noted, the pronouns in such sentences can be derived from copies of their antecedents only if the sentences have deep structures that are infinitely deep. Thus, one must either give up deriving all pronouns from copies of their antecedents (but how then is one to account for the facts discussed by Ross, which appear to show pronominalization behaving like a cyclic transformation?) or allow infinite underlying structures. Of the enormous literature that rapidly developed and continues to proliferate on Bach-Peters sentences as they have come to be called, the work that to my mind comes the closest to successfully resolving this dilemma is George Lakoff's "Pronouns and Reference"

(paper 16 of this volume). Lakoff's paper is one of a number of works that accorded referential indices a more central role than they had in Chomsky's proposal.[13] In Lakoff's underlying structures, a referential index occupies a NP position, and the additional material that appears in that position in surface structure (quantifier, noun, and/or relative clause) is outside of the clause in question. However, Lakoff rejected the previously universal assumption that either all kinds of anaphoric devices are derived from copies of their antecedents or none are. He argued that some kinds of anaphoric devices must be derived from copies of their antecedents (for example, sentence-pronouns and anaphoric one) and other kinds cannot be, and he maintained that only the latter kind of anaphoric devices can give rise to Bach-Peters sentences. Since both kinds of anaphoric devices are subject to the Ross-Langacker constraint, that constraint can be neither part of the pronominalization transformation(s) nor [as in the way out of the Bach-Peters dilemma that was proposed in Jackendoff (1969, 1972)] part of a semantic interpretation rule for assignment of antecedents to anaphoric devices, but must be an overall constraint on pronoun-antecedent relations that is independent of whether a full NP or merely an index underlies the pronoun. Lakoff was not entirely successful in accounting for the facts presented by Ross, but he was able to adduce facts that cast serious doubt on Ross's explanation of them and supported an analysis in which the Ross-Langacker constraint applied to surface structures or to shallow structures, rather than to the immediate outputs of pronominalization transformations.[14]

Output constraints such as figure in Lakoff's analysis of pronouns illustrate a major respect in which much of the transformational grammar of the late 1960s and beyond differs from that of the middle 1960s and before. In transformational grammars up to about 1967, the possible surface structures were precisely those structures that the transformations could convert possible deep structures into (or sets of possible deep structures, in the earlier versions of transformational grammar in which a transformation could have two or more inputs). The form (though not the content) of a transformational grammar was that of a sentence factory: to construct a sentence of language X, put pieces together as follows, then perform the following operations on the result. "Aspects" deviated from this general form of a grammar only to the extent that it allowed for derivations that hung up. For certain kinds of embedding (such as relative clause structures), some transformation had to

apply (such as the one forming a relative pronoun from a NP that matched the head NP), and a derivation in which such a transformation could not apply did not result in a well-formed surface structure. Ross (1967a) and Perlmutter (1968) demonstrated the existence of *output constraints*: restrictions on what is admissible as a surface structure that are not merely consequences of the base rules and transformations but have the status of independent rules of grammar. A particularly clear instance of an output constraint is discussed in detail in the paper by Szamosi with which this volume concludes. The acceptance of the notion of output constraint was a major step in the evolution of what shortly became the standard generative semanticist conception of a grammar (G. Lakoff, 1970b): A system of *derivational constraints*, i.e., rules that specify either what may occur at some stage or other of derivations or how various stages in a derivation may or must differ from each other, with the possibility explicitly recognized that some rules may be *global*, i.e., may involve nonconsecutive stages of a derivation. For what it is worth, I will state here my feeling that this conception of a grammar is better suited to the construction of realistic theories of how human beings produce and comprehend language. Both production and comprehension operate on several levels simultaneously; e.g., while you are already uttering the first words of a sentence, you are still constructing the meaning that the sentence is to express. Both comprehension and production involve the construction of a surface structure and a semantic structure that fit together according to the rules of the language. There is no reason why constraints on surface structure and constraints on semantic structure (which is one way that base rules might be thought of) should not play an equal role in language use; both would be involved in determining what comes next, both in the process of production and in that of comprehension. Moreover, there is no particular reason why global rules should not be involved in the process of constructing a surface structure and a semantic structure that match. The whole process is global anyway, so why shouldn't some of its components be too?

5 I will terminate this historical sketch of transformational grammar here, since to bring it more up to date would take it beyond the period in which the papers of this volume were written and for which it was supposed to provide the setting. I must emphasize that this sketch in no way pretends to be complete. In particular, it shares one major

imbalance with the rest of this volume, namely that it has fairly extensive coverage of the generative semantic branch of the transformational tradition but largely ignores the interpretive semantic branch. The reason for that gap is found in the somewhat haphazard way in which I chose the contents of this volume. My choices were made from among the underground works in syntax and semantics I already knew and liked, a body of works that is so large that it was easy for me to assemble a volume's worth of high-quality material without having to look very far. Indeed, if the underground literature were not so rich, the book would probably have been more balanced with regard to representing different varieties of transformational grammar, since I would have had to rely more heavily on the advice of others in making my selections. As it is, I had to omit a large number of items that I wish I had been able to include, items that could comprise a volume of size comparable to this one in the event that demand for a sequel to this volume should be sufficient to justify one. Should such a volume ever come into being, I hope I will be able to benefit from the advice of linguists whose filing cabinets contain mimeographed and Xeroxed papers that mine don't; I hereby solicit their advice and comments.

I wish to thank the authors represented in this volume for permission to include their papers and for the advice and information that they have given me, which have aided me greatly in the compilation and production of this book.

NOTES

[1]When I say "the transformational-generative tradition", I refer to the scientific communities whose intellectual parentage is traceable back to the small and easily identifiable community of transformational grammarians that existed in the late 1950s and early 1960s. I will use the term "transformational grammarian" to refer to anyone thus included in the transformational-generative tradition without thereby implying that the work he was engaged in is transformational grammar. In this connection, I emphasize that (i) I use the term "transformational grammar" neither as a term of approbation nor as a pejorative, (ii) the question as to whether a particular piece of work is transformational grammar is not meaningful unless one specifies how broadly or narrowly the term "transformational grammar" is to be taken, and (iii) that term can be made precise only through arbitrary stipulation. I have somewhat arbitrarily

excluded from consideration in this volume the versions of transformational grammar associated with the name of Zellig Harris (see, e.g., Harris, 1957, 1965); this exclusion reflects only the fact that Harris-type transformational grammar and the transformational tradition that this book is concerned with have had little mutual influence except in the early days of both. See Lees' "What Are Transformations?" (paper number 2 of this volume) for some remarks on the relationship between Chomsky's conception of transformation and that of Harris.

[2]A revolution in a scientific community usually involves not only a change in what claims and what concepts are generally accepted in the community, but also a change in the standards of acceptability for claims and concepts. Not surprisingly, an approach generally is virtuous by its own standards of virtue, e.g., descriptivism and transformational grammar have the respective virtues of being nonmentalistic and of being mentalistic. Polemics arguing the superiority of a new approach often merely show it to be superior by its own standards. Even when the polemic purportedly shows the new approach to be superior by the prevailing standards, the adherents of the new approach will generally assume new priorities among the prevailing criteria of acceptability and will appear to the adherents of the prevailing approach to be throwing out the baby with the bath water. The effectiveness of polemics is often not so much in popularizing the polemicist's substantive claims as in popularizing his priorities as to what one should care about.

[3]Scientific revolutions are less like political revolutions, in which existing institutions change hands, than like technological or market revolutions, in which a new product outsells an existing product of a similar function. However, a scientific revolution is more likely than a market revolution to be for the worse. The consumers of ball point pens need to write and the consumers of zippers need to keep their flies closed, and they will go back to the old product unless the new one satisifes their needs. But most consumers of scientific theories have needs that are easily satisfied, mainly that of having something that they can teach their students, which doesn't test the product as stringently as the consumers of ball point pens and zippers test those products.

[4]Two of the papers in this volume, Anderson's on do so (number 10) and Morgan and Green's on the cycle (number 14) touch on controversies surrounding this proposed organization of rules. Anderson's paper reflects the fact that it was written during a brief period when Ross and Lakoff, with whom he was working, had abandoned the cycle in favor of an alternative (sometimes called "linear cycle") of which more details can be found in the papers cited in the introduction to Anderson's paper. The Morgan-Green paper presents an argument for cyclic organization of transformations that is noteworthy in that it involves transformations that had previously played no role in such arguments. For information about different conceptions of "cycle", see the glossary of this volume under the headings "cycle", "last-cyclic", "post-cyclic", and "strict cycle".

[5]The perhaps bewildering differences among the papers of this volume as regards what syntactic category various items are assigned to reflects the fact that neither the "Aspects" theory per se nor any of the other versions of transformational grammar that are presented or alluded to in the papers that follow provides a clear basis for determining what syntactic categories there are. In fact, node labels are the detail of transformational analyses for which the argumentation is and always has been the shakiest. No variety of transformational grammar has provided both necessary and sufficient conditions for sameness of category. Generally speaking, interpretive semanticists recognize necessary conditions for sameness of category [i.e., they will provide arguments that two items are of different category on the basis of difference in syntactic behavior, as in, e.g., Jackendoff (1972, p. 100)] but do not recognize sufficient conditions, whereas generative semanticists recognize sufficient conditions for sameness of category [i.e., generative semanticists will provide arguments that two items are of the same syntactic category on the basis of likenesses in syntactic behavior, as in Bach (1968) and G. Lakoff (1970a)] but do not recognize necessary conditions. Moreover, the interpretive semanticists' necessary conditions and the generative semanticists' sufficient conditions cannot be combined, since they are not consistent with each other; e.g., the former imply that modal auxiliary verbs are not of the same category as main verbs, the latter that they are. My current conjecture is that syntactic categories as such do not exist; that the supposed syntactic category differences that have been recognized are merely reflections

of three factors: (i) the logical category (sentence versus predicate versus argument) of the corresponding element of semantic structure, (ii) the morphological category of the head of the constituent, and (iii) the position which the item is in in surface structure (e.g., whether it is in predicate position or modifier position), and that an adequate account of grammatical rules must operate in those terms directly, rather than in terms of syntactic categories.

[6]Of the few existing reviews of "Aspects", Matthews (1967) deserves to be singled out as particularly insightful.

[7]Significant exceptions to this remark can be found in the work of Paul Postal and George Lakoff. As early as 1963, Lakoff proposed (paper 3 of this volume) taking semantic structures as underlying syntactic structures and taking the base component of a grammar as generating the set of well-formed semantic structures. The fact that this proposal aroused no particular interest then (in days when the transformational literature was so sparse that the appearance of any transformational paper was cause for comment) can perhaps be attributed to the fact that transformational syntactic analysis was as yet so poorly developed that there was too great a gap between what Lakoff was proposing and what transformational grammarians were generally doing. Semantics had only been decriminalized and not yet legalized.

[8]This proposal, the so-called "performative analysis" was in fact suggested in Katz and Postal (1964, p. 149, footnote 9) but was dropped without being developed.

[9]The bulk of the papers just cited were written in 1966-1968, though many did not appear in print until later.

[10]Lees and Klima took the reflexivization transformation to be obligatory when the two NPs are first person or second person, because of such examples as *I hate myself/*me*. However, that restriction is incorrect due to the possibility of noncoreferential first person pronouns, as in *I dreamed that I was Brigitte Bardot and that I kissed me,* which refers to a quite different dream than does a corresponding sentence with *myself*. The paper by Warshawsky Harris in this volume (number 4) provides further criticism and revision of the Lees-Klima reflexivization rule.

[11]The problem of determining which transformations require this interpretation of identity conditions vanishes if one takes referential indices as attached not to a noun (as in Chomsky's proposal) but to a NP (as proposed in McCawley, 1968b). Referential indices are relevant to whether two NPs count as identical but not to whether two nouns do. See McCawley (1976b) for further discussion of the role of referential indices in identity conditions.

[12]Indeed, as far as I can recall, Chomsky mentions referential indices in his subsequent writings only in passages (such as Chomsky, 1970, footnote 11) where he condemns the "error" of identifying referential indices with the purported referents of the NPs.

[13]See in this connection McCawley (1968b), where it is argued that referential indices have the structure of sets rather than of discrete units, and Bach (1968), Postal (1968), and McCawley (1970a), where it is argued that in semantic structure, referential indices fill NP positions and nouns are in predicate positions.

[14]To get Lakoff's proposal to work for Ross's facts, it may be necessary to treat Equi-NP-deletion as leaving behind a phonologically null trace that continues to stand in anaphoric relations to other NPs.