J. Linguistics **35** (1999), 151–166. Printed in the United Kingdom

REVIEW ARTICLE

Newmeyer's cyclic view of the history of generative grammar

JAMES D. McCAWLEY

University of Chicago

(Received 9 February 1998; revised 26 June 1998)

Frederick J. Newmeyer, *Generative linguistics: a historical perspective*. London: Routledge, 1996. Pp. x + 218.

In this book (henceforth, *GL*), Newmeyer has collected 11 of his articles (including two co-authored ones), two of them previously unpublished, on the history of generative grammar, supplemented by reprints of two sections of Newmeyer 1986.

Part I 'General trends' begins with two lightweight but interesting papers, 'Bloomfield, Jakobson, Chomsky, and the roots of generative grammar' (11–16) and 'The structure of the field of linguistics and its consequences for women' (17–22). Next comes 'Has there been a "Chomskyan revolution" in linguistics?' (23–38), in which Newmeyer takes a more sophisticated view of the notion 'scientific revolution' than have many authors, dividing the question into three parts: (i) Is the content of generative grammar, especially in the form in which it first achieved prominence, in Chomsky's *Syntactic structures*, 'revolutionary', in the sense of making a major break from the tradition within which it originated? (ii) Has the effect of Chomskyan generative generative grammar been such as to change substantially the kinds of questions that linguists ask and the kinds of answers to which they give serious consideration? (iii) Has generative grammar achieved institutional power within linguistics?

In giving prominence to (ii) rather than to Kuhn's criterion of 'the resultant uniformity of belief, within the scientific community, in the new "paradigm"' (29),[1] Newmeyer follows Larry Laudan in holding that scientific

[1] Kuhn's brilliant but rather sloppily written 1962 book creates confusion through the great profusion of senses (Masterman 1970 identifies 21) in which the word 'paradigm' is used. The relevant sense of 'paradigm' here is what Kuhn later (1970) dubbed the 'disciplinary matrix' of the community, which is what gets replaced in a scientific revolution. I argue in McCawley 1985 that a paradigm in that sense is a system of markedness conventions that govern activity within the given community. It makes sense to speak of 'acceptance of' a paradigm but not of 'belief in' it. A paradigm normally has a theory as one of its components, in the sense that the paradigm entitles one to assume that theory in one's

revolutions (such as those precipitated by the work of Newton and of Darwin) have generally not resulted in any 'uniformity of belief' in a community but have rather occurred when 'a research tradition, hitherto unknown to, or ignored by, scientists in a given field reaches a point of development where scientists in the field feel obliged to consider it seriously as a contender for the allegiance of themselves or their colleagues' (Laudan 1977: 138). Newmeyer's answer to (ii) is: 'There was a Chomskyan revolution because anyone who hopes to win general acceptance for a new theory of language is obligated to show how the theory is better than Chomsky's. Indeed, the perceived need to outdo Chomsky has led him to be the most attacked linguist in history' (30).

Newmeyer's positive answer to (i) singles out as revolutionary features of generative grammar 'its conception of a grammar as a theory of a language, subject to the same constraints on construction and evaluation as any theory in the natural sciences' and the fact that 'it placed syntactic relations at the centre of *langue*' (24, 25). Chomsky raised the stakes in doing linguistics: there were now more ways in which a linguistic analysis could be wrong. Newmeyer answers (iii) negatively, disagreeing with those who hold that generativists have in some sense 'taken over' linguistics; he notes that 'the linguistics programmes at many major [American] universities are dominated by nongenerativists' (34) and that generativists have been underrepresented among grant recipients and the officers of the Linguistic Society of America.[2]

In 'Rules and principles in the historical development of generative syntax' (39–65), Newmeyer argues that the history of generative syntax does not consist of 'steady progress through a process of accretion', but rather involves four distinct historical periods (identified in terms of the variety of generative grammar that he holds to have been dominant at the given time) that were alternately 'rule-orientated' (sic) and 'principle-orientated', these labels referring to different priorities that prevailed in the different periods. The juxtaposition of this paper and the preceding one raise a question that Newmeyer unfortunately does not touch on: were the episodes that gave rise to the later versions of generative grammar that he takes up also scientific revolutions?

His first period is the 'rule-orientated' period of early transformational grammar (in the years 1957–1967), whose 'principal inspiration' is Chomsky's *Syntactic structures*, though Newmeyer makes this period extend

research, but to accept the paradigm it is not necessary to believe that theory – only to recognize the obligation to offer justification for adopting any other theory. (Conventions as to what sorts of justification serve to satisfy this obligation are also a part of the paradigm.)

[2] It would have been useful if notes had been added to update the facts cited in this chapter. For example, less than three years after it was originally published, the one position in the Linguistic Society of America that could be said to involve power, that of Secretary-Treasurer, was held by Newmeyer himself.

to 1967, when Chomsky's *Aspects* (not recognized by Newmeyer as a 'principal inspiration' for any period) was profoundly influential but *Syntactic structures* was no longer so.[3] He argues that generative grammarians in this period gave language-specific rules higher priority than general principles, noting that 'principles of UG were proposed in one work, then ignored in ensuing work' (45): linguists did not treat the principles that they proposed seriously enough to put much effort into seeing how they could be exploited.

Next comes a 'principle-orientated' period (the years 1967–1972), in which generative semantics (GS), with Katz & Postal (1964) as its principal inspiration, was dominant. A more plausible 'principal inspiration' for GS would be *Aspects*: a large part of the research that eventually evolved into GS was attempts by George Lakoff and others to fill in the gaps in the theory presented in *Aspects* and to exploit its ideas in new ways; early GS was Kuhnian normal science, with the *Aspects* conception of the relation between syntax and semantics as a Kuhnian 'exemplar'. Newmeyer's grounds for not assigning *Aspects* that role is that 'for the most part, what was new in *Aspects* was *reacted against* by generative semantics' (182).[4] It would be more accurate to say, though, that generative semanticists simply did not rank the new claims of *Aspects* the same way that Chomsky did and, accordingly, made different decisions as to which of them to retain and which ones to reject when it became clear that they could not all be accepted. From a generative semanticist's point of view in the late 1960s, Chomsky was a reactionary who had gotten cold feet after realizing where some of his new ideas led.

Here and in subsequent chapters, Newmeyer devotes considerable attention to the rapid dissolution of the GS research community in the mid 1970s. To minimize repetition of what I have said elsewhere (McCawley 1980a, b; 1995), I will only mention two respects in which I find his treatment deficient. Newmeyer makes frequent use of the term 'data base', a term that falsely suggests that the data relevant to a discipline constitute its subject matter. In discussing (53) his claim that GS responded to empirical problems by requiring that 'the types of data relevant for syntactic theory [be] expanded', he attributes to generative semanticists the bizarre idea that the relevance of particular facts to an issue is a matter for stipulation. But any

[3] Newmeyer's periodization has the peculiar consequence that 'the two most impressive pieces of scholarship' (57) in the lexicalist framework that predominated in his third period (Jackendoff 1969 and Emonds 1970) date from the second period.

[4] The list with which Newmeyer illustrates this statement includes one item ('the rationalist underpinnings of the theory') that was not an issue on which generative semanticists argued with interpretive semanticists. A similar statement appears on p. 156: GS 'in its late stages had come to challenge virtually every assumption about the workings of UG that had been put forward in [*Aspects*]'. I do not find it clear that 'UG' was conceived of in *Aspects* as something that has workings.

phenomena in which the details of linguistic units or linguistic rules play a role are in and of themselves relevant to the determination of those details, whether linguists like it or not; his statement (with reference to Lakoff 1971b) that 'the presuppositions of a sentence had now become data of relevance to syntax' wrongly suggests that they weren't data of relevance all along.

Newmeyer's account of the demise of GS also gives prominence to Peters & Ritchie's (1973) demonstration that 'transformational grammars, as they were then conceived [sic], had the weak generative capacity of an unrestricted rewriting system' (55). The stir that this result created among generative linguists is evidence of remarkable self-deception on the part of many of them. The notion of a language as a set of sentences was widely assented to by generative grammarians of the early 1970s, but was not taken seriously by them,[5] in that they (interpretivists as well as generative semanticists) were engaged in setting up rules and principles to account for the semantic possibilities of various syntactic structures rather than their mere possibility as purely syntactic entities. Thus, even the notion of 'strong generative capacity' (what sets of structural analyses a given type of grammars can generate), let alone that of weak generative capacity, was of little relevance to the concerns of generative grammarians of that period. Moreover, Peters & Ritchie's formal definition of 'a transformational grammar' reflected the preaching of transformational grammarians (such as the definitions of 'a transformational grammar' that they would offer when pressed), not their practice: it took in grammars that few 1970s generative grammarians would have taken seriously. Nonetheless, the theorem was widely interpreted as meaning that 'the then-current conception of transformational rules was so unconstrained that transformational grammar made no claim at all about any human languages except that its sentences could be generated by some set of rules' (56). In the ensuing panic, many generative grammarians came to regard all transformations as suspect and were receptive to syntactic analyses that made no use of transformations.[6]

This attitude of suspicion towards transformations characterizes Newmeyer's rule-orientated third period (the years 1972–1980), in which lexicalism prevailed, with Chomsky's 'Remarks on nominalization' (1970) as principal inspiration. It may clarify the issues if one distinguishes positive lexicalism (the view that the lexicon plays a major role in syntax, especially in the syntactic relationship between derivationally related words) from

[5] These two clauses embody a definition of a technical sense of 'red herring'. An alternative definition is: a proposition *p* is a red herring for a scholarly community C if the members of C generally do not believe *p* but do feel morally obliged to teach *p* to their students.

[6] According to Newmeyer, they inferred that 'the best hope was to eliminate [transformations] entirely by replacing them with PRESUMABLY less powerful lexical rules' (56, emphasis added). The presumption was baseless, since no one at the time had a clear conception of what should constitute a possible lexical rule, let alone any framework like Peters & Ritchie's that would yield conclusions about their generative power.

negative lexicalism (the view that syntactic relationships between derivationally related words do not involve transformations that combine the predicate element of an embedded S with an element of the frame in which it is embedded). Most authors who have written about such topics as nominalizations, in any syntactic framework, have accepted some kind of positive lexicalism; the most complete positive lexicalist treatment of English nominalizations done so far is a work (Levi 1978) that rejects negative lexicalism. Since the positive lexicalism of Chomsky 1970 is fragmentary and programmatic (Chomsky suggested how one might construct a complex lexical entry that specified what *destroy* and its action nominalization *destruction* share syntactically, but gave no hint as to how to deal even with agent and product nominalizations such as *composer* and *composition*), it was mainly his negative lexicalism that was influential.

Newmeyer describes research in the (negative-) lexicalist framework as taking a 'nihilistic direction', as one by one, the various transformations that had been widely accepted by generative grammarians of the 1960s were argued by lexicalists to fall short of their strict standards of what could be an acceptable transformation. In particular, any irregularity in conditions of applicability was held to disqualify a putative transformation and to require a lexical treatment of the phenomenon.[7]

The last of Newmeyer's four periods begins in 1980, with the principle-orientated Government-binding approach dominant and Chomsky's 'Conditions on transformations' (1973) as its principal inspiration. 'Principal inspiration', evidently means something like 'historical source' here, since Newmeyer clearly regards *Lectures on Government and Binding* (Chomsky 1981) as the most influential work in this period. As evidence for the dominance of GB syntax in period four, Newmeyer presents a breakdown of papers published in seven journals or presented at four conferences in 1986 and finds 72 GB papers, with no more than 10 papers in any other syntactic framework. He holds that solid advances were made in all four periods, but that in the first three the dominant approach 'ultimately degenerated into little more than a form of descriptive linguistics with a generative veneer'. This has not happened in the fourth period, he maintains, though expressing uneasiness about the freedom with which GB linguists have posited 'parameters' that cannot plausibly be claimed to have any universal status. The fourth period had not ended when this paper was published in 1991; it is not clear whether Newmeyer would hold it to have subsequently ended and a fifth period to have begun.

[7] Some of the negative-lexicalist arguments that Newmeyer summarizes are of abysmal quality, as where one author argues against a wh-movement transformation on the grounds that while *What the hell did you see?* is acceptable, **You saw something the hell* is not. That fact bears only on whether indefinite pronouns such as *something* are part of the underlying structure of interrogative expressions, not on whether interrogative expressions, whatever may underlie them, undergo movement.

'Chomsky's 1962 programme for linguistics: a retrospective' (66–79, co-authored by Stephen R. Anderson, Sandra Chung and James McCloskey) is an evaluation presented at the 15th International Congress of Linguists of the significance of Chomsky's contribution ('The logical basis of linguistic theory') 30 years earlier to the 9th International Congress of Linguists.

The theme of 'Linguistic diversity and universal grammar: forty years of dynamic tension within generative grammar' (80–97) is stated in its second sentence: 'From the start, the central generativist research project has been to extract unity from diversity, to uncover those principles of organization common to *all* of the 5,000-odd languages of the world' (80). Extended quotations from Chomsky (1986, 1955) that Newmeyer says 'make it clear that [in generative grammar] description and explanation go hand-in-hand' (81)[8] lead into a discussion of 'critiques of generative grammarians for over-attention to theory at the expense of data'. Newmeyer could have replied to these criticisms by simply pointing out that it is precisely generative grammarians who are most painfully aware that before novel questions were asked about such matters as control and anaphora, the available data were insufficient for linguists even to formulate accurate descriptions of particular languages, let alone universals. Instead, he wastes time on an ill-considered remark by Chomsky (1982: 82–83) that his paraphrase makes sound even more fatuous ('we have enough data at our disposal to formulate profound universals of language' (82)) than it was to begin with. Much of this section is a tiresome overview of linguists' claims to be more virtuous than various other linguists, at a considerable distance from any issues of substance.[9]

Newmeyer concedes that since the 1970s 'the generativist strategy has resulted in descriptive losses as well as gains. Various constructions that once figured prominently in theoretical debate, including *wh*-clefts, comparatives, gapping and other ellipsis processes, are today largely ignored (and unaccounted for), because the descriptive mechanisms they appear to require fall outside the reach of the principles recognized by the current theory' (84–85). At this point, readers can reasonably say, 'So much the worse for the current theory'. Newmeyer's attempt to mitigate this admission ('To be sure, some losses have been recouped: a notable example is the post-1980s analysis of English auxiliaries and negation initiated by Pollock (1989)') exaggerates how much has been 'recouped' in Pollock's analysis, which does not touch on such supposedly once solved problems as that of the possible sequences of auxiliary verbs in English.

[8] Actually, the Chomsky 1955 quote has to do with 'justifying and validating the results' of linguistic research, not with explanation.

[9] Newmeyer states that 'one can almost date a particular linguist's abandonment of the generativist programme with his or her first published attack on the "narrow" data base of the theory' (82). It is not clear how he understands 'the generativist programme' here, but if he means the goal 'to extract unity from diversity', he has not shown that the linguists to whom he refers (Charles Fillmore and George Lakoff) had abandoned it.

Newmeyer responds to the claim that generative grammar gives a privileged position to English[10] by pointing out that generative grammarians often do not force even English into the Procrustean bed of English, in that their underlying structures for English are in some respects more like surface structures of Arabic or Bulgarian, that a majority of MIT Ph.D. theses have dealt with languages other than English, and that the best-known expositions of virtually all other modern syntactic theories have dealt primarily with facts of English and/or the author's native language. He concludes by citing Ebeling's celebrated remark that phonemic analysis works fine on all languages except one's native language, within an argument that superficial analyses of exotic languages that one does not know well are liable to result not only in poor linguistics but in a demeaning view of the speakers of those languages.

Part II, 'The linguistic wars' (a title recycled from chapter 5 of Newmeyer 1980) consists of reprints of two sections of the 2nd edition of Newmeyer 1980 ('The steps to generative semantics' (101–112), and 'The end of generative semantics' (113–126)), and two reviews: a hitherto unpublished review of an earlier volume in the same series as *GL*, namely Huck & Goldsmith 1995 (127–137), and a review of *The best of CLS* (138–142). In virtue of my having already reviewed Newmeyer 1980 (see McCawley 1980a), my discussion of this part of *GL* will concentrate on the review of Huck & Goldsmith.

In *Ideology and linguistic theory*, Huck & Goldsmith attempt to apply Imre Lakatos's 'methodology of research programmes' to the competing factions of generative linguistics in the 1960s and 1970s. They give lists of putative 'core propositions' for the competing approaches, 'core' here referring to Lakatos's (1970) notion of the 'hard core' of a research programme: the propositions that are treated as immune to falsification within that research programme, in the sense that the blame for falsified predictions is always put on other propositions. As Newmeyer notes, what Huck & Goldsmith take to be 'core propositions' for 1967 GS clearly do not constitute a Lakatosian 'hard core', since 'a few short years after 1967, generative semantics had abandoned each and every one on the basis of empirical evidence' (133). It is only for 1967 that Huck & Goldsmith offer 'core propositions' for GS, whereas the 'Interpretivism' whose research programme they tabulate

[10] In his survey of objections of this type, Newmeyer misquotes Hagège (1976) and in the process wrongly attributes to him a blatantly false statement about Japanese word order. Hagège's objection to Chomsky's (1970) rule $X'' \rightarrow$ Spec X' was not that other languages have a different word order from English but that the significance that Chomsky attached to the fact that the various supposed 'specifiers' in English are all to the left of the corresponding X' is spurious: there are many languages in which determiners precede Ns and auxiliary Vs follow Vs, or vice versa, an observation that is made in McCawley 1975. In stating that 'English was being analysed, with no implications for universals', Newmeyer ignores Chomsky's implicit invocation of a universal in treating the supposed parallel between determiners and auxiliary verbs as significant.

putative components of is 'Late Interpretivist' (1995: 43); they thus do not even pretend to present the competing research programmes in forms that competed.[11] Moreover, since 1967 is the first year in which an identifiable GS research community existed, it is doubtful that a Lakatosian hard core had yet emerged: Lakatos (1978: 48) made clear that it takes time for the various parts of any research programme to evolve into a stable form. However, Newmeyer is no better at applying Lakatos's ideas to linguistics than are Huck & Goldsmith. His statement that 'GS was falsified because its core principles were refuted' (137) conflicts with his conclusion that the supposed 'core principles' did not really constitute a Lakatosian core. If he understands 'GS' as a Lakatosian research programme, it is a category mistake to speak of it as being falsified (only theories are falsified – research programmes can at most be discredited); the refutations to which he alludes could be part of an episode that was 'progressive' in Lakatos's sense.

In rejecting Huck & Goldsmith's claim that GB syntax is similar to GS in virtue of its 'transformational mapping of surface forms onto a level of logical form (LF), where quantifier-variable and antecedent-anaphor relations are represented' (129), Newmeyer correctly notes that 'LF' is not a level of semantic structure. He is not very explicit about why it is not semantic structure, but he could have mentioned that (notwithstanding its etymology) 'LF' is not logical form, since it does not in general provide enough Ss to serve as the scopes of all the quantifiers that a sentence contains: quantifier structures ($[_S$ Q′ S] combinations) can be stacked without limit in logical structure,[12] but Chomsky's Projection Principle prevents multiple quantifiers from being stacked in LF. Accordingly, May's (1985) rule of 'quantifier raising' yields outputs in which multiple quantifiers in the same S are sisters in LF rather than standing in aunt-niece relations, as they must in logical structure.[13]

Newmeyer's discussion of the dispute between GS and interpretive semantics (IS) over quantifier scope, gives an inadequate statement of the facts (it deals only with quantified subjects and objects and mentions neither differences among quantifiers nor differences among idiolects with regard to scope restrictions); his statement that 'all agree that the subject wide-scope reading seems somewhat more preferred or natural' (131) ignores such

[11] Huck & Goldsmith's lists of core propositions of the 'Late Interpretivist program' (1995: 43) and the '*Aspects* program' (1995: 26) differ only in respects relating to 'extra-grammatical principles'. I am puzzled at their failure to see in the 'cores' of either program any claims about the levels of syntactic structure that are systematically related to semantics. Newmeyer's discussion of interpretivism suggests a core proposition that Huck & Goldsmith missed: that there is no irregularity in syntax.

[12] To reduce the likelihood of confusion with 'LF', I will say 'logical structure' rather than 'logical form'.

[13] Newmeyer thus errs in saying that May's Quantifier-raising 'creates structures where the structural relation between the quantifiers corresponds to their scope relations' (131).

honorable exceptions as Kuno 1971 and Ioup 1975 to the dismaying readiness of linguists on both sides of the dispute to assent to such kindergarten-level caricatures of the facts, and conflates preferences with categorial restrictions, both of which figure in the dispute. He contrasts 'the generativist account', with semantic structures in which quantified expressions are sisters of their scopes, and a global derivational constraint excluding (in the relevant class of cases) derivations in which the surface c-command relation of two quantified expressions is the reverse of what it is in semantic structure,[14] and 'the interpretivist account', in which quantified expressions are within their Ss in deep structure, and something such as May's Quantifier-raising derives an LF from surface structure. His description of 'the interpretivist account' does not really make clear what mechanism is responsible for the (partial) parallelism between scope relations and surface structural relations.[15]

Newmeyer holds that 'the interpretivist account was superior' beause 'it allowed for a more constrained syntax' and 'it was embodied in a theory positing that scope is *universally* interpreted at surface structure' (131, original italics). Since GS syntax provided analyses both of what IS counted as syntax and of what it counted as semantics, Newmeyer is comparing apples with orange peels. He gives no reason to suppose that the relevant parts of IS that he leaves out of the first point were any more constrained than their rough counterparts in GS. Since IS at the time of this dispute had developed no general policies as to what could be a possible semantic interpretation rule or a possible semantic structure, it was IS that was the less constrained theory. The sole ground that Newmeyer gives for his claim to the contrary is that 'quantifier-lowering was a rule typologically unlike the bulk of the others that had been proposed' (131). But so few IS semantic interpretation rules had been proposed in the early 1970s that there is no base line against which to evaluate an IS quantifier-scope assignment rule for typological normality. Anyway, the mere fact that a rule is typologically unique is no objection to it: there are typologically unique syntactic and

[14] This treatment (Lakoff 1971a) is not the only generativist account of quantifier scope: an alternative has not a global constraint, but a constraint excluding derivational steps that (in whatever the relevant class of cases is) reverse the c-command relation of two quantified expressions. An important advantage of the latter version was discovered only later (McCawley 1984): in virtue of the cyclic principle, it yields an explanation (rather than merely a description) of the fact that it is derived grammatical relations that are relevant to restrictions on scope.

[15] Perhaps that gap is supposed to be filled by Newmeyer's cryptic remark that the Empty Category Principle 'is relevant to quantifier scope' (130). If that is what he means, then it is hard to see how the interpretivist account could deal with dialect variation or with differences among quantifiers (for many speakers, *Few linguists admire all philosophers* only allows an interpretation in which *few* has *all* in its scope, while *Some linguists admire all philosophers* is ambiguous with regard to scope, and for many others, both sentences have a scope ambiguity), or with preferences when both scope relations are possible. Does Newmeyer envision degrees of violation of the ECP or lexical exceptions to it?

semantic phenomena (think of all the peculiar properties of coordinate structures), and to require that the rules for them be typologically ordinary would be to require that one's analysis misrepresent them. Newmeyer's second reason for preferring the interpretivist account is equally unsupportable, since at the time of the dispute, neither side had made clear what parts of their analysis of (one variety of) English they would regard as universal. Each side's position seems to provide about as much that could plausibly be claimed to be universal as does the other's.

Newmeyer suggests two core principles of generative semantics: 'the nonexistence of a syntactic level of deep structure, that is, a level segregating the lexical and nonlexical rules' and 'the idea that all profound syntactic generalizations are semantically based' (135). The first suggestion is correct, provided it is regarded as a markedness principle (in the same way that Newton's first law was a markedness principle: it made uniform motion in a straight line unmarked, by contrast with the Aristotelean tradition, in which it was rest that was unmarked): there was no presumption that such a level existed, and a claim that one existed needed to be argued for. The second suggestion, however, repeats a common error about GS that rests on an equivocation about the word 'base'. Since virtually all generative semanticists accepted lexical exceptions to syntactic rules and all without exception accepted optional transformations, the conditions under which transformations applied could not be characterized in purely semantic terms, since their applicability could depend on something not determined by semantics, namely the prior application or non-application of a lexically conditioned or optional transformation. This was in fact a crucial assumption of the one part of GS that I now regard as a serious error (cf. McCawley 1982, 1988: ch. 7), namely the policy of reducing syntactic category differences to differences in logical category plus the structural consequences of lexically conditioned differences in the applicability of transformations (for example, verbs in English were the items that occupied predicate position after lexically conditioned insertion of copula *be*).

In his remarks about deep structure here and elsewhere in *LT*, Newmeyer takes for granted something that surely qualifies as a 'core proposition' of both the *Aspects* approach and IS, namely that the rules specifying the possibilities for constituent structure and constituent order normally (that is, in the unmarked case) relate directly to deep structure, a commitment that can be held responsible for Emonds's (1970) proposing a principle of structure PRESERVATION as a formalization of the idea that the outputs of most transformations are structures that 'the language allows anyway': for Emonds, the 'allowing' was done by rules that specified what syntactic configurations are allowed in deep structure, not (as in an obvious alternative) rules specifying what surface configurations the language allows. Newmeyer accordingly endorses the idea that 'at best syntactic conditions on surface structure can filter out sequences of elements whose generation a

more constrained approach would have banned in the first place' (136). 'In the first place' begs the question of whether deep structure has some ontological priority over surface structure, and Newmeyer's unsupported claim that an approach that does without surface combinatoric rules is 'more constrained' than one that accepts them sidesteps the issue of whether languages impose combinatoric restrictions on surface structure that have no counterpart in restrictions on deep structure combinatorics. It is in part because there are such restrictions (NPs as dependents on Ns are excluded in the surface structure of English but are allowed in underlying structures) that I have argued (McCawley 1988: §10a) for an inventory of allowed surface configurations as a component of a grammar. Huck & Goldsmith (1995: 50–51) are in error in attributing to me the view that such a module constitutes 'a Generative Semantic equivalent of Emonds's theory of structure preservation'; indeed, the nonequivalence was one of my main reasons for adopting it.

The final part of *GL* consists of three articles on 'Grammatical theory and second language learning'. In view of my unfamiliarity with research on second language learning, I will not comment on but merely summarize those parts of the articles that are in that domain.

'The ontogenesis of the field of second language learning research' (145–154, with Steven H. Weinberger) is a history of research (primarily in North America) on the acquisition of second languages, and of the shift of that field's primary concern from teaching to learning. Newmeyer begins the story by tracing 'contrastive analysis' from its central position in the field in the 40s and 50s until it had become such a marginal concern that it was absent entirely from the 1979 TESOL conference program. He notes that contrastive analysis conflicted with the post-Bloomfieldian descriptivist ideology, since it required that one identify linguistic categories across languages. Transformational grammarians in the 1960s were happy to make such identifications, and accordingly much transformational contrastive analysis was done in that decade. Such research was soon superseded, however, by research stimulated by the mentalistic view of language that transformational grammar popularized, devoted to the analysis of learners' errors in terms of hypotheses as to the grammars that they had acquired. Newmeyer notes the irony that 'the taxonomic theory of structural linguistics spawned the theory-driven second language research programme of contrastive analysis, while generative grammar, committed to deep explanation, gave birth to the data-driven, taxonomic programme of error analysis' (149).

According to Newmeyer, a serious obstacle to productive work in second-language acquisition was the widespread acceptance of the idea (Lenneberg 1967) that the innate capacity of humans to acquire languages is lost roughly by the time of puberty. He sees this obstacle being overcome in research dating from the 1970s that claims to show that 'second language acquisition

is, in crucial respects, like first language acquisition', for example, 'developmental L2 errors tend to mimic those committed by the L1 learner' (150).

In 'The current convergence in linguistic theory: some implications for second language acquisition research' (155–169), Newmeyer argues that, since the end of the dark decade that he sees the 1970s as constituting (he repeats accusations against GS that readers of *GL* will by now be weary of), 'there has been a surprising (and welcome!) convergence among leading frameworks on a wide variety of issues, some of them at a rather detailed level' (157). The 'leading frameworks' to which Newmeyer alludes are GB, generalized phrase structure grammar (GPSG) and lexical-functional grammar (LFG). He sees the major points of convergence as instantiating two ideas: 'that linguistic phenomena are best explained through the modular interaction of autonomously functioning systems' and 'that relations between grammatical elements meet strict locality conditions' (158).

The view of 'modularity' that Newmeyer presents is so nebulous as to take in virtually every attested approach to language, even those from the dark decade and before. The diagram that he labels 'The [sic] modular conception of language' (159)[16] includes a module labeled 'Formal grammar' and thus conceals sharp disagreement among linguists as to what modules make up 'formal grammar'. He holds modular explanations to be valuable because they enable one to 'regard superficial complexity as the product of simple principles, each from a distinct domain' (158), but if one deletes 'each from a distinct domain', one gets a concise statement about the virtues of interactive explanations. As I have argued elsewhere (McCawley 1997), many putatively modular explanations succeed not because they are modular (it is often immaterial whether the interacting principles and/or rules belong to different modules) but because they are interactive. Newmeyer's statement that 'virtually all grammarians take a modular approach to the interaction of grammatical principles' (168) is reminiscent of the ecumenical bromide that 'We all believe in the same God'; it ignores the wild differences among various schools of linguistics as to what modules there are and how they can interact.

Newmeyer treats the GB rejection of the idea of syntactic constructions as if it were a consequence of a commitment to modularity, which it is not,[17] and

[16] Notwithstanding Newmeyer's 'following Anderson 1981: 494', the diagram differs considerably from Anderson's.

[17] A theory of grammar that includes as a module an inventory of the language's constructions can of course be just as modular as Newmeyer's favorite modular theory. The quoted statement is part of a discussion of research in second-language acquisition that exploits Keenan & Comrie's (1977) accessibility hierarchy. Newmeyer states that that research is 'in need of reinterpretation' because the notion 'relative clause' 'has no theoretical status'. His statement (161) that the hierarchy 'centrally incorporates the

speaks of that dogma as if it had wider acceptance than it actually does, as where he offers as a truism a claim which will strike many of his readers as preposterous ('the notion "relative clause" is an epiphenomenon, a chance by-product of the interaction of a set of structural principles', (161)). His approving reference to Gazdar et al. 1985's treatment of subject-auxiliary inversion ('four separate principles interact to yield the surface forms, only two of which are particular to the construction', 161) indeed makes clear that the quite modular analyses of GPSG still make extensive use of construction-specific rules.

In 'Competence vs. performance; theoretical vs. applied: the development and interplay of two dichotomies in modern linguistics' (169–77), Newmeyer surveys invocations of generative linguistics by second-language professionals in the 60s and concludes that most of them either imported terminology but not content from linguistics or attempted to apply linguistic concepts in simpleminded and unproductive ways. In the 1970s, 'a consensus began to form that the theory has profound *implications*, though probably few if any direct *applications*' (173). He rejects arguments by Karl Diller (1971) and Chomsky (1970b) that 'empiricism is to the audio-lingual approach as rationalism is to naturalistic teaching methods' (174), arguing (as I would) that both empiricist and rationalist views of the nature of linguistic knowledge are consistent with a wide range of pedagogical ideologies, and argues more generally against 'the curious idea that a theory of the representation of knowledge invites a strategy for the imparting of that knowledge' (176).

GL contains a considerable amount of perceptive observation, as well as much obtuseness and myopia. In his interpretations of statements by generative semanticists, Newmeyer often sees things that aren't there, as where he takes my statement (McCawley 1979: vii) that 'I reject ... the notion of the "grammaticality" of a sentence considered apart from its meaning, use and context' as implying that 'the formal structure of language is derivative of extralinguistic context' (133). My statement in fact says nothing about 'the formal structure of [a?] language' or about anything being 'derivative of' anything. He goes on to say that that move 'went hand-in-hand with the abandonment of the idea that there might be principles independent of grammar interacting with it to produce the observed complexity of language' (133). His first illustration of such an interaction, namely Chomsky & Miller's (1963) attribution of 'the deviance of multiply centre-embedded sentences to memory limitations' is beside the point, since the acceptability of multiply centre-embedded sentences is not affected by meaning or context,

notion "relative clause" is incorrect: Keenan & Comrie make clear that the hierarchy applies to 'accessibility' in general, but concentrate on facts about relative clauses in illustrating it; their final section shows how it can be put to work in deriving implicational universals about causative constructions and advancements.

and moreover, generative semanticists never took exception to memory limitations as an extragrammatical factor that influences linguistic behavior.[18]

There is a lot of annoying repetition, as where four times (51, 82, 121, 149), George Lakoff's remark that 'I think that the time has come to return to the tradition of informal descriptions of exotic languages' is quoted out of context.[19] The editors at Routledge could have made *GL* much more appealling by insisting that Newmeyer eliminate much of this repetition, but it might have been a better choice still for Newmeyer to postpone publishing a collection of his historical articles until his output in that area had grown to the point where a significantly less repetitive selection was possible.

REFERENCES

Anderson, S. R. (1981). Why phonology isn't natural. *Linguistic Inquiry* **12**. 493–540.
Chomsky, N. A. (1955). The logical structure of linguistic theory. [Abridged edition published 1975, Chicago: University of Chicago Press.]
Chomsky, N. A. (1957). *Syntactic structures*. The Hague: Mouton.
Chomsky, N. A. (1965). *Aspects of the theory of syntax*. Cambridge, MA: MIT Press.
Chomsky, N. A. (1968). *Language and mind*. New York: Harcourt, Brace & World.
Chomsky, N. A. (1970a). Remarks on nominalization. In Jacobs, R. & Rosenbaum, P. S. (eds.), *Readings in English transformational grammar*. Waltham, MA: Ginn. 184–221.
Chomsky, N. A. (1970b). Linguistic theory. In Lester, M. (ed.), *Readings in applied transformational grammar*. New York: Holt, Rinehart & Winston. 51–60.
Chomsky, N. A. (1973). Conditions on transformations. In Anderson, S. R. & Kiparsky, P. (eds.), *A Festschrift for Morris Halle*. New York: Holt, Rinehart and Winston). 232–86.
Chomsky, N. A. (1981). *Lectures on Government and Binding*. Dordrecht: Foris.
Chomsky, N. A. (1982). *The generative enterprise*. Dordrecht: Foris.
Chomsky, N. A. (1986). *Knowledge of language*. New York: Praeger.
Chomsky, N. A. & Miller, G. A. (1963). Introduction to the formal analysis of natural languages. In Luce, P. et al. (eds.), *Handbook of mathematical psychology* (vol. 2). New York: Wiley. 269–322.
Diller, K. (1971). *Generative grammar, structural linguistics, and language teaching*. Rowley, MA: Newbury House.
Emonds, J. E. (1970). *Root and structure preserving transformations*. Ph. D. dissertation, MIT.

[18] Starting with Chomsky & Miller (1963), linguists have sidestepped the question of whether sentences that exceed short-term memory capacity might IN ADDITION violate some grammatical restriction, with the notable exception of Grosu & Thompson (1977), who argue that memory restrictions in themselves yield only a gradient lowering of acceptability, whereas sharp declines in acceptability that accompany small increases in memory load are evidence for grammatical restrictions that have at most a diachronic relation to memory: the restrictions may have arisen as conventions for avoidance of structures that often (but not always) seriously tax the short-term memory.

[19] Lakoff's point was not the anti-theoretical one that Newmeyer suggests but rather the practical point that informal descriptions will generally be more useful to scholars in subsequent generations than descriptions in terms of formal theories of grammar, because formal theories change so rapidly as make the descriptions quickly become unintelligible and the descriptions are liable to ignore important questions that the theory does not lead its practitioners to ask.

Gazdar, G., Klein, E., Pullum, G. K. & Saq, I. (1985). *Generalized phrase structure grammar*. Oxford: Blackwell.
Grosu, A. & Thompson, S. A. (1977). Constraints on the distribution of NP clauses. *Language* **53**. 104–51.
Hagège, C. (1976). *La grammaire générative: réflexions critiques*. Paris: PUF.
Huck, G. & Goldsmith, J. (1995). *Ideology and linguistic theory*. London: Routledge.
Ioup, G. (1975). Some universals of quantifier scope. *Syntax and Semantics* **4**. 37–58.
Jackendoff, R. S. (1969). *Some rules of semantic interpretation for English*. Ph. D. dissertation, MIT.
Katz, J. J. & Postal, P. M. (1964). *An integrated theory of linguistic descriptions*. Cambridge, MA: MIT Press.
Keenan, E. & Comrie, B. (1977). Noun phrase accessibility and universal grammar. *Linguistic Inquiry* **8**. 63–99.
Kuhn, T. (1962). *The structure of scientific revolutions*. Chicago: University of Chicago Press.
Kuhn, T. (1970). 2nd edition of Kuhn 1962. Chicago: University of Chicago Press.
Kuno, S. (1971). The position of locatives in existential sentences. *Linguistic Inquiry* **2**. 333–78.
Lakatos, I. (1970). The methodology of research programmes. In Lakatos & Musgrave, 91–196. Reprinted in Lakatos 1978, 8–101.
Lakatos, I. (1978). *The methodology of research programmes*. Cambridge: Cambridge University Press.
Lakatos, I. & Musgrave, A. (eds.) (1970). *Criticism and the growth of knowledge*. Cambridge: Cambridge University Press.
Lakoff, G. (1971a). On generative semantics. In Steinberg & Jakobovits, 121–196.
Lakoff, G. (1971b). Presupposition and relative well-formedness. In Steinberg & Jakobovits, 329–40.
Laudan, L. (1977). *Progress and its problems*. Berkeley & Los Angeles: University of California Press.
Lees, R. B. (1960). *The grammar of English nominalizations*. Supplement to *International Journal of American Linguistics*.
Lenneberg, E. (1967). *The biological foundations of language*. New York: Wiley.
Levi, J. N. (1978). *The syntax and semantics of complex nominals*. New York: Academic Press.
Masterman, M. (1970). The nature of a paradigm. In Lakatos & Musgrave, 59–89.
May, R. (1985). *Logical form: its structure and derivation*. Cambridge, MA: MIT Press.
McCawley, J. D. (1975). Review of Chomsky, *Studies on semantics in generative grammar*. *Studies in English Linguistics* **3**. 209–311. [Also in McCawley 1982b: 10–127.]
McCawley, J. D. (1979). Preface to J. D. McCawley (ed.), *Notes from the linguistic underground* (*Syntax and Semantics* **7**), New York: Academic Press. vii-ix.
McCawley, J. D. (1980a). Review of Newmeyer 1980. *Linguistics* **18**. 911–930.
McCawley, J. D. (1980b). Newmeyer on what happened to generative semantics. *Linguistics* **18**. 1099–1104.
McCawley, J. D. (1982a). The nonexistence of syntactic categories. In McCawley 1982b, 176–203.
McCawley, J. D. (1982b). *Thirty million theories of grammar*. London: Croom Helm.
McCawley, J. D. (1984). Exploitation of the cyclic principle as a research strategy in syntax. In de Geest, W. & Putseys, Y. (eds.), *Sentential complementation*. Dordrecht: Foris. 165–183.
McCawley, J. D. (1985). Kuhnian paradigms as systems of markedness conventions. In Makkai, A. & Melby, A. (eds.), *Linguistics and philosophy: studies in honor of Rulon S. Wells*. Amsterdam: Benjamins. 23–43.
McCawley, J. D. (1988). *The syntactic phenomena of English*. Chicago: University of Chicago Press.
McCawley, J. D. (1995). Generative semantics. In Verschueren, J. et al. (eds.), *Handbook of Pragmatics*. Amsterdam: Benjamins. 311–319.
McCawley, J. D. (1997). Review of Huang, C.-T. J. & Li, Y.-H. A. (eds.), *New horizons in Chinese linguistics*. *Journal of Chinese Linguistics* **25**. 341–356.
Newmeyer, F. J. (1980). *Linguistic theory in America*. New York: Academic Press.
Newmeyer, F. J. (1986). 2nd edition of Newmeyer 1980. Orlando: Academic Press.
Peters, P. S. & Ritchie, R. (1973). On the generative power of transformational grammars. *Information Sciences* **6**. 49–83.

Pollock, J.-Y. (1989). Verb movement, universal grammar, and the structure of IP. *Linguistic Inquiry* **20**. 365–424.
Steinberg, D. & Jakobovits, L. (eds.) (1971). *Semantics: an interdisciplinary reader*. Cambridge: Cambridge University Press.

Author's address: *Department of Linguistics,*
University of Chicago,
1010 E. 59th St.,
Chicago IL 60637,
U.S.A.
E-mail: jmccawle@midway.uchicago.edu