

# PROSPECTS FOR GENERATIVE GRAMMAR IN THE 1990s\*

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## 1. Introduction

About eight years ago, early in the 1980s, I began to reflect on the then current directions that were being taken by those parts of the field of linguistics that I felt I knew. My assignment then was to write the first in what was to be a seven-year series of opinion columns under the TOPIC...COMMENT banner in a new journal, *Natural Language and Linguistic Theory*.

My predictive successes were not negligible, but they were somewhat outweighed by my glaring failures, the developments I didn't see coming at all. I foresaw a consolidation of radical lexicalist approaches, and I guess the relatively harmonious relations between GPSG, LFG, and other unification-based versions of syntactic theory attests to the fact that consolidation took place. But I did not foresee the amazing battle of the initials that Generalized Phrase Structure Grammar (GPSG) gave rise to: HPSG (Head-driven Phrase Structure Grammar), IPSG (not currently in use, but it could be, for the Information-based Phrase Structure Grammar heralded in Pollard and Sag 1987), JPSG (Japanese Phrase Structure Grammar, developed by Takao Gunji in Japan in the middle 1980s), KPSG (Korean Phrase Structure Grammar, developed by a team in Korea some time later), LPSG (Linear Phrase Structure Grammar, also developed in Korea, by about 1988), and heaven knows how much further we may have moved toward the ultimate ZPSG theory by now.

I predicted a rapid spread of the government-binding (GB) theory, which was about as difficult as predicting continued movement of the earth around the sun. But I did not foresee the utterly extraordinary proliferation of phrase types and the reconstruction of features as abstract morphemes with X-bar projections that now adorn GB-style tree structures (more on this later).

I predicted increasing consensus about relatively surfacy syntax; the appearance at long last of a large number of relational grammar (RG) works; the failure of RG to become a mainstream paradigm for syntax; the cannibalization of RG work by GB researchers; the return of highly abstract phonology and the demise of 'natural phonology'; and the continued rise of interest in learnability issues. But there were many other significant developments that I did not see coming down the pike at all — for example, the return of so many classical generative semantic ideas in GB guise, the revival (after thirty years of neglect) of categorial grammar, the fact that RG would still be in reasonably good health by 1989, and in phonology, the rapid pace of tier inflation leading to phonological geometries that make fractals look positively unimaginative.

I have very little chance of doing much better as we stand on the threshold of the last decade of the twentieth century. But having been invited to address a conference on the theme of linguistics on the verge of the 1990s, I feel it is incumbent upon me to try. Like any other scientist attempting to make predictions, I will try to work by relying on standard historical and scientific principles — the principle of induction, which says that the future will be much like the past, and more specifically, the principle of uniformitarianism from

geology, which says that the present and future have causes of the same sort, i. e. that the processes that worked in the past to produce the record that we see in the present are working now to shape the future. Naturally, I shall do some browsing of past events and present trends before presenting any conjectures about the future.

## 2. Generative grammar in the decade 1979–89

Let me begin by pointing out an unwelcome truth that many will wish to resist: the actual achievements of generative grammar so far are remarkably slender. If one looks, for example, for known, uncontroversial, well-formulated, precise, universal principles, one finds there are virtually none.

My favorite example of a solid universal, one on which I felt I could give a convincing lecture to a room full of unsympathetic psychologists or biologists and make them see the point of generative grammatical study, used to be the Coordinate Structure Constraint of Ross (1967); but some recent work of George Lakoff's has changed matters decisively. Lakoff (1987) examined examples of VP coordination with multiple conjuncts, and he found an astonishing thing: any number of examples could be constructed in which some of the VPs had extraction sites and others did not. Here are a few examples, either taken from Lakoff's paper or modelled on examples of Lakoff's.

- (1) a. What did he go to the store, buy [e], load [e] in his car, go home, and unload [e]?
- b. How many courses can you take [e] for credit, still remain sane, and get all A's in [e]?
- c. Sam is not the sort of guy you can just sit there, listen to [e], stay calm, and not argue with [e].
- d. That's the stuff that the guys in the Caucasus drink [e] and live to be a hundred.
- e. This is the kind of brandy that you can sip [e] after dinner, watch TV for a while, sip some more of [e], work a bit, finish off [e], go to bed, and still feel fine in the morning.
- f. That's the type of firecracker that I set off [e] and scared the neighbours.
- g. It's a problem that I stared at [e], sat around for a while, fiddled with [e] some more, started working seriously on [e], got bored, and finally gave up on [e].

The implications of these examples for a universal principle like the Coordinate Structure Constraint are extremely serious. Whether the constraint is seen as blocking *wh*-type movement across the boundaries of a coordinate structure (Ross 1967), or coordination of dissimilar phrase types (Williams 1978, Gazdar 1981) or failure to instantiate the SLASH feature in accord with the Head Feature Convention (Gazdar, Klein, Pullum and Sag 1985; Sag, Gazdar, Wasow, and Weisler 1985), Lakoff's data motivate a fundamental re-thinking. His own analysis does not provide a serious new approach to the topic, for over and above its vagueness, it has nothing to say about all the facts concerning

coordination of other categories that the Coordinate Structure Constraint does account for, or about the related phenomena correctly predicted by the various syntactic accounts — e. g. the ungrammaticality of the following phrases:

- (2) a. \*the man who you saw [<sub>NP</sub> [<sub>NP</sub> e] and [<sub>NP</sub> a picture of [<sub>NP</sub> e]]  
b. \*the man who you saw [<sub>NP</sub> [<sub>NP</sub> the mayor of New York] and [<sub>NP</sub> a picture of [<sub>NP</sub> e]]

The Coordinate Structure Constraint held a special interest for me because it made predictions fairly directly about strings of words that would not have a grammatical structure. If it had been true, as Lakoff's data give us little hope that it can be, we would know exactly what to expect to be grammatical and ungrammatical by virtue of it. Other putative universal constraints in syntax have clearly been devised according to a very different philosophy. In particular, neither Subjacency nor the Empty Category Principle have this kind of closeness to the facts of grammatical constructions. Both are settling down to be organizing principles rather than hypotheses.

The ECP says that if nonterminals without dominated terminals are postulated, lexical categories governing them (roughly, immediately ~~not~~ c-commanding them) must also be postulated. This does lead us to look for any constituent types or other specific clusters of syntactic properties. Rather, it has led to a lot of linguists postulating invisible governors (e. g. null prepositions), shifting rules to the PF component (if they seem to violate the ECP), reworking constituent structure assumptions (to make sure there is a governor), and so on.

Similarly, Subjacency says that if long-distance movement is postulated, intermediate stopping places for the phrase that moves must also be postulated (how many being a point on which languages can differ). This has led to new stopping places being postulated (COMP nodes in PPs, for example; see van Riemsdijk 1978), proposals for varying the list of categories that can define stopping places (Rizzi 1978), and so on. This may lead us to new insights into syntax or it may not; but my point is that Subjacency does not present the same kind of opportunity for falsification that the Coordinate Structure Constraint does, and it is not intended to. It represents a very abstract limitation on the way in which theories are to be constructed; it is very securely insulated from confrontation with surface facts about grammatical constructions; and sociologically it is in the position of always being assumed to be correct: no one who mentions it imagines they might discover that it is wrong.

It is obvious that both principles (and many others in current syntax) are being used as guides to organizing the construction of theory, not as hypotheses about what syntactic structures will be observed in languages. This is not a bad thing, for every theoretician needs such organizing principles; but it is not something that adds up to any discoveries about syntax that could be called significant.

There have been no major descriptive syntheses produced in the last fifteen years of work in syntax. Generative syntacticians have become quite content with the idea that it is not their job to describe languages; their job is to search for over-arching principles of great depth and generality. The problem with this is that many of them seem to have completely forgotten what these principles are supposed to be about. The principles of universal grammar are principles of

design and function for grammars, that is, descriptions of languages. If there are no efforts at describing languages — efforts at saying for some fair-sized range of sentence types which sentences are in and which are out — there can be no evaluating of principles of universal grammar.

This is a point so obvious that it sounds almost inane to reiterate it; yet it is largely forgotten in the presentations today's syntacticians make of their work. Challenges oriented toward the question of whether an adequate description of the facts has been provided are rebuffed with an assertion that describing the facts ('merely' describing the facts) is a quite unimportant, even demeaning task; what is important is the glimpse provided of the grand, universal principles of the mind. It is hard to get across to people who think they have glimpsed a principle of the linguistic faculty of the human mind that they will have to substantiate that by exhibiting descriptions of linguistic phenomena which both appeal to the putative principle and equal or surpass previous descriptions in breadth or depth of insight.

It is worth keeping in mind, for comparison, that major synthetic works on language that deserve a permanent place in intellectual history have existed in the past, especially in the pre-generative era. *Roget's Thesaurus* is an example. The *Oxford English Dictionary* is another. And more recently, the 1630-odd pages of description produced by Quirk, Greenbaum, Leech, and Svartvik in their *A Comprehensive Grammar of English* qualifies. In generative grammar, nothing of this sort has been attempted since Stockwell, Schachter, and Partee (1973) published the results of the late-sixties research on English grammar funded by the United States Air Force.

When one reflects upon the expenditure of time and energy that has occurred during the generative period, the lack of substantive products is utterly amazing. The number of linguistics programs in North American linguistics departments grew from about 30 in 1963 to about 140 in 1984 (Newmeyer 1986, 45), much of the growth unquestionably being driven by the increasing influence and dynamism of the generative movement. The active membership of the Linguistic Society of America (LSA) roughly tripled between the publication of *Syntactic Structures* (1,354 in December 1957) and the publication of Ross's dissertation by Indiana University Linguistics Club (4,000 by the end of 1968, remaining there ever since; Newmeyer 1986, 44). About a thousand people now attend a large annual meeting of the LSA (like the one in Washington D.C. in December 1989) or a summer Linguistic Institute (like the one at Stanford in 1987), many of those people being younger faculty and graduate students, generally the most active and engaged members of the profession.

Suppose we assume, very conservatively, that about 100 full-time linguists world-wide were interested in generative grammar by 1960; that it was 200 by 1970 (177 linguistics PhDs were awarded in 1972–73), and that by the end of the 1980s 1,250 people world-wide were devoting their working hours to research in generative linguistics in various ways (the number of abstracts received for the 1990 West Coast Conference on Linguistics meeting was around 250, and surely not more than 20% of the world generative linguistics community sent in abstracts). No matter how you count it up, it amounts to a probable number of person-years of research in generative grammar that rises above 10,000. What do we have to show for that 10,000+ person-years of work, in terms of

substantial scholarly achievements that will stand the test of time as regards their results about language? I find the question rather spine-chilling, and I suggest that a minimal answer is that we cannot show enough.

One symptom of this is that major controversies in syntax still go completely unresolved for decades. In the earth sciences during the same period the theory of plate tectonics (continental drift) has gone from being radical to being solidly established fact about the structure of the planet (Tarling and Tarling 1971). In cosmology the Steady State theory has confronted the Big Bang theory, been tested, failed the test, and quietly given up the ghost, the Big Bang now being universally accepted as the correct conception of how the universe started. Meanwhile, in generative linguistics? Central questions both large and small stand unanswered, to be debated and redebated repetitively without closure.

Consider a fairly major issue in syntax: are there orphan VPs (complement VPs that do not have a unique dominating S at any level of syntactic structure) or not? The work of Brame, Bresnan, Culicover, Emonds, Fiengo, Gazdar, Lasnik, Wilkins, Williams, and many others since the early 1970s has argued that there are; these authors have analyzed at least some surface VPs (*try* complements, *tough*-movement complements, conjoined VPs, etc.) as orphan VPs. But some syntacticians have resisted this conclusion absolutely — notably Chomsky and Postal, plus all those who have followed Chomsky most closely, all generative semanticists, and all relational grammarians. Some have attempted to argue for closure of the issue (Koster and May 1981), but others have answered those arguments from a broadly Chomskyan perspective (see especially Culicover and Wilkins 1986). Nonetheless, the matter remains completely open, and the field remains obdurately split. Those who are ideologically committed to sticking with PRO subjects ignore the issue, and those who see no need for PRO subjects continue to employ orphan VPs. Neither side sets a high priority on determining which kind of analysis is optimal.

The related issue of whether auxiliaries are main verbs likewise never got settled. The arguments of Ross (1967), Pullum and Wilson (1977), and Gazdar, Pullum and Sag (1982) address some specific details of contemporary analyses, but in general are remarkably independent of changes of assumptions within generative grammar. They have not been answered. Instead, the AUX constituent of the *Syntactic Structures* has been quietly renamed INFL (in Chomsky 1981) and subsequently I (in nearly all current transformational work) without any clear changes in the basic character of the assumptions made about auxiliaries. The English modals, for example, are still regarded (in discussions that give decreasing amounts of detail) as some kind of nonverbal tense-related particle, in defiance of what is universally maintained by traditional grammarians like Otto Jespersen (1949), more recent descriptive grammarians like Harold E. Palmer (see e.g. Palmer and Blandford 1939), contemporary grammarians acquainted with generative analyses like Rodney Huddleston (in numerous publications) and so on, namely that the modals are verbs like all the 'auxiliaries' in English.

The problems posed by items like *would rather*, *had better*, *ought to*, *is to* (brought to the attention of the generative grammar community by Huddleston 1978) are forgotten in current work. These items are main verbs that have modal morphosyntactic properties. They are the only candidates for verb in the

topmost clauses in sentences in which they appear (for example, *would* is surely the only candidate for verb of the matrix clause in *I would rather you told him*). They lack third person singular present tense inflectional -s, but in current terms they must originate in VP and raise into I position. It follows that the alternative source (base generation in I?) assumed for modals is redundant, since all modals could be given the analysis that the *would* of *would rather* calls for. But instead, current analyses just skirt the issue uneasily. For example, Baker, Johnson, and Roberts (1989, 245, n23) say:

We are setting aside the exceptional, and somewhat archaic or dialectic, instances of main verb raising with *need*, *have*, and *dare*. See Pullum and Wilson (1977) for discussion.

The Pullum and Wilson discussion presses the case that the 'exceptional, and somewhat archaic or dialectic, instances of main verb raising' have to be taken very seriously as clear evidence that all auxiliaries can and must be treated as main verbs, but Baker, Johnson, and Roberts are content to be 'setting aside' this crucial dilemma. The issue remains unresolved.

Putting the auxiliaries problem together with others, we find, astonishingly, that simply saying what categories the words belong to in a short English sentence is a task that remains utterly beyond today's generative grammar community. Take a sentence like *We ought to be near the rich*.

- Is *we* entered in the lexicon as an N (though it doesn't take articles)? An NP (though phrase nodes are not usually found in the lexicon)? Or a special Pronoun category? And didn't Postal (1966)
- argue that pronouns are really definite articles?
- What is *ought*? A member of V? Or of M (modal)? Or of I (INFL)? What about *to*? Another base-generated I element — or did *ought* take up that slot? Is it under a T (Tense) or Agr (Agreement) node, perhaps (see Pollock 1989)? Or some kind of funny modal? Or a complementizer (that's what Postal and Pullum 1978 called it)? Or is it a preposition, like dictionaries say? Or even a verb, as Pullum (1982)
- and one or two other people have argued? Is *be* a verb? Or is there a special label COP for the copula? Or is *be* always generated in I?
- How about *near*? It takes a straight NP objects, so is it a P (Preposition)? But it inflects for comparison, so is it instead an A (Adjective)?
- Is *the* labelled Art (a lexical category for articles) or Det or Spec:N' (phrasal categories that can also dominate possessive NP determiners)? What about Sommerstein 1972, where it was argued that articles were really pronouns?
- Is *rich* a head noun here (since otherwise there isn't one in this noun phrase), or is it an adjective?

This brief consideration of live alternatives implies over four thousand analyses for the *flat* structure analysis of *We ought to be near the rich* (4,032, to be precise); then we start asking about constituency (and there are hundreds of logically possible bracketings, of which the number that linguists have considered for an example of this sort is larger than you think). What I am pointing to is that there is absolutely no sign of generative grammar reaching the point where

randomly selected practitioners will give approximately equivalent answers when asked for the syntactic surface structure of simple English sentences. The diversity of opinion is remarkable.

Take another big issue: the Projection Principle, and the question it was designed to short-circuit, namely whether there is such a thing as Subject-to-Object Raising (SOR), or more generally, whether there are any constructions which are correctly analyzed in terms of movement-derived or nonthematic objects (or the analogs of these in any other theoretical terms). The split here goes right across the whole field. It is now by no means the case that all relational grammarians believe in SOR and no Chomskyan transformationalists do. At the January 1990 conference on grammatical relations at the University of California, San Diego, there were three consecutive papers on whether there is SOR in Japanese (there isn't), Javanese (yes there is), and Korean (no there isn't), so the split begins to look as if it is by language (or language family) rather than by sociological subgroup within the syntax community.

But the big question is whether English has SOR. Since the feud between Chomsky and Postal on this issue began in 1969 (when Chomsky cast aspersions on the existence of raising into object position in his remarks at the conference on the Goals of Linguistic Theory at Texas (Chomsky 1972, 86) and Postal began to compose his book *On Raising* (Postal 1974) as a response), there has been no hint of a generally accepted resolution of the issue. In fact, things have spiralled downward from direct argument (as when Postal 1974 was answered by Bresnan 1976 and the latter was promptly rebutted by Postal 1977, and so on) to the much lower level of bluster and dishonesty (as when van Riemsdijk and Williams (1986, 33) assert that the dispute over SOR was a 'battle, which basically had already been won with the appearance of Chomsky (1973)', i. e. had already been decided by right-thinking people in Chomsky's favor before Postal 1974 even appeared). The generative grammatical community is quite unable to say univocally whether or not it agrees with traditional grammarians, who without exception describe English constructions like *We hold these truths to be self-evident* (the 'accusative and infinitive' of Latin grammar) as involving an object and a complement verb phrase — and it remains unable even after thirty-five years of research in English syntax and a major argument on the specific question that has been raging in the literature for nearly twenty years.

This kind of inability to achieve consensus or establish any general acceptance of results, even internally, does not betoken a field in good health. And the vanishingly small array of clear and uncontested results in generative grammar makes an investment of 10,000+ person-years look excessive.

One other observation I would make about generative grammar so far is that it is showing a retrogressive tendency to replace well-understood concepts by less well-understood ones. The strict cycle concept in syntax (Thompson 1975) was much better understood than the obscure conditions on argument structure with which Freidin (1978) began to replace it. The structure claimed for AUX was elaborated (e. g. by Akmajian, Steele and Wasow (1979) and Steele (1981) in much more detail than what has been suggested for I and its successors T and Agr (Pollock 1989). The classical transformational idea that expletive (dummy) NPs were those not present in deep structure but present in

surface structure was much clearer than the obscure present accounts, where nothing is clear about where expletives come from. The idea of deep structure or 'logical' grammatical relations was clearer than the notion 'theta-marked,' which apparently now does duty for it; and the idea of grammatical relations in cycle-final structure clearer than the notion 'Case-marked,' which has led to a succession of equivocations about what Cases there are, at what stage the marking takes place, how Case marking is authorized, and what elements are allowed to Case-mark NPs.

### 3. Generative grammar today

The central thing about the study of syntax as we enter the 1990s is that work in the 'principles and parameters' or 'government-binding' style (which for convenience I shall go on calling GB) dominates it as no variety of theory has ever dominated it in all the previous history of linguistics. The domination is almost total. The few conferences at which other approaches to generative grammar are featured (the bi-annual peripatetic Conference on Grammatical Relations, for example, primarily a relational grammar event) are like small specialist workshops. At the big conferences syntax means GB. And even the Conference on Grammatical Relations gets GB papers these days.

Much less widely recognized is a fact about the nature of GB work: it is moving ever closer to revealing itself as simply a reincarnation of Generative Semantics (GS). The parallels are striking, and numerous. I will give a round dozen.

- (A) No orphan VPs. As mentioned above, a one-to-one correspondence between deep subjects and deep verbs was a non-negotiable principle of the abstract syntax of the late 1960s that led to GS. Chomsky has never relinquished this principle, which entails many key similarities between GS and GB analyses.
- (B) Movement passive. If no VPs are orphans, a movement analysis of the passive construction can be maintained. The concern to have a simple object-movement operation to relate actives to passives motivates Chomsky (1981) — with its 'Move  $\alpha$ ' — as much as McCawley (1970) — with its movement of object NPs one constituent to the left.
- (C) Traces. Traces begin not with Thomas Wasow's dissertation (Wasow 1972) as is often suggested, but with Postal's ideas about DOOM marking, developed around 1968. In Ross (1969) a structure is given for a raising example in which a pronoun bearing the feature [+DOOM] is left in the pre-movement position (and an argument is given for the presence of the trace thus indicated).
- (D) The Universal Base Hypothesis. Linguists like Emmon Bach, George Lakoff, James D. McCawley, and others were talking about the idea that the base component might be a substantive universal — identical for all languages — from some time in the late 1960s. GB has remained true to this quixotic hypothesis, and since Stowell (1981) has maintained a somewhat attenuated version of it in which the universal base is in effect an infinite set of phrase structure rules (see Kornai and Pullum 1990 for discussion).



- (E) Abstract constituent order. The tradition of deep structure constituent orders distinct from surface orders (recall McCawley 1970) today lives on solely in GB work, where it is a major consideration. No other line of research in syntax retains anything like it; RG abandoned the idea of linear order at pre-surface levels in 1974, and in GPSG, HPSG, and LFG the notion cannot even be expressed.
- (F) High node-to-word ratios. The ratios of deep structure nodes to surface structure words in the abstract syntax of the late 1960s was astonishing then; some linguists laughed openly at the Lakoff and Ross structure for *Floyd broke the glass* (see Newmeyer 1986, 84, where it is recorded for posterity, apparently from lecture notes; it was never published by Lakoff or Ross). There were 7.75 nodes per surface word if we ignore the performative hypothesis (the top-level 'I declare to you'). Yet structures of the sort posited by Pollock (1989) and others have at least 8.25 nodes per surface word (also without assuming the performative clause). To save paper and artwork costs, I will not reproduce complete structures here to demonstrate this; the reader may easily verify it.
- (G) Quantifier lowering in the syntax. The quantifier lowering of generative semantics work from the later 1960s to the mid 1970s (Postal 1974 gives a more explicit account than is found almost anywhere else) is not really a different analysis from what is argued for in May 1977 and subsequent works. For May and other GB linguists the quantifiers are in fact raised, because the rules derive 'Logical Form' configurations (GS deep structures) from 'S-structure' representations (shallow structures) instead of the reverse; but this makes no substantive difference, as was well understood in the heyday of the arguments between generative and interpretive syntax. The important thing is that quantifier scope is being treated as a syntactic phenomenon, to be handled with movement rules and tree configurations, not a semantic one.
- (H) Predicate raising. Transformational raising of verbs and amalgamation of their clauses (and sometimes their morphological identities) by means of derivational steps was a hallmark of GS work. Despite the remark of Chomsky (1972, 86) that predicate raising 'surely is not' motivated, verb raising in syntax is today the hallmark of GB. Many of the applications (e. g. causative constructions, or combining verbs with their tense properties) are the same as the ones for which GS used predicate raising.
- (I) Tense and Neg as a higher verbs. The main content of the GS claim that elements like Tense and Neg were verbs of higher clauses was not so much that they were verbal (they were uncontroversially a bit different from most verbs in morphosyntactic behavior) but that they were higher: they represented whole separate domains of predication asymmetrically commanding the verbs they superficially appeared on or adjacent to or above. GB today represents this by having such elements as bases for complete maximal projections superordinate to the VP domain. The inspirations of this idea come from Ross (1967) and McCawley (1971), though these are not cited in works like Pollock (1989).
- (J) Cyclic and postcyclic rules. Developing throughout the GS period was the idea that cyclic and postcyclic rules were very different, the former being

local and involved with grammatical relations like subject and object, the latter being nonlocal and never concerned with the creation of subjects or objects. This typology of rules was rendered more explicit in the early stages of the presentation of relational grammar by Perlmutter and Postal. In GB the GR-changers are called A-movements (movements to argument positions) and the formerly postcyclic rules like *wh*-movement have been named A'-movements (movements to the set-theoretic complement of the A-positions, i. e. non-argument positions). Cyclic application of transformations has been jettisoned, but the typology is the GS/RG one; the obscure new names seem to have been chosen to disguise the conceptual debt.

- (K) Deep case participant roles. The theta-roles of GB are of course essentially a rebirth of Fillmore's deep cases. In recent work of Fukui & Speas (cited by Pollock), Fillmore's 'subjectivalization' transformation has been brought back quite explicitly.
- (L) Transderivational constraints. There can be no question that if Pollock's (1989, 420) notion of an English-specific 'Avoid *Do* principle' (supposed to guarantee minimal use of the lexeme *do* and thus block auxiliary *do* where it needs to be blocked) were made precise instead of being left in hand-flailing mode, it would have to surface as a transderivational (i. e. interderivational) constraint of the sort that George Lakoff was being ridiculed for in the early to mid 1970s. Pollock seems to mean that the grammar contains a constraint preventing use of *do* where there is an equivalent derivation of a different sentence that has the same meaning and lexemic content except that it lacks *do*.

This list can be continued almost indefinitely. GS is a rich lode to mine, and GB work is expanding at great speed. But one feature of GB links it to GS even more deeply and at a more general level than any of the small theoretical borrowings listed above: the irredeemably informal character of rule and principle statements and the avoidance of grammar fragments recalls irresistibly the days when once Postal (see Peters 1972, 168, n. 50) announced 'a no doubt never-to-be-written paper' by Lakoff, Postal, and Ross, to be called 'What to do till the rules come,' which would argue against the construction of generative grammars — the formalization of rules or constraints — and in favor of general argumentation surrounding what character rule systems (if they ever arrived) would need to have.

The vagueness of the general principles given in GB has been alarming for some time; this is a framework in which 'Move  $\alpha$ ' was supposed to be the key explicit statement of the theory of movement rules (which is something like suggesting that set theory should be based on the axiom 'Pick element' and leaving everything else undelineated). Yet it has slid downward from there toward even more obscure 'principles' like 'Affect  $\alpha$ ', 'Assume Grammatical Function', and 'Avoid Pronoun'. (Note the imperative mood of these, and consider this question: who is the addressee?)

The 1980s end with the vagueness and sloppiness of GB work reaching levels that seem all the more ludicrous because they lack the self-conscious whimsicality of later GS work. The flavor is of solemn self-parody. Consider again the note in which Pollock (1989, 420) suggests an 'Avoid *Do*' constraint:

Perhaps there is an 'Avoid Do' principle in the grammar of English falling under some version of Chomsky's (1981) 'Avoid Pronoun' principle, itself conceivably the by-product of some more general 'least effort' principle.

Perhaps this, conceivably that, maybe grammars are trying to save energy . . . It is just astonishing that such maundering should be turning up in what purports to be a refereed journal of a field with formal underpinnings and technical content.

Again, consider the furtive treatment of features and feature percolation found in current GB, with its impressionistic diagrams of arrows pointing in the direction of some flow of features that the theory does not actually determine (some examples from recent issues of *Linguistic Inquiry* are reproduced in Figure 1). GB desperately needs a serious theory of features, if only to save the costs of extra paper and charges for artwork which are called for at present. The level of precision has gone dramatically downward from works like Lakoff (1970), and at the same time works like Gazdar et al. (1985), which attempt to make some progress on developing an explicit theory of syntactic features, are rigidly ignored in GB, for ideological reasons. Where ideas from such work are needed, as when Abney (1987, 236) finds use for the idea of making bar level a syntactic feature, they are simply reinvented or lifted without remark (Abney proposes his

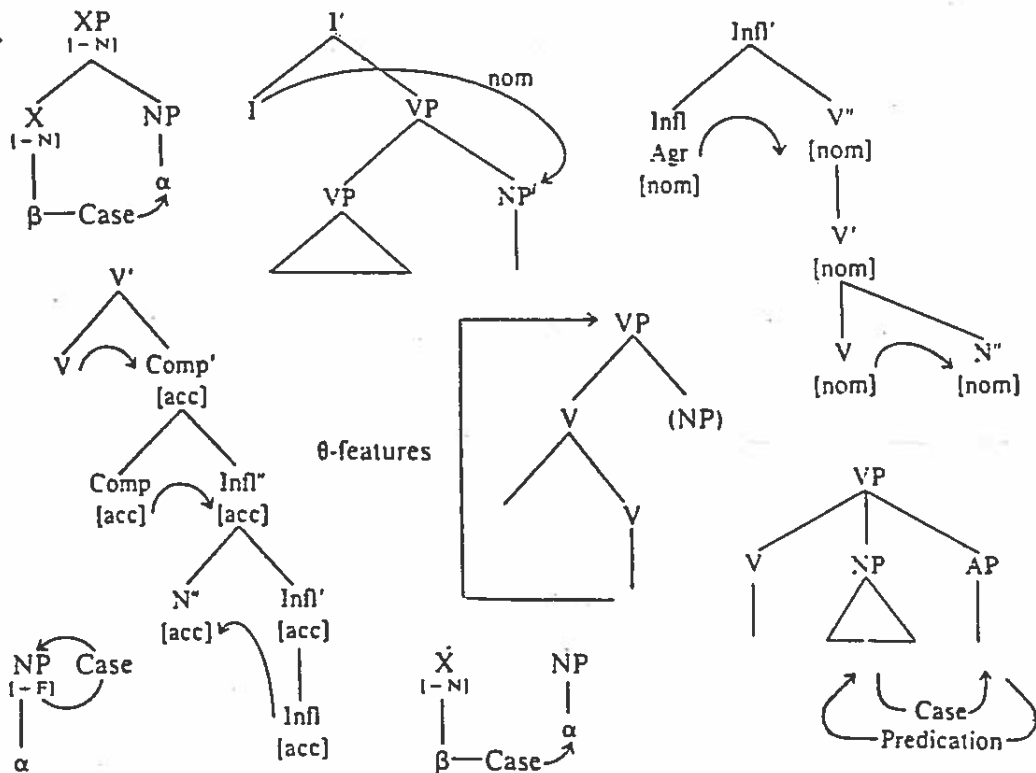


Figure 1: Some recent impressionistic pictures of feature migration, taken from issues of *Linguistic Inquiry* during the late 1980s.

bar level feature with no apparent realization that it is already five years old in the GPSG literature).

We should distinguish at least four properties of grammatical work here: *detail, breadth, precision, and formalization*. These are mutually independent (the third is not the same as the fourth, for example). The remarkable thing about current GB work is that it lacks all of them at once: it is not detailed like Jespersen (1949), or wide-ranging like Greenberg (1963), or precise like Bloch (1946) or formalized like Montague (1973). It deals in a selective way with narrow ranges of facts and provides accounts that are both vague and informal.

In fact, there are some signs of undervaluing of work exhibiting improved descriptive coverage and thoroughness. It is clear, for example, that the thoroughness and responsiveness to the relevant facts of a work like Kuno (1987) merits considerable influence in the field, but instead this work is hardly ever cited. Something similar could be said about Gunji (1987) and about the many papers published (mainly in *Linguistic Analysis*) by Kunihiro Iwakura. (I hope it is just an accidental fact that these scholars are all Japanese. We have seen before, in a variety of industries, the consequences of American assumptions that their sloppy work will always rank higher than more careful craftsmanship from Japan.)

In sum, I am skeptical about the chances of today's generative grammar lasting very long in the intellectual history of linguistics. I am not asserting that nothing of it will survive; doubtless, some concepts of current generative grammar are here to stay. The idea of analysis in terms of categories and features is likely to be robust. Headedness, the fundamental concept explicated by X-bar theory, is doubtless quite important. But these amount to little that is new; both were implicit or even explicit in context-free phrase structure grammar and dependency grammar some thirty years ago.

The most important developments of the generative period were perhaps the recognition of the notion of unbounded dependencies and the discovery of syntactic constraints on anaphoric relations. But the analysis of unbounded dependencies into local domains, either GB-style, in terms of subjacency, or the even more local analysis of GPSG (introduced in Gazdar 1981) has altered the status of the former; it falls into place as an oversight of traditional and structuralist grammarians, but not a fundamentally new kind of grammatical phenomenon.

The discovery of syntactic constraints on anaphoric relations seems truly new; there just isn't anything on the subject in traditional grammars as far as I am aware. But in this area the problem for generative grammar is that despite an enormous amount of work on the subject, the widely accepted, precisely delineated description of how those constraints are framed, even for one language, has yet to appear. Problematic examples have shifted back and forth across the grammaticality line for years, and highly salient counterevidence has been casually described as on the 'marked periphery' of the syntactician's purview. Even the most fundamental elements of the paradigm that is to be used seem still to be up for grabs; Zribi-Hertz (1989) provides a good example of a recent contribution that makes it clear how far the field is from being able to say where you use what kind of pronoun in standard English.

In a review article published in the last issue of *Language* in the 1980s, Stephen Anderson (1989) laments the failure of linguistics to make enough of an impact on cognitive science to show up centrally in a textbook like Johnson-Laird (1988). But it is not surprising to me that Johnson-Laird pays scant attention to the generative syntax of the last ten years when introducing students to cognitive science in a book entitled *The Computer and the Mind*. There is very little in such work that can be genuinely (as opposed to rhetorically) connected to what is known about either the computer or the mind. It would be good if there were more work that did make the connection, to be sure; but in berating Johnson-Laird for including little recent linguistics in his book, Anderson is merely shooting the messenger.

As a reference to support his claim that 'The list of linguistic constructs and results that have been subjected to concrete empirical examination is truly massive,' Anderson cites the papers in Newmeyer (1988). But amusingly, only one page of *Language* need be turned to find the review of Newmeyer's collection by Richard Hudson (1989). Hudson (p. 814) quotes Newmeyer's introductory claim that 'the prestige of generative grammar among psychologists, neurologists, computer scientists, and so on has reached an all-time high' (a claim that would contradict Anderson's main complaint), but comments that in actuality Newmeyer has simply 'managed to bring together a fair number of scholars who share his enthusiasm for GB,' and that he is 'scraping the bottom of the barrel too hard in his search for motivation for GB.'

I think Hudson is correct on this: linguistics does *not* currently have much prestige in psychology or computer science. Anderson sees a sign of that in Johnson-Laird's book, and lays the blame mainly on the book. Hudson sees, as Anderson apparently does not (though he may suspect it just a little), that linguistics has yet to show that it would merit such prestige. Hudson notes, for example (p. 813), that Lightfoot's theory of 'trigger experiences,' to which Lightfoot accords a central place in the chapter he contributes, is not supported or mentioned by any of the acquisition-related chapters elsewhere in Newmeyer's survey; he notes that Weinberg's argument for GB grammars on grounds of their brevity is fatuous, since no GB grammars have ever been exhibited; and so on. It is the vacuity of generativist works such as these that lies at the root of generative grammar's low profile in mainstream cognitive science.

What generative grammar should undertake during the 1990s is a program of work that would have enough substance to potentially earn it a place in cognitive science. There are many possible directions to take in future developments (not by any means mutually exclusive alternatives). One might imagine pursuing work on the mathematical foundations of linguistic theory, either in grammatical descriptions or in studies of formal learning theory as applied to human languages;<sup>1</sup> or on increasing breadth and depth of treatment of languages, either through comparison across a wide range of languages or through detailed description of a particular language; or insight into psychological or biological capacities, either by experiments on language use in mature users or by observation of language acquisition; or an understanding of change and variation, either via the historical evolution of languages or via variation in and between idiolects; or the development of practical applications, either in terms of pedagogical applied linguistics or in terms of industrial applied linguistics

(linguistic engineering).

All such types of investigation have some value. What worries me about the current scene in generative grammar is to see signs of work that cannot be regarded as advancing toward *any* of these goals. Formal rigor is neglected in favor of buzzwords and speculation; breadth is maintained neither in the range of languages made relevant to the inquiry nor in terms of the array of constructions marshaled within one language. Experimental psycholinguistics and developmental acquisitions studies are both neglected as sources of data. Change and variation are rigidly idealized away. Practical applications are spurned. It is unfortunately all too easy to find examples of work satisfying this checklist of negatives, in any recent issue of *Linguistic Inquiry*, *The Linguistic Review*, or other journals publishing contemporary syntax.

It is particularly strange to watch generative linguistics drift away from the psychological and biological goals it claims to have set itself. For example, anyone who truly believed that the biological capacity for language was a genetic attribute, transmitted through the gene plasm in a quite detailed form, would surely (at least, if they understood genetics) look for biological differences between breeding populations of human beings that correlate with differences in UG. This is a point that was made very explicitly by McCawley (1978, 216). It was also made independently by Sampson (1979, 142ff).<sup>2</sup> The strange thing about generative linguistics of the Chomsky school is that it maintains the geneticist line for rhetorical purposes (as a way to connect linguistics to discourse about biological studies of cognition), but pays no attention at all to the questions that McCawley and Sampson raise.

There are in fact works in the literature that seem to suggest genetic variability in the language faculty, but they are resolutely ignored by generative linguists. A clear recent example is the work of Cowart (1987) and the work of Bever and associates cited there. Cowart's result is that right-handed people with left-handers in their family process anaphoric linkages differently from right-handed people who have no left-handers in their family. This really does suggest something genetic that connects with syntax and semantics. But as far as I know, syntacticians and semanticists have paid not the slightest attention to it.

That generative linguistics apparently never gets serious about its purported psychobiological kinship structure is seen in the way it does not allow evidence from experimental or observational psycholinguistics to intrude on its hypothesizing. No generative linguist ever sets up crucial experimental or observational opportunities for falsification of purely grammatical hypotheses. As far as I know, there is no instance in the literature of a generative grammarian accepting an *unwelcome* conclusion (as opposed to the one their theoretical arguments incline them to anyway) on the grounds that experimental or observational data from psycholinguistics forced it.<sup>3</sup>

Generative grammar stands today, then, in a rather isolated and unstable position, little of its methodological stance finding real support from its practice. It is in this context that I attempt, rather uneasily, to glimpse something of the likely future of the field in the 1990s.

#### 4. The 1990s: some predictions and an appeal

What can be predicted about generative grammar in the 1990s? It is rather easier to discern developments related to the profession, of course, than to the intellectual drift of things. For example, one thing that seems clear about the profession is that after the rising profile of work that applied generative grammar to symbolic computational linguistics in the 1980s, there will be a decline in such work in the 1990s. The withdrawal of military funding in the late 1980s will hit hard, virtually destroying the active groups at SRI International, Bolt Beranek and Newman, and the Information Systems Institute in Southern California. The highly statistical research that has begun to take over natural language research at sites like IBM Research (Hawthorne and Yorktown Heights) and AT&T Bell Labs (Murray Hill) will find scant use for linguists. The main jobs for linguists will continue to be in academia.

Within the academic profession in the United States, there is likely to be an increase in the number of academic jobs for linguists, especially west of the Mississippi. The increase will be slow, but will continually pick up during the 1990s unless slowed by serious economic disasters (stock exchange crashes; great earthquakes; or the kind of penny-pinching kitchen-table legislation seen in California's proposition system, where voters attempt to deal with highly technical State budget issues by referendum).

Demographics will be driving this growth in the number of academic jobs. In some areas (California being an example) there are signs of an upsurge in the number of young people who will be wishing to enter universities (especially State universities); but there is also a coming wave of faculty retirements. During the 1990s, people who got their PhDs in the early 1960s, and took university jobs in the expansion of universities that was then going on, will be coming up to an age when, despite laws ensuring that they cannot be required to retire, they will nonetheless be thinking about retirement. A linguist who received the PhD at the age of 30 in 1960 will be 60 now, and looking at retirement within five years or so. And by the year 2000, the pace of retirements will be quickening.

Another predictable element in the linguistics profession of the 1990s is that there will be essentially no Blacks or Chicanos in general and theoretical linguistics at all. I know of three African-American US citizens in linguistics graduate programs in the whole of the United States today. This is as close to nonexistence as makes no difference; the likelihood of a Black candidate turning up in theoretical linguistics faculty searches in the next few years can confidently be set at approximately zero. This should be reflected upon in the context of a country at large in which there are increasing cries from minority students in all subjects for more representation of their groups in university faculties. This generally neglected feature of linguistics — the fact that the profession is about as ethnically diverse as physics — is something that linguists should be considering more seriously.

In terms of theoretical framework, it is quite obvious that GB-style theorizing is set to dominate the whole field of linguistics throughout the 1990s. The present graduate students, after all, will be the research-active junior faculty of tomorrow. Just as we can see that hardly any of them are Blacks or Chicanos, we can see that nearly all of the non-phonologists do GB syntax. But there is a problem for GB's internal development. For the last few years, GB has been

developing through rediscovery of generative semantics ideas from about ten to fifteen years before. The problem is that the vein being mined is about to run out. By 1975, generative semantics had collapsed and disappeared. Once 1975 is reached, there is nothing more to borrow. It is hard to see what GB can do about this. Perhaps its best bet would be to work on mining the history of RG, borrowing and recasting the work that Perlmutter, Postal, and others have been doing over the last decade and a half. To some extent this is already happening.

There are other developments that I would predict with more or less confidence: some continued popularity for 'functionalist' syntax (there has to be something to say about grammar at Berkeley Linguistics Society meetings); further interest in the lexicon (fueled by the mutually reinforcing trends of GPSG/HPSG syntax, GB lexicon/morphology interests, and lexicographical work in computational linguistics); a return to some extent of corpus-based linguistics (the Association for Computational Linguistics has a Data Collection Initiative aimed at amassing 100 million words of machine-readable text, and something will have to be done with it); and just possibly, the beginnings of some serious grammar-testing by computer (machines powerful enough for this are now reaching linguists' desks).

In phonology, of which I have said very little here, I would predict that phonetics will receive some renewed interest (there is positive pressure from the speech industry as well as from military funding agencies, which continue to be interested in speech analysis and synthesis, as well as a variety of reinforcing phonology-internal developments and a rash of forthcoming efforts to formalize nonlinear phonology in various ways that involve explicit phonetic representation).

But beyond these hints, I have great difficulty in fulfilling the terms of my mission. Switching from the future indicative to the optative, I will close with an appeal rather than a prediction. If I had to name one thing that I felt would be most valuable for the health of linguistics in the 1990s, I would say that what was needed was a large-scale theoretical synthesis and description effort on the syntax of a single language studied in depth — probably English. What is needed is an effort that would combine the energy and consciousness of detail seen in the best work of Bresnan, Emonds, Postal, Kayne, McCloskey; the organization and cooperative spirit seen in the team of Quirk, Greenbaum, Leech, and Svartvik; the exhaustive coverage seen in the finest dictionaries of the English language. Linguists are not pulling together the ideas they entertain. The discipline of a team effort to lay out a serious reference grammar of English has been lacking for too long. The task will be a large one, and difficult to organize, but it would be worth it.

I disagree diametrically with Anderson (1989, 809) on the risk that linguists might be 'sent back to the narrowly humanist ghetto from which the field managed to emerge in the 1960s.' A lot of discipline and scholarship was left behind during the flight from that ghetto; linguists have plenty to gain from rediscovering their roots. And they will not lose whatever respect they might have in the cognitive science community by doing their descriptive work more thoroughly (any more than the computer science community has lost prestige from supplying us with the UNIX operating system and windowing environments). Anderson bemoans the fact that linguists at the moment are not convincing



cognitive scientists and the like of their claim to be heard. They will do much better, I believe, if they can point to a basis for their elaborate conjectures about mental structure in a comprehensive and widely supported description of the syntactic structures found in even a single language.

Those who are inclined to dismiss such encyclopedizing work as relatively dull when compared to the exploration of the origins of the universe or the probing of human cognitive capacities and their genetic basis should reflect on the fact that astronomers and cosmologists have spent the better part of the last decade constructing detailed maps of the universe, and scientists who work directly on the foundations of genetics have decided that they will spend several billions of dollars over the coming decade or two constructing a complete map of the human genome. An exhaustive account of what we now know about the syntax of English will be a small job by comparison with these giant cartographic endeavors. We can spare a few hundred person-years, surely.

#### Footnotes

\* My remarks here, though adapted to the written medium to some extent, are largely in the form of the lecture I gave at the conference, and some informality with respect to documentation and citation may be evident; I hope the reader will forgive these. Thanks to those in the audience at Tempe who argued with me; they will note in reading what follows that unfortunately their arguments had very little effect.

<sup>1</sup> The view that linguistics is actually a part of human cognitive psychology or even human brain neurobiology is one that I cannot accept (or see as fully coherent). But since I take cognitive science to be a field much broader than human psychology or brain biology, concerned with the abstract structure of entities of any sort (computing machines included) that might be said to be capable of cognitive activity, I do not have trouble with the idea that formal grammar (or parts of logic or theoretical computer science) fall within cognitive science.

<sup>2</sup> Sampson's remarks are part of a highly iconoclastic argument in which he links the geneticist position on universal grammar to Jensen's racist views on IQ and intelligence. I do not accept the whole of Sampson's argument, for reasons relating to the concept of IQ and the use Jensen makes of his data on race and intelligence, but this is irrelevant here. Sampson is perfectly correct that if universal grammar (or 'intelligence') is inherited genetically we should expect genetic variation in it.

<sup>3</sup> This claim, which I have enunciated before in various lectures, finds an echo in Hudson (1989, 819), discussing Newmeyer (1988): 'The survey does not contain a single example of a grammatical analysis that has been rejected (or adopted) because of some facts other than informant judgments.'

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