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THE WAY WE WERE;

or;

THE REAL ACTUAL TRUTH ABOUT GENERATIVE SEMANTICS:

A Memoir

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> Infandum, Regina, iubes renovare dolorem
> Troianas ut opes et lamentabile regnum
> eruerint Danai, quaeque ipse miserrima vidi
> et quorum pars magna fui.
>
> Vergil, *Aeneid* II, 3–6

It is now over a dozen years since generative semantics (GS) ceased to be considered, by its practitioners and by the field of linguistics as a whole, a viable theory of language. Perhaps now it is possible to look at its history in a more or less dispassionate way; and evaluate its contributions so as to understand what the enterprise was really about (which would have been impossible at the time, had we even been inclined to reflect upon it).[1]

I am also moved to take computer in hand as a result of reading Newmeyer's (1980) commentary. Further, I have learned in the last few months of several attempts to write histories of GS, by people who must have been scarcely babes in arms at the time it was all going on. All of this moves me to try to get it right, to discourage future distortions of the Newmeyer type and others, perhaps even further from any reality.

Newmeyer's work has been valuable to me in clarifying other perspectives and encouraging me to put some years of thoughts on paper. Newmeyer certainly is to be admired for working through a formidable amount of complex, contradictory, and often ill-expressed prose in an attempt to make sense of fifteen years of theoretical developments in linguistics.

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[1] I would like to thank Dwight Bolinger, Georgia Green, Randy Harris, George Lakoff, and Toshio Ohori for their comments and suggestions.

At the same time, I feel that his work suffers from flaws which detract critically from its utility and belie its author's claim to be presenting a generally dispassionate history. Newmeyer's bias is the most dangerous kind – inexplicit, perhaps not fully recognized even by the author. No one who lived through the period as a participant in the 'linguistics wars' can claim the status of disinterested observer. By his appearance of doing so, Newmeyer misleads the reader and distorts the facts. I believe that, when a historian is in a position to be biased, it is his or her responsibility to discover the form and extent of that subjectivity, and make it clear to the audience.

In this work, whose subjectivity is rampant indeed, I have chosen to lay stress on the extra-theoretical content of the GS/EST debate, since the theoretical arguments have been, and no doubt will be, dissected sufficiently by others in the future. Besides, I have come to realize that theory alone does not make for linguistic schools, much less linguistics wars: To understand the theory itself as well as the history, we have to understand the people.

1. The basic history

> Those who do not remember the past are condemned to repeat it.
> Santayana

Generative semantics is, or was, an offshoot of what Chomsky was later to call the Standard Theory and I like to call Classical Transformational Grammar, or CTG, the model described in *Aspects of the Theory of Syntax* (1965). In that book, Chomsky discussed his model of a two-tiered syntactic component: surface structures (SS), the sentences we observe, with information about the grammatical relations among their parts; and the deep structure (DS). These two levels were linked through a system of transformational (T) rules.[2]

Aspects theory assumed that DSs and Ts would be strictly constrained as to form and function, but not all that much was said about what those constraints were. Examples were given in *Aspects* in the form of 'fragments' of a grammar of English. But what was not specified was what kinds of items could or could not be in the DS; how the DS order related to the surface order of Ss; and what kinds of processes transformations could and could not

[2] Readers should note that the intended audience for this paper is diverse. It was originally written for the first-year graduate students in a proseminar I was teaching, and some of the background information reflects this origin. On the other hand, I suspect that in some cases I have presupposed knowledge on the reader's part of particular points of linguistic theory that are not part of the linguist's current armamentarium. I would hope that readers would try to piece things together, skimming over what they already know, or not worrying too much about what is obscure, in order to get the gist of what I'm trying to say.

effect. It was impossible to tell, either from written documents or from oral statements by the author, the limits on the abstractness of DSs, how universal they were meant to be, and how much Ts could change them to create surface structures.

On the other hand, in his less technical writings, like *Cartesian Linguistics* (1966) and *Language and Mind* (1967), which were being discussed and worked on in the immediate post-*Aspects* period, Chomsky implied a lot in these areas. He talked of DSs as linked to universal human cognitive structures, Ts as windows into the mind. Now if a DS was to have universal implications, it was clear to at least some readers of the whole *oeuvre*, the model set forth in the various grammatical fragments in *Aspects* could not be taken literally, or surely not as an exclusive statement. This version of DS contained, for instance, articles and modal auxiliaries; DS sentences were in an NP–V–NP (SVO) order. In this and numerous other ways, these DSs were English specific. They did not represent semantic 'primes' in any sense, they were syntactic bases, not semantic (as Chomsky himself made clear by making the DS the basis of an autonomous syntactic component, which was 'interpreted' by an autonomous semantic system). The DS was relevant to, but not a part of or incorporating, semantics. Then if the Cartesian universals of language that connected all human beings by virtue of their reason were (as surely seems to be the case) semantic entities, having to do with meaning and reference, this notion of DS was irrelevant – but Chomsky claimed otherwise in his other work.

It's important for this and much subsequent discussion to understand the overwhelming influence Chomsky had on his disciples during this period, an influence both moral and intellectual. He spoke in a soft voice, persuasive it seemed by reasoned argumentation alone: He carried conviction, in part, because he appeared to be above the academic fray, interested only in truth for its own sake, not winning in the doctrinal rivalries that characterized other disciplines.

A significant part of the attraction of CTG, and its meteoric rise, lay in its presentation of a formal linguistic theory, the promise of a complete and rigorous model. It was covertly assumed that all the properties of language could be described by a system that utilized dichotomous choices, as a formal system must. The 'justification' of the idea that a formal system of this kind was adequate for linguistic description mainly consisted of the sorts of 'fragments' of grammars of particular languages (most often English, or anglocentric versions of other languages) found in *Aspects*. While this showed that, for carefully selected cases, a particular formalism *could* work, it didn't show that it could work for the grammar as a whole, much less that it really captured what was going on. The assumption was that the fragments we had in 1965 would, in a couple of years, increase and meet one another to form a complete grammar of English. Students would then take this grammar and apply it to many 'exotic' languages, shortly producing complete grammars for each and all. The distillation of this effort would be, in the not-too-far distant future, an inductively-derived universal grammar which would (undoubtedly) jibe smoothly with the deductive version then being discussed, based on the way language 'had to be' due to the theoretical assumptions of CTG. It was a heady promise and we all believed it wholeheartedly. But at least some of us wanted to be more precise about the details of this grammar than *Aspects* or its author allowed.

Important at this point and later, overtly and implicitly, was the 'paradigm argument' of CTG, the way in which DSs could be justified: both the idea of implicit underlying structure, and particular realizations thereof. To avoid phenomenological criticism about postulating forms that were in some sense interpretively derived, theorists had to offer a rigorous means of deriving the abstract from the concrete, as well as reasons why this potentially dangerous step was justified in terms of the increase in generalizing power of the grammar. These arguments were made elegantly in a 1964 article by Paul Postal. He took an example familiar and comfortable not only to transformational grammarians and structural linguists, but to grammar teachers everywhere: the idea that, intuitively, English imperative sentences were not, at heart, 'subjectless' as they appeared; but, in order to explain what could and could not occur as imperative sentences, one had to 'understand' a second-person subject not ordinarily accessible to superficial observation. Postal's example was inspired, as the presence of a 'you' in our understanding of imperatives was uncontroversial; his leap was to build that intuition into the syntactic grammar, to justify the formal, systematic existence at a specified level of structure of entities whose presence could only be discerned indirectly. He did it by arguing that such an assumption simplified the grammar, allowed its rules to be more general and therefore, overall, fewer in number. So ordinary intuition was linked to formal theoretical 'simplicity'.

His most telling argument was based on the distribution of reflexive pronouns and tags in imperative sentences compared with other types. The basic question concerned the most economical statement of a set of observations. Everyone could agree that the most economical grammar, the one requiring the fewest rules, was to be preferred. Postal discussed a class of cases in which the apparently simplest statement required either a complication in the grammar or, more probably, the inability to state at least one generalization at all. A theory of grammar requiring more complex assumptions and a more abstract grammatical structure permitted an elegant generalization and a simpler overall grammar – and therefore, Postal argued, was to be chosen over its ostensibly-simpler rival.

In general (says Postal) English sentences require overt subjects. But there is one class of sentences which doesn't, imperatives: *Go home, wash yourself.* The simplest analysis of these would be to consider them truly subjectless. This violates an apparent generalization about English sentence structure, but that in itself is not a deadly problem. But consider this:

There exists in English a set of sentences whose direct objects and subjects are coreferential. The direct object noun phrases in such cases are obligatorily replaced by reflexive pronouns: pronouns with *-self/selves* added to them. *I wash I* is replaced by *I wash myself; Mary washes Mary*, by *Mary washes herself*; and so on. As long as the subject is present in the surface structure to trigger reflexivization, there is no problem.

But consider an imperative like *Wash yourself*, which looks like an ordinary reflexive and occurs in non-subject position, but not coreferentially to any superficially observable NP. Besides, only the second person occurs here. How should we handle this? The apparently simplest solution is to treat these as special cases, not true reflexives. But Postal shows that this misses important generalizations and complicates the grammar. Or we might treat imperatives as originally declarative in form, with a second-person subject. The reflexive rule could apply at this point – and could apply only in case the object was second person, of course. Hence only the second person shows up as a reflexive in imperatives. Later, the subject was deleted. Imperatives then work according to the same principle as declaratives (and other sentence types). Postal makes analogous arguments based on tags of imperatives and declaratives.

Postal's argument can be seen as the 'specimen case' (or 'paradigm' in Kuhn's (1962) sense) of classical transformational grammar. By illustration more than by the explicit statement of rules and regulations, it set out the ways in which syntactic arguments could be made, hypotheses justified. Syntactic factors alone were permitted to function in these arguments (at an explicit level, in any event): distribution and cooccurrence of lexical items in sentences. (CTG always and necessarily was a *sentence-level* grammar, with S as the starting point in the grammar's generative capacity and the unit over which generalizations and connections could be stated. So larger and more abstract textual units, e.g. the written paragraph or the conversational turn, were off limits.) But as the specimen case, the article also provided ammunition for expansion of the theory, extension of the notion of DS, just in case distributional arguments rooted in superficially accessible syntactic structure could be offered in justification. At the time this article was published, it is improbable that its author or his co-workers at MIT saw in it the seeds of revolution or heresy: It appeared to be more a propaganda text designed to persuade the outside world of the logic and reasonableness of the still-threatening transformational paradigm, more an act of homage to the Master than a gauntlet flung in challenge.

If we are to understand the history of the next few years, we must confront a major enigma: As of 1965, and even later, we find in the bowels of Building 20 a group of dedicated co-conspirators, united by missionary zeal and shared purpose. A year or two later, the garment is unraveling, and by the end of the decade, the mood is total warfare. The field always was closed off against the outside: No serpent was introduced from outside of Eden to seduce or corrupt. Any dissension had to be home-brewed. Yet, at the time *Aspects* was published, we detect no trace of disaffection, nothing published or (as well as I recall) openly discussed involving opposition to standard theory or proposals of novelty. Where did the split come from? And why did it take so long to come to consciousness?

It is true that as early as 1963, there were proposals made (George Lakoff's memorandum of that year, 'Toward Generative Semantics', being one) that would have extended the model to be proposed in *Aspects*. But these were not seen as expressions of opposition, much less as theoretical heresies. Chomsky, then as ever, tended to be vague about what each version of his theory permitted in terms of abstract structure, what the limits of his model were. What seems retrospectively to have happened is this: There were from the start two kinds of people who went into transformational grammar. Each kind went into it with the belief that Chomsky and his system were valid and worth following – as they stood. But because of the vagueness of Chomsky's published and oral formulations (a brilliant theoretical stroke, first, because it diminished the potential for external opposition; second, because it made less likely the internal factionalization of the field), both sides saw in what they read and heard what they wanted to see. It was a version of the blind men and the elephant, except that the men had perfectly good vision, but the elephant was behind a screen, perhaps less elephant than chimera.

But the parable applies to the situation: the question of the nature of language and therefore the linguistic theory needed to capture it. As I said, two kinds of people entered the field, in those first days.[3] One group were basically mathematicians and logicians, by temperament if not by trade. Their fascination with language was in seeing it as a quasi-mathematical system, in isolation, like the systems of topology or algebra. They were concerned with predictable regularities, patterns that recurred, and the formalisms necessary to capture those generalizations. To show that language functioned in this

[3] This dichotomy, like others, is deceptively sharp. It would be erroneous for example to suggest that the EST'ists (EST = Extended Standard Theory) had no interest in the cognitive aspects of language. This of course was one of Chomsky's motivating concerns, and one of particular interest to Ray Jackendoff among others. Similarly, it would be absurd to suggest that GS'ists were uninterested in formalism: George Lakoff and James McCawley in particular were at pains to develop formal devices and systems and considered these central to GS. I mean rather that each group tended toward their own emphasis, the end toward which they strove. EST was – and its descendants remain – chiefly concerned with the description of language as an autonomous system (which might, significantly, shed light on other, autonomous, human psychological processes); GS moved more and more in the direction of seeing language as the reflex of – and inextricably interconnected with – other human processes, social and cognitive. It is a matter of preferred emphasis more than anything.

way would be to make a deep claim about the logical capacities of the human mind, to give deeper and more rigorous meaning to the Cartesian claim that man was a reasoning animal and that those formal rational capacities were intrinsic to and universal in – that is, provided a definition of – humankind.

If that was one's aim in doing linguistics, certain assumptions were natural. You would tend to search for generalizations, stop with the simpler cases on the grounds that they represented the deeper reality; more complex examples did not necessarily show the system to be wrong, or entail more abstract or more complex versions; but merely were static, interference with the deep patterns based on mathematical logic by surface annoyances – other psychological capacities and incapacities, social involvements: interesting to other kinds of social scientists, maybe, but off-limits to linguists, irrelevant and uninteresting. And just as, for the logician or mathematician, the universe could be fragmented up without distortion into sub-systems, smaller worlds, within which generalizations were more readily accessible, so too language could be seen as a network of autonomous systems: phonology, syntax, semantics, interdependent but not formally interconnected. The rules of syntax did not – could not – mention semantic criteria, and vice versa. Hence, the existence of abstract elements in deep structure could be justified only on the basis of superficial syntactic regularities, not semantic criteria like synonymy.

But another group were, at heart, humanists, with the significant parts of their background in language or literature. For them, transformational theory promised something rather different – something for which they had entered the humanities, only to find that the assumptions and methods of those fields closed the door to their curiosity. For them, the promise was made in Chomsky's statements that language was a window to the mind, a way to enter that black box, to see how people actually worked. To them, being human entailed more than manipulating formal structures. It had to do with how we thought, and avoided thinking; how we got together in groups, and why groups had misunderstandings; language as a logical mode of expression, but 'logical' in the sense of 'reasonable', not 'systematic': a means of encoding all those complexities that produced literature, war, and puns. They read the same articles of Chomsky's as the other group, but read something quite different into them than their author (Chomsky was of course a charter member of the first group) intended – but nothing that could not rationally be derived from what he said explicitly. For them, the *Aspects* model was a sketch of the ideal system, an invitation to go deeper in order to make the language-specific and rather concrete system of *Aspects* into the truly universal, abstract system that would be the 'window into the mind' promised largely in works directed outside the profession, such as *Language and Mind*. For them, a theory and a grammar that would link language with the reality, psychic and social, that it reflected and created, was the point. So more complex and irregular structures were not only interesting, they were the crucial cases. Their complexity gave hints to the complexity of the system under study, showed how all parts were interrelated. If you ignored the hard cases as irrelevant, you would make wrong predictions – and have a pointless and boring theory to boot.

For both groups, then, *Aspects* was just a point on the path, not the end. But its vagueness enabled both groups to envisage different ends and not perceive their essential disagreement. It's important first that this disagreement was never made explicit – indeed, was visible only retrospectively; and that it was not a theory-internal conflict, such as can be resolved in terms of a Kuhnian paradigm shift based on mutually-agreed upon examples and counterexamples. Rather, the disagreement was about the subject-matter of the theory: what it should encompass, what language was. Therefore, no evidence could ever have been devised that would convince one group that the assumptions of the other group were correct. (Individuals could, and did, on occasion, cross theoretical lines. But this presumably occurred because, as individuals, they changed their minds about what they were interested in, not because they determined that their preexisting interests were better served by the other side's model.)

2. The theory

La théorie, c'est bon, mais ça n'empêche pas d'exister.
J.-M. Charcot

I have touched briefly upon the basic tenets of CTG, and the ways in which GS differed, and some reasons why the proponents of each felt as they did and worked as they did. But before we leave the arena of scientific disputation, we should examine more closely the claims of GS. As I see it now, although it seemed then that there were myriad aspects comprising a richly textured conceptual system (as indeed there were), they pattern together into four major claims, each diametrically opposed to some tenet of CTG, and all fitting together into a cohesive whole, all interdependent.

2.1. The Base Component

We have seen that, in some of his writings, Chomsky held forth the promise of a syntactically-rooted Deep Structure that, nevertheless, was based upon the universal rational capacities of the human mind. We have also seen that, as he exemplified the model DS in other works, it could not have functioned in this way. GS theorists, troubled by the inconsistency, resolved to reconcile it (as, indeed, Chomsky did later, with Extended Standard Theory (EST), though in the opposite direction). They took the less-formal Chomsky at his word: The basis of syntax was logical and universal. But then, all traces of English-specific features in the DS, or Base Component as a whole (the latter term unlike the former including the lexicon) had to be eliminated in favor of forms that could fit in agreeably with the underlying structure of any natural language. Problems with the CTG model included:

(a) The lexicon. It was often noted that words in one language did not generally correspond, in meaning or syntactic constraints, to their 'synonyms' in any other. The structures in which a causative verb like *kill* could be inserted in English were not identical to their Japanese counterparts. And, clearly, the more figurative uses of words – *kill time, a killer exam* – would not necessarily transfer from one language to another. One solution might be to assume that, in the basic lexicon, 'complete' surface-structure type words did not exist. Rather, what were found were atomic elements, semantic primes, basic concepts common to all languages, which (more or less language-specifically) could be combined together by transformational-type processes to form the surface words of each language. *Kill* in this theory is decomposable into several primes: *cause, become, not, alive.*

A bigger problem than the composition of individual lexical items was the nature and number of lexical categories at the basic level. In CTG, to the extent that the issue was dealt with, the categories in the base were essentially those found at the surface. Later, this covert assumption was explicitly codified by the descendants of CTG: No categories were permitted in the base that did not exist in the language at the superficial level; and no lexical item could change categories by transformational rules (or any other way).

The first postulate meant that 'abstract' entities of various types, with one or two exceptions, could not exist. Both in *Aspects* and earlier, Chomsky permitted two types of abstract markers. One was the 'dummy' symbol, Δ, used to indicate (as for the underlying subject of an agentless passive) an item that was necessarily deletable because it was semantically not fully specified, and was not needed to function syntactically at a superficial level. For instance, the grammar recognized that the underlying subject of the passive was a NP, and that it was semantically not incompatible with the verb selected from the lexicon. Since the passive transformation removed it from subject position, it was no longer syntactically required, and could be deleted: All relevant semantic information about it could be discerned from the choice of main verb. The other abstract category was more problematic. It included a set of items dictating that specific transformations were to operate on the trees which contained them. Each referred to a category of sentence-types: Imp, Q. Thus, if Q was selected in the DS, in English, the tranformational rule of subject–auxiliary switch was triggered. A late transformational rule deleted these markers; their only surface trace (making them recoverable) was in the transformations they had triggered. It was never really clarified in *Aspects* just what sorts of phenomena could be handled by these mechanisms. By permitting into the DS abstract categories (both the 'dummy' and these categories were chosen from the lexicon, like nouns or verbs), CTG seemed to promise that other abstract categories could be justified, as long as they were recoverable from the sentence's later transformational history and superficial cooccurrence patterns. These, then, functioned as an Open Sesame to GS, particularly combined with Postal's paradigmatic illustration.

GS argued that abstract lexical items parallel to these types existed, as long as they could be justified as suggested above. Postal's discussion of imperatives, his claim that abstractness, and consequent transformational complexity, could be justified as long as it allowed more general statements and a simpler overall grammar, was the basis of the argument. First, the claim was made that the surface categorial assignment of a lexical item did not necessarily reflect its membership in DS; second, that abstract lexical items could be generated in the base, leave traces through transformational operations but be deleted before the surface structure.

So, for instance, G. Lakoff made the argument that the kinds of structures that could function as (say) the objects of the verb *believe* were the same as those that could follow the noun *belief: John believed that bats ate cats, John's belief that bats ate cats*. In both, what followed the word in question had to be an abstract nominal form: **John believed that oranges/*John's belief that oranges*. In the CTG lexicon, two separate lexical entries were required: one, the verb, one, the noun. Clearly, many significant facts about where each could be inserted were repeated, a non-economy and a loss of generality. But if in the lexicon only one of these was listed (say, the verb), then those properties need only be listed once; and in specified syntactic conditions, a later transformational rule would change the verb into the corresponding noun. Since V–N relationships of this kind, and analogous types with other categories, are very prevalent in language, a great deal of duplication would be saved by using this system, a saving that would more than offset the expense of a few extra transformations (since the lexical properties would have to be written in individually for each such lexical item, whereas a single T-rule would apply to a whole class of cases).

Beyond this, just as CTG permitted abstract categories such as the one that included Imp and Q, it might as well (with no unwarranted increase in power) include other types which had no possible superficial representation at all: for instance the logical operators: $\forall$ and $\exists$. In several papers McCawley (e.g. (1969)) argued for the existence of such items as parts of lexical structure, and in so doing made the claim that a level of deep structure which did not permit such abstractions necessarily lost generalizations and was thus logically untenable.

Related to these simplifications of the lexicon was one other: The number of categories to which lexical items were assigned was radically diminished in GS. In CTG, lexical categories in the base were identical to those on the surface in two ways: One, as already noted, a single lexical item could not change its category transformationally. Secondly, the categories themselves were the same: As just stated, no categories were permitted in DS that did not have possible SS representation; and then, those categories that could be observed at the surface, *in toto,* were to be found in the base. For CTG, to find the deep (and surface) category assignment of any item, all one had to do was look it up in any dictionary. A noun is a noun is a noun. A preposition is a preposition. And so on. (Although these constraints are never fully or explicitly stated, they can be extrapolated from Chomsky (1965, 1970, 1972).)

But if, as GS postulated for the reasons already given, lexical items could switch categorial assignment in the course of a sentence's derivational history, this offered the possibility to radically simplify the lexicon. If *some* apparent nouns had to be analyzed as verbs in DS to achieve generalizations, then perhaps it made sense to look at *all* nouns this way: Was there really a need for 'noun' as an underlying category? That was a problem that perplexed GS'ists: Some (e.g. Bach (1968)) argued that even concrete nouns like *table* were really representations of the results of actions or events, that is, states, and therefore verbal at base; others were happy to accept this analysis for abstract nouns but not concrete or proper names. For other lexical categories, falling-together made more sense. George Lakoff (1966) argued that adjectives were but a subclass of verbs. Properties like stativity were distributed across both classes, and a generalization was achieved by considering them as identical. Moreover, it was pointed out in the same discussion that the adjective/verb distinction, so salient in English, was much less so in other languages. If one was working towards a universal base, surely it made sense to admit as part of that base only those lexical categories whose existence could be persuasively demonstrated in *every* language. The 'minor' categories, too, could be combined with others using arguments like these. Many types of adverbs could be seen to function as verbs, as could prepositions and conjunctions. Articles were not present in the lexicon, but transformationally inserted in English as the reflexes of presuppositions. So ultimately only two categories could be justified at a DS level: Nouns and Verbs. The others derived their existence through the operation of transformational rules.

(b) The nonexistence of VP. CTG followed traditional rules of sentence parsing in dividing a sentence basically into two major components: NP (nee 'subject') and VP (or 'predicate'). This division worked well for an English-specific DS in which main verb and direct object were more or less inseparable. But it was obviously less viable for a language in which the verb was followed by subject, then DO (VSO), or any other such possibility. As long as

the opening rule of the phrase structure separated sentences into NP and VP, it had to be non-universal.

Further, GS began to find arguments for a single underlying word-order (VSO) for all languages quite different from the prevailing-SVO order of surface-structure English. Such an order was essential for a theory involving lexical decomposition and prelexical predicate-raising to procuce the complex lexical items of the surface structure. McCawley (1970) argued for VSO at the level of underlying structure on the grounds that it permitted a simplified and more understandable statement for several important transformational rules: various raising operations, *there*-insertion, and passive. The GS'ists argued that the justifications CTG proposed for SVO, and VP in particular, were weak. What was needed to prove the existence of VP was (at least) one transformational rule that explicitly mentioned it, that could not be stated at least as well in some other way, by mentioning some other category (e.g., V alone). No such rule was ever incontrovertibly found, and at least to the GS mind, all the rules that ever mentioned VP were stated at least as well if not more elegantly without it.

(c) Word-order. In CTG, the (again covert) assumption was that the word-order postulated for a language in the DS should be as close as possible to the normal or prevalent word-order in surface structure. (In general, CTG preferred a rather spare transformational component, with as few rules as possible, working on as few sentence types as possible.) So in the DS, English was represented in *Aspects* as SVO, since that is the normal order of the declarative, assumed to be the 'basic' sentence type on statistical grounds. (Covert again.) But in other languages (as Greenberg (1966) had demonstrated), there was reason to believe that at a superficial level, many other either possible or mandatory word-orders occurred, and CTG would treat each of these as having a different DS word-order (that is, if the prevailing order in surface declaratives was VSO, that would be taken as the base order, and so on). One problem CTG couldn't cope with at all was so-called 'free word-order' languages, where, as we would say now, word order was governed pragmatically rather than semantically. (Latin is an excellent case.) In CTG, such a language could be dealt with only very artificially and ad hocly: The most 'prevalent' surface order was decreed to be the 'basic' order, and generated in the base – e.g., in Latin, SOV. But the 'basicness' of SOV in Latin was of a very different status from the 'basicness' of SVO in English, since word-order changes in the former did not affect semantic reference, but rather pragmatic function: vividness, topic, cohesion, etc. More troubling than the false analysis itself is the fact that CTG theory did not provide a way to preclude it. A favorite validation for formalism is that it provides a 'garbage detector' for incorrect analyses. This one didn't.

The postulation of a universal basic VSO order had more than aesthetic justification. McCawley argued that it was not coincidental that VSO was analogous to the order of items in the propositions of symbolic logic, where V = predicate and N (that is, S and O) = arguments. GS suggested that the underlying semantic order of natural language propositions was to be equated with that of propositional logic, and that the universality of VSO, in a truly Cartesian sense, rested on its logical structure.

Additionally, VSO was argued for via the familiar Postalian paradigm: It allowed for the simplification of the transformational component. McCawley suggested that a number of important transformational rules could be radically simplified if they operated on a structure of VSO form. For instance, CTG assumed two rules of raising: to subject and to object. Intuitively the processes seemed similar, but if the rules operated on SVO structures, they could not be handled by a single rule, since the raised NPs had to move in opposite directions. But if the subject and direct object were on the same side of the verb, the two rules could be compressed to one, moving the subject out of the leftmost NP of two, or the only NP in an intransitive sentence. Thus the SD of both forms of raising was greatly simplified, and the two were related formally as well as intuitively. Other rules that could be significantly simplified and made more intuitive were neg-raising, passivization, and *there*-insertion.

The result of the foregoing claims was an underlying structure quite different from the CTG DS tree – and, at the same time, from the GS or CTG surface tree. CTG's trees – at both levels – were horizontally expansive: lots of categories, lots of divisions into separate phrases: NP, PP, AdvP, and so on. Because these categories so closely replicated the SS ones, the transformational component had relatively little work to do: It didn't radically alter the shape of trees, just reorganized constituents. But the transformational component of GS did a great deal of constructive work. Even at the DS level, prelexical T-rules put atomic predicates (e.g., [cause], [not], [alive]) together to form surface-type lexical items (e.g., *kill*). And T-rules had to convert verbs into nouns, prepositions into verbs, and so on. They had to delete many types of abstract items (performatives, for instance). But of course, the GS surface tree was the same as the TG model. So, for GS, transformations had to convert a much skinnier, underlying tree to its horizontally-spreading SS counterpart. One Christmas, Haj Ross's students at MIT gave him a mobile made of wire hangers, its nodes represented by Christmas tree balls, representing the GS version of the DS of *Floyd broke the glass*. In CTG, the tree would have been quite simple, as exemplified in figure 1.

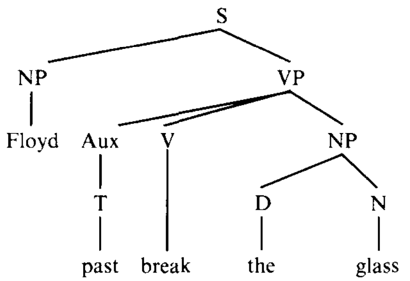

Fig. 1. The CTG DS Tree for *Floyd broke the glass.*

But the GS tree-mobile stretched from the ceiling to the floor of the office in Building 20, and this was rather early in the history of GS: By the time it was over, it would have been necessary to break through to the floor below, as figure 2 shows.

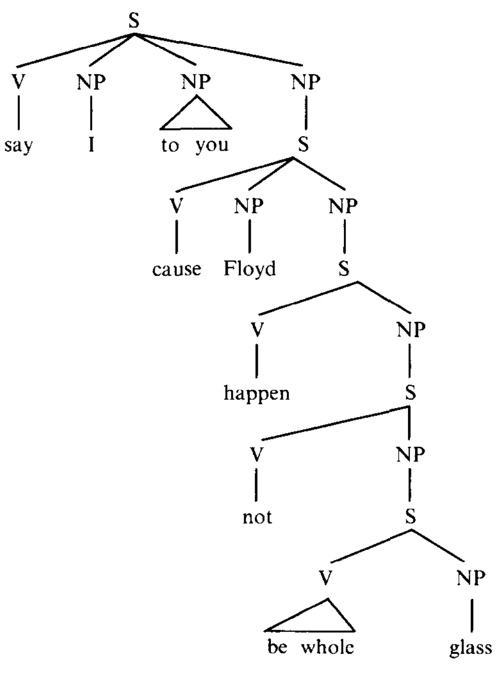

Fig. 2. One version of GS Underlying Structure for *Floyd broke the glass.*

A GS derivation, then, would involve a very much simpler PS component (there might have been no more than three expansion rules: $S \rightarrow V + NP$; $NP \rightarrow N$; $NP \rightarrow S$); but in any derivation, there would be more applications of specific transformational rules, as well as more kinds of transformational rules: This was, in fact, a considerably more powerful system. The dispute was not over relative power, but over its necessity, with CTG claiming its system was sufficient to cover all that its proponents required of a grammar, and GS affirming the same of its. But, as we have seen, the argument existed because each side had a very different notion about what the grammar ought to do, which in turn went back to a covert dispute about the nature of what the grammar was supposed to describe – language.

2.2. The power of transformations

From the beginning of the schism, one of the favorite insults the traditionalists could fling at the rebels was to accuse their grammar of being 'too powerful'. The proper riposte was that the former's grammar was insufficiently powerful; or that the GS version was, in fact, no more powerful than its competitor, if fully understood. Assuming the validity of formalism itself, the criticism was serious if justified: Under the assumption of Occam's Razor, the best theory is the one which can accomplish all that is necessary with the least amount of machinery. A too-powerful theory is one whose mechanisms allow it to do more than is actually needed, and is therefore uneconomical. Hence, it was important that transformations be limited both in number and in type: The fewest possible rules, and the fewest possible kinds of rules, made for the simplest system.

The problem with this assumption, said GS, is that it is difficult or impossible to gauge simplicity over the grammar as a whole. Maybe you can count rule-applications, or features, in fragments of the grammar, but until you have a complete grammar of a language (by this time, recognized as not an imminent possibility), you cannot talk sensibly of economy or power. What might look like an unnecessary efflorescence in the transformational component when the latter is considered by itself might arguably effect significant savings overall, by radically simplifying the base component. Indeed, this is just what was argued.

It is true that in GS, the transformational component did a good deal more work. It did so most obviously because the DS trees needed more processing by the same rules, cyclically iterated level by level, than did their equivalents in CTG. (But it was by no means clear that merely having rules apply more often made for non-simplicity of an interesting kind.) Then too, GS introduced new rules into the grammar: There were prelexical transformations, for example, which combined the atomic predicates of the lexicon into the nouns and verbs of the surface structure. There were rules deleting abstract elements such as the higher performative. There were global rules. Additionally GS developed a rich stratum of constraints on the applicability of rules, of which the profusion of cyclical types is but one instance. There were Ross's islands

(1967); notions of precedence and command (Langacker (1969)); governed rules and major and minor rules (G. Lakoff (1970)); transderivational constraints; and much, much more. There were category-changing rules, turning verbs into abstract nouns. There were insertion rules, for instance those adding articles based on presuppositional information. But, GS'ists argued, even this did not really complicate the grammar significantly, since these rules, while new, were of the same *types* as already-extant processes: They utilized the same familiar elementary operations of deletion, insertion, permutation and substitution. There were some wholly new processes, to be sure: precyclical and postcyclical rule application, for instance. But it was argued that any theory of generative grammar would require them.

But the way GS argued most strongly against the criticism of too great power was that, in fact, overall GS was economical. The proliferation of the transformational component was more than compensated for by the austerity of the base, the paring of both the lexicon and the phrase structure rules. And more: the postulation of VSO, with its consequent simplification of key rules, provided a diminution of power in the T-rules. In CTG, individual T's could operate quite unconstrainedly: Permutations might occur over a wide swath of structure, as was the case for both passivization and raising. GS suggested that the operation of T's be constrained: that operations only take place between contiguous items in an SD. So GS restricted the power of T's by confining their operations on P-markers; while CTG did so by restricting the forms the rules themselves could take.

2.3. Pragmantax vs. autonomy

That leads directly to the next point of difference. I have already noted that CTG and EST were predicated on a notion of autonomous language, as well as autonomous levels of grammar. In a way there is an irony here. It is well known that CTG took pains to distinguish itself from structuralism (the 'Bad Guys') on the phonological level by rejecting the latter's maintenance of a level of autonomous phonemics.[4] (No such effort was made for syntax, largely because structuralists generally left syntax alone.) In general, said CTG, levels are non-autonomous, systems are interconnected. But in fact, CTG (to the extent that it troubled itself with semantics at all) saw semantics as separate from syntax. Like phonology, it interpreted the output of the syntactic component, but did not interact with it.

[4] It is curious that each side saw the other as relapsing into Bloomfieldian heresy. While the GS'ists saw the ESt'ists as neo-crypto-Bloomfieldians because of their non-universalist Deep Structure, their belief in the autonomy of levels, and their rejection of true mentalism, EST saw GS as similarly benighted because of their 'empiricism' – a dirty word in the Cartesian circles of CTG/EST, albeit a curious charge against a group who were at least as introspective and intuitive in their methods as they were themselves. (Cf. Katz and Bever (1976).)

One of the earliest and strongest thrusts of GS was its persistence in connecting levels, insisting that there was no logical reason to have an autonomous syntax – indeed, that such a level was logically and formally untenable. McCawley (1969) argued that the *Aspects* DS itself was logically untenable, and that, therefore, syntactic mechanisms must be linked directly with semantic entities. Others gave examples showing that speakers' assumptions about reality, or the context in which sentences were uttered, crucially affected their syntactic form; presuppositions, formal and otherwise, were necessary to predict grammaticality (e.g. R. Lakoff (1969, 1970)). Indeed, the purely syntactic */ of CTG was already in deep trouble (as we shall see in section 2.5.); what GS showed was that the trouble arose because extra-syntactic context had to be taken into account, and that the issue was appropriateness rather than pure grammaticality. The choice of *some* or *any* was not determined only by syntactic context (presence of a Neg or Q), but as much by a speaker's assumptions about the proposition: Was it good or bad; likely or unlikely? The outside world was starting to impinge.

GS first gave to the impingement, or interaction, the semi-frivolous name 'semantax' suggesting the indivisibility of levels. Later, as pragmatic theory developed and was incorporated, the name transmogrified into 'pragmantax'. Ultimately formal means were developed to bring to the 'sentence' of TG the idea that it did not exist in a communicative vacuum, but was informed by the fact that it was uttered by one speaker to another, in a particular place, at a particular time. Speech act theory, as devoloped by ordinary language philosopher J.L. Austin (1962), proved the magic link between syntax and pragmatics, with Ross's 'On declarative sentences' (1970) providing a way to incorporate the real-world speech act into the syntactic form via the incorporation of the performative abstract verb into the sentence as its highest unit. Ross devised arguments to justify the postulation of abstract declarative verbs by the usual syntactic methods: distribution and cooccurrence. In fact, his arguments can be seen as no more than an extension and refinement of Postal's paradigm case – transformational orthodoxy.

Later, arguments were made to incorporate more of pragmatics – the interactional and contextual component – into syntax, or rather, to make the two, plus semantics, indistinguishable. Just as natural logic had been made a part of syntax, or semantax, in the late '60s, with arguments demonstrating the necessity for incorporating the propositional structure and quantifiers of symbolic logic into syntax, so conversational logic was brought in, using Grice's (1975) theory of implicature. Speech act theory, introduced into generative syntax through Ross's performative arguments, was further refined by Gordon and Lakoff's (1971) conversational postulates, allowing a formal representation of indirect illocutionary force. A theory of politeness was connected with these, suggesting a link between syntactic form, pragmatic intention, and discourse context. From there the theory went in many directions: discourse type and structure; contextual influence on syntax (involving the influence of personal and psychological characteristics of participants on language form); and special contextual functions, like the courtroom and therapeutic discourse; and functional grammar, the explicit discussion of the way in which function governs form. The problem, of course, is that all of these made syntax much more complex: The possibility of one-to-one relationships, simple predictions like selectional restrictions, became unthinkable. While much insight was gained, the development of a complete, rigorous and explicit theory was rendered highly improbable, now and perhaps forever.[5]

2.4. The strength of the Katz-Postal hypothesis

Let us return, gratefully, to an issue more bound to language-internal syntax, the Katz-Postal hypothesis (K-P). This theoretical claim goes back to 1964 with Jerrold Katz and Paul Postal's monograph, *An Integrated Theory of Linguistic Descriptions*. In it, the authors had a serious problem to solve with pre-*Aspects* transformational syntax and semantics. In this early form of the theory, transformations were not necessarily meaning-preserving. Transformations could, for instance, insert meaningful elements: a negative, a question. Therefore, semantic interpretation had to be applicable to derivations at two levels: before and after the transformations had applied. Not only was this uneconomical, it led to some rather unpleasant results, possibilities for self-contradictions, nondeterminacy of meanings, etc. So Katz and Postal proposed that the theory be reformulated so that transformations could not change meaning. The meaning given at the basic level (soon to be christened 'deep structure') was the meaning to be found at the surface. Therefore the semantic ('projection') rules needed to apply only once, at the deep level. But to

[5] Newmeyer (1980: 168) criticizes GS practice as unnecessarily fuzzy:
"This 'exuberant cataloguing of ... facts' became a hallmark of generative semantics, as every counterexample to a claim (real or apparent) was greeted as an excuse to broaden still further the domain of formal grammar. The data fetishism reached its apogee in fuzzy grammar. Many staunch generative semanticists who had followed every step of Lakoff's and Ross' up to that point turned away from fuzzy theoretical constructs. 'Of course there's a squish', they objected. 'There's always a squish. It's the nature of data to be squishy. And it's the purpose of theory to extract order from squishy data.'"
Yes. But 'order' is not synonymous with 'discreteness', particularly when imposing the latter creates distortions. In that case, it is the job of the responsible investigator to divest his or her mind of outmoded beliefs in what 'science' or 'theory' must be (according to whom) and tailor explanation to observations, not vice versa. Granted that fuzziness is unsettling, even frightening: that is no reason to deny its reality. To abjure non-discrete theories because they are unsettling, or because they conflict with the kinds of formalisms we currently feel comfortable with, is antiscientific in the most dangerous way: analogous to the Church's determination that Galileo's claims were heretical because they were antithetical to current established wisdom.

accomplish this desideratum, transformations had to be stated so that they did not change meanings. If, for instance, a sentence was negative at the surface, the negative element had to be introduced at the underlying level. If an S was to be interrogative, a Q marker had to be present in the base, conditioning the application of subject–auxiliary inversion later in the transformational cycle.

This was a considerable simplification, but from the start had problems of its own. Chomsky (1965) noted, for instance, that one of his star transformations, passivization, ran into difficulty as a result of Katz-Postal. It was a tenet of CTG that passive sentences were transformational equivalents of actives – indeed, this had been a primary selling point for the theory, that it could relate these types so elegantly. But it was noted that pairs existed that were not (it was claimed) truly equivalent in meaning. The application of passivization appeared to change meaning:

(1) Everyone in the room speaks two languages.
(2) Two languages are spoken by everyone in the room.

As a result, passivization, which had been cast as an optional rule operating on any transitive active sentence, was recast as obligatorily operating, but only on structures that had some sort of triggering device built into them. The solution destroyed some of the elegance of the original formulation and introduced some distinctly ad hoc elements, but it allowed the preservation of Katz-Postal (without which *Aspects* CTG could not function).

But more problems emerged. One was that 'meaning' was imperfectly defined. Under what conditions were two sentences said to be paraphrases – that is, having the same meaning? When did a derivation change meaning, and when did it not? And if it was doctrine that transformations did not change meaning, was that to be taken to imply that, if two surface structures were equivalent in meaning, they must (the inverse of K-P) be taken to have a common underlying source? This was the strongest interpretation of Katz-Postal, and GS essentially adopted it, without too much explicit consideration. The argument was that it was always in the interest of economy to derive paraphrases from the same underlying source: Thus, selectional restrictions and other constraints need be stated only once in the base. Then, if two sentences were arguably paraphrases (which was defined as, if neither could be true in a context where the other was false), Occam's Razor required and K-P allowed that they share a common source. But what was a true and complete paraphrase? Was (3) accurately paraphrased by (4)?

(3) John sliced the salami with a knife.
(4) John used a knife to slice the salami.

Was (5) paraphrased by (6)?

(5) John killed Bill.
(6) John caused Bill to die.

These issues turned out to be unresolvable, with GS saying 'yes' and CTG/ EST 'no', with no agreed-upon way to decide.[6]

Moreover, the old active/passive bugbear was understood very differently by GS and EST. The latter (as discussed by Partee (1971) among others) kept to their guns: It demonstrated a deep problem with K-P; and the introduction in EST of interpretive semantics, essentially the reintroduction of pre-*Aspects* projection rules operating on surface structures, made K-P unnecessary or untenable in many cases. GS, on the other hand, saw the problematic examples as special cases that did not cast doubt on K-P, but only on the CTG statement of passive and its lexical theory, as well as its difficulties in dealing with multiple meaning and non-dichotomy. The problem with the active–passive pair given above is not with active–passive *per se*; the transformation is involved in the problem only in that it moves subject NPs and direct object NPs over each other. When quantifiers cross this way (as argued by Postal (1971)), changes of meaning may occur, unrelated to the transformational operation itself. Only where quantifiers play the roles of subject and object do passives display this meaning change, so it is not characteristic of passivization itself, and therefore, K-P does not fail with passivization. Moreover, it might also be argued, CTG suggested that the two sentences in question had distinct and different meanings, which would cause trouble for K-P. Rather, both sentences have the possibility for both meanings; but each tends to favor one meaning over the other. Thus, (1) has (in isolation – always a tricky criterion) the primary meaning 'Each person speaks two languages, but they could be any two'; (2) most likely means 'Two languages (the same two) are spoken by all the people'. Within autonomous syntax, the meaning preference must remain mysterious. If, however, we introduce pragmatic, textual, or functional considerations, things get clearer. There is a tendency in English, other things being equal, to use the subject position for topics, for focal points. So (1), *Everyone in the room* ..., suggests, 'I'm talking about these

[6] The Bach-Peters Paradox was an additional complication, arguably making things worse for both sides. The problem, as it was originally propounded by Bach (1970) was the existence of surface structures with apparently infinite underlying structures, which therefore could not be represented either as GS logical structures or EST DS's, e.g.

(i) The man_i who deserves it_j will get the $prize_j$ he_i wants.

If (as GS argued) SS pronouns represented underlying full (and concrete) NP's, there was no way this example could be completely accounted for. But EST ran into the same problem, though it might transfer the problem of the representation of the full NP's to the level of surface structure, via a Semantic Interpretation Rule (SIR).

people, my emphasis is on them', and there is no reason to think that the languages themselves are being stressed and pointed out. But in the passive case, the only reason (in isolation again) why the sentence is passivized (a marked construction) is that we are being asked to focus our attention on the languages themselves. It is a great deal easier to imagine doing this in the case of specific languages that are under discussion, rather than the vague reference point of languages in general. So functional considerations and discourse expectations motivate one interpretation over the other, and even so, there is no 100% correlation, as would be expected if the distinction were based on syntax – that is, on the operation of passivization. So passivization doesn't pose a problem for K-P, except within a theory of autonomous syntax.

This doesn't resolve the issue about the strength of K-P; but again, this cannot be resolved theory-externally. If you believe in autonomous syntax and semantic interpretation of surface structures, then a weak or nonexistent version of K-P works best for you; if you believe in a semantics or pragmatics-driven syntax, then paraphrase relations are deep and important, and K-P must be maintained.

2.5 Continuum vs. dichotomy

CTG saw language as a candidate for formal description, and therefore, as a system whose components could be assigned to either-or categories (Noun/Verb; grammatical/ungrammatical; transitive/intransitive; count/mass; obligatory/optional, and so on). GS at first fell in with this system, but as its proponents started looking at more complex sentences and more intricate relationships, it became gradually clearer and clearer that such a systematicity, however attractive because easily formalizable and readily organized, would provide a distorted view of language. In particular, Ross, in a series of papers (e.g. (1972, 1973)), talked about 'squishes': cases where one category flowed into another, where phenomena were best organized as continua, rather than dichotomies. There was a continuum going from 'verb' to 'noun', rather than an item being always and unambiguously assignable to one category or the other. It was probably as well that this understanding came late in its history, as it would have made any formal statements profoundly more difficult (and perhaps was partly behind the ultimate despair): Transformations, after all, necessarily mention discrete and dichotomous categories, and no alternative system was offered.

In a sense, the continuum problem was the first thorn in the flesh of CTG, but it existed mainly as an unseen irritant until suite late in the history of the field. The problem surfaced first with the redefinition of grammaticality as a subcase of appropriateness or acceptability. The first examples to be discussed – as is necessary in the development of theories – were unambiguous in this respect. *John admires sincerity* was fully grammatical; **Sincerity admires John,* fully ungrammatical, and everyone was in complete agreement on these judgments.

Over several years, disturbing examples turned up as people began looking at more complex rules, more intricate constructions. Often there was real uncertainty in an investigator's mind over the assignment of an asterisk to an example. Recall, too, that CTG's methodology was that of ordinary-language philosophy: The notion of testing examples on large groups of informants (or even – perish forbid! – getting examples from real, spontaneous data) was unheard-of. So one linguist's intuitive judgment was equal to another's, and there was no way to discriminate. "That's not in my dialect", you could say to a colleague, but that didn't obligate him to change his mind. Hence Ross's version of the Linguist's National Anthem: "Oh, see if you can say ...".

As time went on, things got worse. Not only was an example apt to be judged differently by different people with different theoretical positions; but it became clear that the simple grammatical/ungrammatical distinction was an oversimplification. Rather, sentences were strung along a continuum, from the unquestionably-grammatical to the out-and-out salad; from *John admires sincerity* through *Sincerity admires John* to *Admire John sincerity* or *Run a afterward toast*. Sentences ran the gamut from the totally unmarked through those marked ?, meaning that the linguist him/herself was wavering; ?*, wavering in the direction of ungrammaticality; ??, not only the linguist but several friends and relations were uncertain; **, not only bad but absolute, positive garbage, and so on. Later others were added: %, for 'dialectal' in the peculiar CTG sense: Some of us like it, and some don't (where ? = all of us are unsure); ✡ Yiddish-dialectal, e.g. for Y-movement, as in *trouble you want, trouble you'll get*. Until quite late, though, the problem was not recognized as intrinsic to, and highly damaging to, the formal superstructure of CTG. It fell into the category of observer's weakness: the linguist's personal fault. That context of various types was the crucial factor was of course ignored.

Other continua began, sooner or later, to emerge from the woodwork growling ominously. Early on, Ross's (1969) 'Auxiliaries as main verbs' paper made the point that an essential (and oft-lauded as brilliant) dichotomy of Chomsky's, Aux vs. V, led to wrong predictions. Rather, an Aux *was* a verb, but with special properties. And there were many intermediate points on the Aux-to-V scale: if a modal like *can* is a pure Aux (a dangerous assumption, of course), then *ought* is less of one in one way, and *need* and *dare* in another; and *have* in still another; transitive verbs are closer to Auxes than are intransitive; verbs taking *to* as a complementizer, closer to Auxes than those taking *that*, and among those taking *to*, those which can or must delete it (like *let* or *make*) Auxier (as we began to phrase it) than those which cannot. And so on. This perspective made the writing of either-or, all-or-nothing transformational rules and environment statements seem more and more an exercise in fantasy or futility. Figure 3 lists a few of the recognized continua, from the theory-internal to the meta-theoretical. (For more on this, cf. R. Lakoff (1982).)

I. *Grammar-internal false dichotomies*
grammatical — ungrammatical
unambiguous — ambiguous
category A — category B (e.g., Noun — Verb; Verb — Aux)
transitive verb — intransitive verb
word — sentence
creative product — rote product

II. *Theory-internal false dichotomies*
rule fully applicable to S — rule inapplicable to S
syntax (autonomous) — semantics, pragmatics (etc.)
Meaning A — Meaning B (for quantifier-crossing cases, e.g.)
performance — competence

III. *Theory-extrinsic false dichotomies*
linguistics-internal — real world (psychology, anthropology)
I/you win — I/you lose (argumentation style)

Fig. 3. Continua.

Of all the innovations of GS, continuousness, while the first to be observed, was the last to receive official and explicit discussion; was perhaps the only one (maybe because it came late) not to be vigorously resisted by CTG/EST; and in all likelihood will stand as the most significant contribution of GS to human understanding of language, after all these disputations are forgotten and the disputers in their graves. For one thing, all of the other points of argument are in one way or another based on the continuum/dichotomy split; for another, our being forced to see language as crucially consisting of uncertainties, imprecisions, and indiscretenesses must ultimately cause us to rethink our hopes for formalizing linguistics and for assigning it to the realm of 'science'. Gazdar and Klein (1978) take the opposite tack. They argue that, because no current mathematical theory can deal with Ross's continua, the latter should be discarded as a theoretical concept. The thinking is oddly reminiscent of that of the Church in its confrontation with Galileo: If the current conventional wisdom, or scientific method, is in disagreement with the facts, deny the facts. *Eppur si muove.*

Figure 3 may be helpful as a summary of the positions of the two sides.[7] Some of the listings are a trifle frivolous, but the whole diagram ought to suggest that *any* kind of all-or-nothing viewpoint, whether it is brought to

[7] The immediately preceding statement should, of course, be taken cautiously (cf. fn. 3 above). In fact, there were many intermediate positions, between the far left of GS and the far right of EST. We might think in this connection of Paul and Carol Kiparsky, Susumo Kuno, David Perlmutter, and perhaps Barbara Partee and Charles Fillmore as centrists of diverse kinds.

bear on grammaticality judgments or modes of argumentation, is damaging to a field and its practitioners.

The operative assumptions defining generative semantics were these: universality, natural logic, abstractness, continuousness. Each was a gauntlet in the face of CTG.

3. Behind the rift

Facts are stupid things.
Ronald Reagan

By now it should be clear that the theoretical assertions of GS would be problematic to those preferring a CTG world-view. But this does not explain the bitter emotional discord nor the rancor with which cross-theory arguments were carried out, nor does it shed light on another important difference between the schools: the personality and stylistic differences between their proponents. Both of these issues (the bitterness and the stylistic clash) are traditionally ignored by historians of the field, or at best dismissed without close examination or understanding as irrelevant to the 'science' itself, the theoretical and doctrinal division. I would argue on the other hand that without an understanding of the differences between the kinds of people in each group – both in the sense of their bitter divisiveness, and their personality differences – we cannot really fathom what the two theories were about. Theoretical models, after all, are devised by human beings to meet some emotional and aesthetic needs of their own, conscious or not. We have seen that the two sides went into linguistics with two utterly different agendas, and to a significant degree these agendas reflected their personalities and both personalities and agendas were in turn reflected in scientific styles: The kind of people they were governed the way they made science, which in turn affected the way they wrote up their work. Science, I would argue – if the linguistics of my generation is any example – does not consist of theories and data alone: Those are the bones of the dinosaurs, but from their bones we know altogether too little about the creatures, don't really know how they lived and worked. In the same way, their theories are what scientists leave behind of their work, accessible to outside inspection. But it is their approach, their passion, that informs and enlivens the theories – and, ultimately, accounts for the success or failure of the theory, as much as any bloodless assessment of 'right' and 'wrong'. So I want to reminisce at some length about these two areas, recollections certainly tinged by passion and by no means 'objective'. But part of what I am arguing is that in this field and perhaps all fields, pure objectivity is a myth. If we think we are objective, we are deluded, disengaged, or dead. The best we can do is acknowledge to ourselves and others our bias and subjectivity, and try to be as reasonable as we can despite it. Newmeyer makes the 'objectivity' error in two dangerous ways – he assumes that he usually is objective, and that he should be – so let me try to do better here. I will speak first of the style of argumentation between CTG and GS, the reasons for it, and the results; and then, I will talk about the personal styles of the two groups, and the consequences for their theories and for linguistics, with some thoughts on how to do 'science' of this kind, or perhaps better, how not to.

One reason for the bitterness of the fight that eventually erupted lay in the origin of CTG as a union of empathic souls, like-minded thinkers fiercely arrayed against a common enemy. The sense of camaraderie was there from the start, in TG's 'us against the world' format. It should be noted as well that the earliest adherents tended to be people who had tried other fields and found them, or been found by them to be, wanting. They were outcasts looking for a group to belong to and be accepted by, something larger than themselves to which they could make a meaningful contribution. In other ages they might have chosen the Church or the cloister, only to be expelled for heresy and found a nonconformist group of their own. We find, then, an unusual group: unusually close-knit, since until the late '60s virtually all had spent significant time at MIT officially or otherwise, and all felt they owed an allegiance deeper than professional connection to Chomsky – it verged on worship; unusually committed – they were unwilling to stay in fields that didn't promise to make a difference, allow them to do something important. They were, then, ambitious, as well, but at the same time willing to run risks, as they were willing to break away from careers in established fields. When they joined the movement, therefore, they tended to be older than beginning graduate students ordinarily would be, with pre-developed ideas and personalities. And this suggests too that they were unusually strong-minded, even abrasive: They could and did tell their superiors in established branches of linguistics and other fields that the new work they were leaving to do was the 'real thing'; that the old stuff was not worth staying around for or committing your life to. They didn't all say it directly in so many words, but by leaving they said it. By breaking those ties, professional and personal, with their own past and the history of their field they effectively isolated themselves, making all the more crucial the relationships that were to be forged within the new group: It was to be family, world, church.

So the rise of dissension around 1965 was unwelcome and frightening, not unlike the parents' divorce to a child. It was different, of course, in that they themselves played a pivotal role in the disagreement – and so each side tended to blame the other as the starter of the fight. Then not only did each side have its sense of its own intellectual rightness and the other's wrongness to drive them apart, but the additional rancor based on the feeling that *they* had broken up an idyllic family.

There was added to the undercurrent one more ingredient that made the eventual fight nastier than it perhaps needed to have been. Any academic field, over its history, develops or borrows the means to defend itself: evolves, if you will, teeth and claws. In the humanities, which at that time would have been taken by many if not most linguists to encompass their field, the notion of argumentation was scholarly and gentlemanly. Unlike the hard sciences, the humanities did not have (as they still, for the most part, do not have) the concept of work being done by competing paradigms, one of which wins out over the others by dint of demonstrated superiority. Rather, one achieved repute by demonstrating a fine aesthetic sense, good judgment, the ability to see many facets of a work, to digest all that had previously been learned about it and add to that — not overturn it. So it didn't make sense to engage in brawls about who was the best: It was in bad taste, and there was no way to determine the 'right' idea in any case: It didn't even make sense.

In the 'hard' or physical sciences, on the other hand, there was such a criterion: The 'best' theory was the one that most economically accounted for the observed data. Both what was 'economical' and what was 'data' were givens: Everyone in the field could be counted on to agree, at least in principle. One theory could and in time probably would destroy another: Patricide and fratricide were daily events, necessary parts of progress. But at least according to the mythos of the sciences, these debates were solved by the data, external to the investigators and equally accessible to all: It was impersonal and objective, it came out of microscopes and telescopes, not one's own mind. (Of course, occasional leaks about the way it really is in science, such as the autobiographical writings of James Watson, should encourage us to view this myth with a little skepticism; but there was a truth to it: It was a feasible position because the data were objectively verifiable and competing positions could be tested in replicable ways.)

The social sciences have always held a problematic place in human knowledge. As ways of understanding reality, they came last, after the humanities and the physical sciences. Their very name seems an attempt to filch some of the glory that the latter fields had achieved. Why 'social sciences' rather than 'social humanities'? (I mean, of course, not just the name but the associated method.) Their proponents will claim, naturally, that the 'scientific' aspects of the fields are extant and valid: quantification, hypothesis-formation, falsification, replication, lots of impressive-sounding -ations. The question is what they tell us, what we are enabled to know with certainty as a result of these methods. What we note in all, over the last three-quarters of a century or so, is steady factionalization, into smaller or different, competing, often acrimonious fields. Psychology begets sociology; anthropology begets linguistics; linguistics splits into infinite subfields, hyphenated and otherwise. Each generation has its own paradigm, or many, its own revolution, or many; in each case, at least judging from the perspective of my own field, the new one appears to conquer because it elegantly handles data that the old one could not, or did not address itself to. But always overlooked is the fact that the old one elegantly did things that the new one cannot, or disdains to do. There seems to be no theory remotely capable of rigorously making order of all the richness of the data, and one chooses one's theory partly according to where one goes to school and what generation one belongs to, and partly, as I suggested earlier, based on one's personal preference about what issues were interesting, central, and crucial.

In other words, my feeling at this juncture is that there is no hard evidence that the social sciences really are 'sciences' rather than other modes of organized knowledge-acquisition masquerading in the garments of science, but no more science than I would be Einstein if I put on an Einstein mask and talked with a German accent. One can imitate science – as astrology does in a somewhat different way – by insisting on its surface features: avoidance of 'mentalism'; quantification; formalism; discreteness of categories. But the results do not resemble those of science, nor does the behavior of social scientists resemble the ways in which the participants in those fields behave. It may be that we are taking the image of 'science' based, after all, ultimately upon astronomy as the first modern science, and basing our behavior as linguists upon that. But this might be a fallacy. Astronomy worked according to its rules – as originally postulated by the likes of Ptolemy, Galileo, Copernicus – and worked well as a predictive model because it was focused on objects totally remote from ourselves, not in any direct way connected to us. That disconnection allowed the kinds of observations that in turn permitted the quantification and discreteness that underlie the physical sciences and allow them to produce their profound results. But when observation of necessity turns inward, when the investigator in one way or another must be the instrument that evaluates the data, or at least some human mind must gauge the meaning of products of some human mind, then those methods become less reliable. When the data themselves do not come in finite, discrete and unambiguous format; but everything is relative to its context, and context itself is highly subjective, and these are basic realities about language use, crucial parts of the structure, not annoying encumbrances – then the methods that work so well for other kinds of data cannot be automatically appropriated for these new ones. It may be that they could yet be shown to be equally valid, though probably with profound modifications. But the social sciences have adopted the methods as unthinkingly as they did the name; and the fact that they get 'results' thereby doesn't mean they are correct or meaningful results. Of all the social sciences I fear linguistics is the most culpable here, since it depends the most crucially on the kind of data I am talking about – the artifacts of the mind.[8]

[8] It may be useful here to recognize a distinction between 'doing science' and 'playing science' – as children play Doctor, or House. Doing science entails utilizing scientific method because it has

Linguistics, like the other social sciences, assumes that it can and should operate via the 'scientific method' developed for the physical sciences since the fifteenth century; assumes that it is both possible and desirable to treat the artifacts of language, which are discoverable only through the use of the investigator's mind as an instrument (that is, partially or wholly through the use of intuition), as though they were molecules or stars. But some residual doubt remains in the social sciences about whether this assumption is reasonable; the problem is that the doubt remains repressed, covert. It seems to me the question should be asked and investigated, or we should admit we cannot do so, leave the question open, and call our various disciplines by some other blanket term than 'social science'. Otherwise, as psychoanalysis (another field subject to the same sorts of uncertainties, for many of the same reasons) would argue, the repressed fear is apt to lead to distressing superficial behavior. For instance, philosophers of science, taking the unquestioned physical sciences as their model, have proposed definitions of 'science' meant to generalize about what links these fields conceptually. For practitioners of those fields, if the question is of interest at all, it is of passing interest, and has not (to my knowledge) sparked much soul-searching. But the work of these scholars (especially Popper and Kuhn) has been taken up with passion within a number of the social sciences, where it has been treated as a litmus test for legitimacy or entry in the club. Therefore, the claims assume huge symbolic importance: If you can prove that the findings of your field are falsifiable, that your field has paradigms, etc., you are respectable. The implicit assumptions, of course, boggle the mind.

The regrettable surface behavior that arises from these self-doubts is not long in coming. I recall an article written by Ray Dougherty (1974). In it he argued that Extended Standard Theory was a science because it had scientific revolutions; and indeed a better science than any of its competitors because it had *more* revolutions. I cringed at the time to see this sort of argument appear in a refereed journal (its very appearance might be said to disprove its claim);

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proved, over time, to be useful in facilitating lasting discoveries and deep understanding of natural phenomena. On the other hand, when children play at adult occupations, they grasp at superficial behaviors without understanding their deeper purpose. We must be very sure that that is not the case when we, as linguists, call ourselves 'social scientists'. There is a danger of a valuing the superficial manifestations of 'scientific' behavior as validating for their own sake: quantifying, formalizing, replicating and so on make us feel like real scientists, grownup and responsible - but do they produce lastingly valid results in our field? A corollary of 'playing science' is the overvaluation of theory at the expense of observed data, as represented perhaps most obviously in the many Chomskyan gibes at 'empiricism' as stupid, culminating in utterances like Newmeyer's cited in footnote 5, or Gazdar and Klein's (1978: 666) statement to the same effect. It may also be germane to note that the paradigm 'science' social scientists like linguists are prone to take as a model is Newtonian physics, with its dichotomies, obsectivity, and certainty. But quantum physics has cast doubt on all these vaunted desiderata, and we might ponder the dubious advantages of modeling our own theory and method on those of an obsolescent field. Phlogiston, anyone?

but my chagrin was somewhat assuaged a few years later (misery loves company) when I encountered, in an equally august psychoanalytic journal, an article by Kurt Eissler, one of the giants of the field, arguing that psychoanalysis was *too* a science, *pace* Popper, because it had lotsnlots of paradigms. Case closed.

Suppose what I have proposed has some truth to it: Suppose the social 'sciences' in general, and linguistics in particular, have yet to demonstrate the appropriateness of the scientific method to their subject matter, the working of the human mind. Suppose a large part of the work of this field is, despite our recent disclaimers, still humanistic at heart: dedicated to figuring out what is individual, how a person creates him or herself, and therefore beyond the reach of statistics, of replicable experimentation. Then what happens if we graft the argumentative techniques that work so well for science upon a humanistic study? What will we get? Modern linguistics, I suggest – to its misfortune.

Science, with its distinct and contrasting paradigms, works by an adversarial method: Only one of us is right, and I intend to show that it has to be me. Because there is agreement on the basic issues – method and data – if one interpretation can be shown to be superior in its explanatory capacity, everyone will sooner or later agree on it, and that will become the prevailing model. While at any moment, in any active science, there are always several areas of passionate disagreement, there is normally expectation that sooner or later evidence will transpire that will be persuasive for one approach. And while scientists do of course take the disagreements somewhat personally and get excited and even angry with one another when their views are challenged, it does not seem that these passions normally result in formal rifts. (One can, of course, think of occasional exceptions, such as the Leakey/Johanson fracas in physical anthropology. It may not be coincidental that this field is on the border between science and social science, nor that the necessary data are inadequate, perhaps forever.)

So adversarial argumentation is not a serious problem in fields in which there is confidence that external and objective grounds are bound to emerge to prove one side right. And in those fields in which this is not part of the world-view, this kind of conflictual discussion is not encountered, for it would clearly accomplish nothing. But the social sciences, and most especially linguistics, are in the middle, with the focus of the humanities but attempting the methods of the sciences. And Chomsky brought to bear one further tool of the latter, the better to make linguistics rigorous and respectable: the tradition of contentious and acrimonious adversarial argumentation.

It was first applied, of course, to foes within linguistics or allied fields (Lees (1960) against Bolinger; Chomsky (1959) versus Skinner). This served, more than anything, to create the climate in the early years of CTG of 'us against the world', TG surrounded by vicious enemies. I remember well the times that non-transformationalists would speak at MIT, in those early years when the field still saw itself as fighting for survival in a hostile world. Rather than attempting to charm, conciliate, find points of connection, the circle at MIT regularly went for blood. Points were made by obvious public demolition; the question or counterexample that brought the offender to his knees were repeated for weeks or months afterward with relish. TG did not win, then, by gradually persuading its opposition, but mostly by waiting until they retired or died. Since the field had been quite small, this didn't take very long. Those who were not won over or gotten rid of were rendered ineffectual. There was no place for pluralism.

This habit of victorious battle felt very good to these young people, ardent and ambitious. But by 1964, certainly, the battle was won. No more opponents came riding into Cambridge eager to joust with the champion. Could they let their lances rust, slide into a gentle middle age? Impossible! So when the time came that dissension arose within their own ranks, they were primed for blood. Everything led inevitably to conflict and implacable hostility: (i) the fact that people had entered TG with two very different agendas, and didn't know it; (ii) the fact that linguistics was caught between the methods and data of science and humanities, and wasn't cognizant of this; (iii) the fact that early transformationalists had emerged, rather late in life, from other fields that had disappointed them, and they were thus feisty, ambitious, and in a hurry to prove themselves; (iv) the fact that they acquired a taste for blood early on, then ran out of prey, and had no one to turn their weapons against but one another. Thus the bitterness, the divisiveness, the insolubility of the struggle. The seeds were sown long before dissension appeared; once any source of serious disagreement manifested itself, the outcome would be inevitable.

The extramural hostility had one other unfortunate result, at least for one side in the eventual struggle. It was seen as very important not to let the bad guys see your weaknesses, if any: Rather than be honest, acknowledge that TG had its flaws, things it couldn't do, the requirement of adversarial discourse was that one present a pose of perfect poise and complete certainty. Within, at least sometimes, one could groan about the failings of one's field, the immensity of the labor (although there was normally an atmosphere of buoyancy, the sense that all apparent problems were capable of solution, and very soon); but to the world, one presented one's best face. Not only did this serve to further alienate the outsiders, the brighter of whom could clearly see the weaknesses inherent in the model, but were hooted at by the faithful if they attempted any critique; but it meant that generations of students were educated to believe that success involved PR as much as insight or hard work. If you could overlook your own inadequacies, maybe they would go away, and at least no one else would tease you about them. The latter might be true, but they didn't go away, and the attitude created closed minds and an unwillingness to question the established doctrine, to explore, to combine their theory with others. Along with the adversarial argumentation, it guaranteed that CTG would remain closed to outside influences – ensuring a short life, or at least a tedious one.

Because they had closed themselves off to acceptance by the outside world, it was critically important for transformationalists that they feel loved and accepted by each other. It was of equal importance that they be able to see their colleagues as worthy of love and respect. Both of these attitudes were especially true with regard to Chomsky, and became more true with regard to him as time passed and his reputation soared in the outside world. In the early days, now barely remembered folklore, before his fame, it is related that he was readily available – physically and even psychologically. You could wander into his office, and he'd take time to talk to you. He might even be persuaded by something you'd said (Postal managed to persuade him of the need for deep structure, quite a change in theory!), and maybe if you were favored you'd get a footnote or bibliographical reference in his next article. As time went on, both kinds of availability were lessened. One would think that as someone's repute grew, they would feel more secure, would be able to be more open to reasoned discussion, new ideas .... One would think that, but in some cases one would be wrong. It might work the other way: Such a person might feel insecure despite the laurels, and feel that only by maintaining a firm hold on the doctrine could he maintain his influence. Or someone might feel that his glory only proved that he alone had possession of the truth, and therefore to listen to anyone else was injurious to the field. In either case, the result was closure of the mind.

Not really paradoxically, as Chomsky himself became less available in mind and body, his status among the students as cult figure rose. He had always been seen as a figure of towering intellect and integrity; these perceptions increased. Hence, to have Chomsky's approval meant even more than it had before, even more than a prominent professor's blessing means to graduate students generally. Yes, to be in Chomsky's good graces meant mentions in his writings, getting your work published, getting a job; but it also meant that you were worthy of him, you partook in some small way in the godhead. For that to be withdrawn was equivalent to banishment from the Kingdom of Heaven. I realize I am again straying into hyperbole and religious imagery, and I can only assure the reader that it is employed to capture a mood. To lose the goodwill of one's fellow-transformationalists was less serious than losing Chomsky's but was still painful in this close-knit and indrawn community. For all these reasons, then, when the split came, it had to be highly unpleasant.

The worst of it was that, when Chomsky finally did address himself officially to the arguments of GS (as early as 1967 in lectures, later (1970) in print), the level of hostility and closed-mindedness was truly disillusioning. It wasn't even the sense of having fallen from favor that stung the most, though that certainly smarted; worse was the sense that an idol had fallen, leaving nothing else to believe in. The late '60s was an era of idealism and hope for a better world, and to see for the first time that the person one was following to create the New Jerusalem had serious human foibles probably hit worse, at this moment in history, than it might have earlier or later, more cynical times. So people unconsciously blamed Chomsky not only for being unreasonable as a scholar (which was legitimate), but also for destroying their illusions (which scholars are not supposed to carry into their professional lives).

The result can be seen as instantiating a process first described by Elisabeth Kubler-Ross in those facing death, and then extended to people in situations in which their sense of self and their relation to their reality is shaken: divorce, serious illness, job loss. It happened here too. First there is a period of denial: Chomsky didn't say that, didn't mean it that way; it isn't the case that he is refusing to see me or talk with me, it's just he's terribly busy; he didn't deliberately distort my position in his paper, he just didn't read what I said fully – probably because I wasn't clear enough. But as incidents multiplied, the denial became untenable and the next stage was reached: anger. This occupied a great deal of the next several years, with hostile argumentation back and forth in public and private. Once Chomsky was seen not to be an idol, he was recast as satanic, the Enemy. A great deal of the history of GS occurred within this framework. Then there is a period of bargaining: If I am good, if I am reasonable, maybe this horror will go away. GS entered a time of persuasion, attempts at rapprochement with the official successor to CTG, named by Chomsky Extended Standard Theory. The assumption was that *if* we could but find the perfect, clear, telling example, analogous to the passive transformation 10 years earlier in persuading the heathen to religion .... *If* we could but show them that we were looking at things in a way that was not entirely incompatible with theirs, but ours was better .... And so on. But since the GS/EST split mirrored the real world/autonomous system split that had existed underground from the beginning, persuasion was out of the question. And then, finally, the end: acceptance, the realization that there were finally and irreparably two schools, no more unity, no more us-against-the-world. Camelot had fallen. Not surprisingly, given this perspective, not long after GS realized the futility of the battle, it disintegrated on its own. Much of the fun was in the fighting, and without fun, there was no GS.

4. The role of the personal in the theoretico-political

> Bliss was it in that dawn to be alive,
> But to be young was very heaven.
> Wordsworth, *Preludes*, xi

Which brings me to my next point: the personalities of the two sides, how these are reflected in their work and their presentation of their work, and what that implies about how scholarly work ('science' if you insist on that term for linguistics) is done.

If you watch movies or television shows in which scholarly persons play a role, you can get some notion of how a scholarly person is supposed by the outside world to be, and by extension, how scholarship is supposed to be done. Scholars are often depicted in the media as abstracted from reality, and either humorless or possessed of a fey humor lost on more practical people. They babble on unintelligibly about things no sane person could possibly care about. Whatever it is they do, they don't have fun. Indeed, scholarship as a whole is distinguished by being that area of human endeavor that is neither fun nor useful in any way. And, just as (as I suggested earlier) linguists took *having Kuhnian revolutions* as the litmus test of science, and thereby could consider their field a science, so (one could argue) at least some of them, still insecure about their place in the universe, took the stereotypes of *not having fun* and of *being uninvolved in reality*, as yet another litmus test: If you Had Fun or Dealt with Reality, you weren't playing the game right, you were not a member of the club. It is useful to bear in mind here too that a generalization, even if it were accurate, is not the same as a test of membership; and only an insecure group would claim it was.

I have already said that the early proponents of transformational grammar came into the field with two very different, implicit agendas. This should suggest that, in other aspects of personality, they were very different kinds of people. It is often argued by the Pure Science Club that theory-formation, or preference for one theory over another, is based on purely intellectual criteria: just the facts, ma'am. But in a field like linguistics in which (as we have seen) both the very identity of the facts and what they proved were open to dispute, obviously the criterion of factual accuracy was not sufficient (and maybe not necessary either, judging from some of the claims made over the years about what was a grammatical sentence of English and what was not). Although most of us would not have admitted this under torture, I think it probable that, as often as not, we select our theoretical positions based on other, more personal biases: the way we want the world to work. We justify these positions *ex post facto* by finding the right kinds of facts for our preferred positions. Conveniently, in linguistics, it generally happens that there are enough facts to go around: some fact that fit neatly with Theory A, others that tend to support Theory B, still others that dovetail with Theories C ... N. No theory yet known, of course, covers more than a fragment of the observed and collected data without a lot of Procrustean pulling and chopping, which tends to do the facts something of a disservice. In any case, the incipient generative semanticists gravitated by force of personality into the kind of work they did and concern for the kinds of data they dealt with, and likewise for the proponents of EST. Thus, there were three basic facts about the personal styles of generative semanticists which had great influence on their work and the way they talked about it: (i) disorganization; (ii) non-hierarchy; (iii) non-formalism, or at least, an interest in formalism only as a descriptive convenience – not as an end in itself. These stylistic issues, when they are discussed at all, which is seldom, are treated dismissively: trivia which at best just underscore the theoretical perversity of the lot; or, occasionally, as much more seriously thought out decisions than they were, based on doctrine rather than – as is actually the case – personal caprice. But rather than doctrine molding style in this situation, it makes much more sense to say that underlying personality style molded doctrine.

These three significant traits were shared more or less by most of the people who formed the hard core of GS, and thus gave shape to the organization (even as the same was true of their counterparts in CTG and EST). Actually, only one can properly be said to be characteristic of the individuals themselves; the others have to do with the idiosyncrasies of the GS organizational structure itself. They were all interconnected, though.

First, and perhaps most significant: GS was, organizationally, disorganized, or at best, collaborative rather than hierarchical. There were leaders, but they were seen not so much as masters but *primi inter pares*. The spirit was democratic. (The leaders are, as they were sometimes called sardonically, the Four Horsemen of the Apocalypse: George Lakoff, James McCawley, Paul Postal, and Haj ('John Robert') Ross.) Doctrine, therefore, was not determined by one Personage at the top of the hierarchy, who alone determined what was in, what was out; but was arrived at in debate, discussion, anguished late-night phone calls. This made for a lively group and a lively field, but one that must have seemed chaotic to the outer world, especially those used to the rigid *ipse dixits* of MIT. Chomsky had disciples in a strict hierarchy: There were the inner circle, the various outer circles, Limbo, and Bad Guys. GS just had a bunch of people who got together at conferences to make puns and play Fictionary and smoke funny cigarettes. Theory, and fact, changed rapidly, depending on whom you asked and when you asked it. There was no central bureaucracy to tell people the doctrinal Flavor of the Month. It took a certain sort of mind to tolerate this chaos and flourish in the climate. Disorganized, said the outside world. Wild and crazy, they themselves would have said had they given it much thought. They thought of themselves as rebels, young Turks – but so had Chomsky and his first disciples, a mere dozen years before. The latter, however, turned into the Old Guard at the first convenient moment.

There was no GS Establishment, no *Congregatio de Propaganda Fide*, to keep GS pure. I think in retrospect that, at least some of us, sometimes, saw GS as presenting a united front, and its adherents as people with similar beliefs and ultimate aims. But there was never a shibboleth equivalent to CTG's assumption of a syntactically-based DS, and the various adjerents of GS had very different notions about practically all the tenets that Newmeyer identifies as Generative Semantic Orthodoxy. Indeed, judging from where we each stand now, years later, it seems fair to say that GS represented at best a loose coalition of interests: We joined together only at the points where our interests happened to coincide.

It makes sense, I suppose, that people with a high tolerance – or preference – for chaos would tolerate and delight in a theory which presumed and necessitated chaos, one which encompassed as its domain all human endeavor. And those who preferred a tighter organizational structure would also prefer a more rigid theory that drew strict limits on what was a part of its realm, what could be part of the theory and what could not. One might also argue that these basic personality structures colored other aspects of the theories of each side. If your preference is an organization in which one prestigious person controls what is believed, then there will be a certain tendency to authoritarian argumentation; a desire not to make one's case too simply, in such a way that it might be intelligible to the *hoi polloi*, the non-elect. In such a system profundity or brilliance might be directly equated with turgidity; preferred style would bristle with arcane references, innumerable vaguely relevant footnotes, untranslated quotations in fifteenth century French or nineteenth century German: anything to intimidate and stun the reader (something no representative of the GS mentality would ever do, as the epigraphs scattered herein make clear). Examples were shunned, to be used sparingly only when utterly unavoidable: Otherwise the opposition might see what facts you meant to allude to, and test your theory out on them. In short, orderly minds too often display a fondness for authoritarian systems. Mussolini made the trains run on time.

CTG and EST, then, as well as their current descendants, are hierarchical in organization. In this respect as well as in their dependency on formalism, they are masculine.[9] I had mentioned in earlier work that formalism was a male

[9] Once again the reader is adjured to beware of easy dichotomies, including M/F. But although individuals display a range of behaviors and orientations along a continuum between those points, societies tend to see the sexes as polarized: every behavior, every physical and psychological trait, is identified as either Male or Female, Masculine or Feminine. It is in this sense that we might say that GS reflects certain stereotypically feminine properties, EST more (stereotypically) masculine ones. The fact that the majority of both groups were males (as was to be expected in academia of that period – and this) is not especially relevant in this perspective. We would say rather that GS allowed both its male and its female members to have access to aspects of their psychologies that this society has identified as typically masculine; and the opposite for EST. I do find it surprising though that more than one of my colleagues of the GS persuasion, and of both genders, have responded to the suggestion that GS was in some sense 'feminine' as though it were an insult. Rest assured, it isn't.

I wonder, though, whether the 'feminine' nature of GS and the 'masculine' one of EST is what has led to the perception of the former within academic linguistics as less successful than the latter. After all, a 'feminine' perspective is antithetical to the hierarchical, dichotomizing tendencies that are characteristic of academic discourse and masculine world-view. (And the academic perspective is inextricably masculine, as it has been for the last couple of millennia in the west.)

perspective, and Newmeyer (1980: 169) has seen fit to make fun of the statement, apparently without asking what lay behind it. I meant more than the obvious fact that men tend to be overrepresented in the formal end of the field – the more formal, the more masculine – and women at the informal, data-oriented end. Of course there are counterexamples – it would be stupid, especially for a generative semanticist, to claim that humans are irrevocably dichotomized by gender. I am speaking, as we must in talking of human possibilities, of general tendencies. But I meant more than this. There is now an impressive array of evidence that, from earliest infancy, males respond to stimuli differently from females. Male neonates are more responsive to their external physical surroundings: light, warmth. As they get older, they are more active – interactive with their physical environs, more eager to play with things, objects. This involvement with inanimate externals is evident again in the games they play, having to do with external goals, manipulation of objects. Little girls, on the other hand, are immediately more responsive to people, being held, voices, eyes. They smile sooner, recognize others sooner. Later, their games are interactive, concerned with reaching out to others: House, Doctor, and so on.

We can see formalism as maximally non-interactive; and autonomous language theories as treating language as an external, impersonal object. On the other hand, a theory that is concerned with language as an interactive strategy, linking people with one another more or less successfully, is closer to the way women tend to approach the world. And a hierarchical theory is masculine as well in that it tends to recapitulate the structure of male institutions: government, the military, the university, and so on are and have always been organized in a hierarchical fashion, with a single authority at the top – like CTG. On the other hand, female institutions or groups, in those cultures in which they exist, tend to be more collective, cooperative, or collaborative: There is no formal leadership structure. This tends to make such groups more fluid and impermanent, and sometimes more disorderly, but they usually get things done. I am – it should be clear – not saying that EST was the field for manly men and only those, and GS only for womanly women. But I am equating the theoretical preferences of each with their organizational structure, suggesting the connection was not coincidental; and suggesting that there are models of each in typical gender-oriented behavior.

Generative semantics .dealt with a much more convoluted world, but attempted to make sense of it without reducing it to orderliness. We were not all born writers, some of us were clearer than others. But we tried to be intelligible, did the best we could to overcome the stylistic handicap of being academics. We tended to keep down the number of footnotes, and avoided

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Here, as often, to understand what the reality is, we may have to wrest ourselves from the prevailing ideology. Several books have recently appeared within a feminist framework offering reassessments of some of the culture's unexamined verities: 'objectivity', 'science', 'reason', and so on (Belenky et al. (1986), Gilligan (1982), Keller (1985)).

long foreign quotations. Above all, we liked examples. Actually, we loved examples – couldn't have too many. They tended to proliferate on the pages of GS articles, an embarrassment of riches, more than was needed to establish the point being defended. The reason, really, was that there was always another that proved the point in a slightly different way. The real reason, though, invokes another point in my discussion: the examples were fun. Fun was what we were in the world to have. So there would be examples.[10]

In fact, it is arguably and regrettably the case that, all too often, we would sacrifice force and clarity for fun in compiling examples. In choosing between a boring sentence that was unquestionably grammatical and clearly made its point, and a droller one that was dubious or murkily relevant, we tended to choose the latter. Political references, arch invocations of sex and drugs, weird names, peculiar behavior – we had 'em all, and wouldn't have traded them for the world. It is in part for this reason that vintage GS papers are often so hard to comprehend today: We have lost the context (ironically enough): Both the topical references and the spirit that pervaded the writing are impossible to recover.

While we might have made things a little more unclear by our choice of examples, I think in the long run their frolicsome nature worked in the direction of intelligibility. It is axiomatic among educators that the livelier the text, the more likely it is to stick in the student's mind. Readers will be more apt to make sense of an argument if its examples are fun, they'll be less apt to get drowsy, and less apt to forget: If they at least remember the examples, they may be able later to reconstruct from them the arguments they supported. I don't want to suggest that we invented those examples from such laudable motives, but they can be defended in this way. Certainly I would take issue with those curmudgeons (I think of Stockwell and Newmeyer)[11]

[10] There is more to the GS romance with examples, though I don't think we saw it at the time. Examples are egalitarian: They allow each reader to form his or her own conclusions, based on direct access to the same evidence used by the writer. The hermeneutic Master, who controls the disciples' exegesis, in disciplines as diverse as CTG and deconstructionism, makes decisions on an *ipse dixit* basis: He (and it is by no accident virtually always a *he*) decides what his pronouncements meant, mean, and will mean; he controls the development of the theory because he is the source of understanding. By contrast, a tradition relying on prodigality of examples is saying to the reader, insider or outsider: Here are the facts. Make what you will of them. The reader is thereby empowered, relative to the author(ity). One can see why this style was natural to a group that came to maturity in the late '60s, and why it might be threatening to those, younger or older, who preferred a more authoritarian relationship between Master and disciples.

[11] Stockwell (1977: 131, fn. 2):

"In the history of transformational grammar, several scholars have given humorous names like Pied Piping, Though-Movement, and Sluicing to syntactic rules. Unfortunately, such names, a few years later, are neither mnemonic nor transparent in their meaning."

We might note in rebuttal that many CTG/EST names are no more mnemonic: Root Transformations, Strict Subcategorization, the A-over-A principle being just the first that spring to mind. But the real point is that there is no reason for names to be mnemonic: One associates the names with processes, just as one associates human names with faces. Stockwell doesn't especially *look* like a Robert, but that doesn't make it harder to recognize him.

who object to GS writing on the grounds that examples were funny, names for rules or principles were frivolous ('Pied Piping', 'Flip', 'WH-iz deletion'), or they wrote using slang or colloquialisms, or heaven forfend, puns. Their argument seems to be that it is indecorous, unscholarly – scholars don't want to Have Fun, and so anyone who is observably Having Fun is not a scholar.

If GS had had fun at the expense of accurate description, that would be grounds for criticism, of course. But this is not anyone's claim. Just as the establishment made assumptions about the scientific status of linguistics, it made the same unexamined assumptions about what constituted responsible scholarship. One can certainly ask which stylistic strategy is the more preferable: murky unintelligibility or quirky frivolity. I don't know how to decide, but I do know which I'd prefer to read.

Their collective high spirits point up another salient trait of GS as a field: the tendency for its writings to point inward, for arguments and claims often to be implicit. I know I just said that GS writing is especially accessible. But I ought perhaps to qualify that statement, and shed more light on another claim I made above, about the evanescent quality of GS writing. On a sort of local or superficial level, GS papers tended to be quite clear and aboveboard. But at a deeper level, it was different. This was not apparent to the original writers and their primary audience, but surfaces embarrassingly when we reread our writings now. It is often very hard to discover just what claims are being made, with what ultimate theoretical purpose: How do these arguments fit into a more general scheme? What prompted them? Why are they important? The feeling is that the writer knew, but didn't care to say.

If GS was in fact turned inward, there is a historical reason for it, the same one as informs the style. TG, under Chomsky's tutelage, always saw itself as oriented to the outside world, persuading the heathen. They wrote for others, and were generally careful to define their positions relative to opposing ones, and state their premises explicitly. They assumed, in other words, that the reader was an interloper, one who had not been present throughout the development of the thought processes represented in the paper: everything of importance had to be spelled out therein.

The generative semanticists, on the other hand, had originated as a tight-knit group within MIT. Their earliest comunication was with one another. They did, of course, wish to persuade others to their view, but they always saw their primary audience as one another. They talked to one another continuously, and their papers often seem to be written as offshoots of these

In the same vein Newmeyer (1980: 171 f.) grouses:

"Such stylistic traits [of GS: he is referring to its practitioners' 'whimsical style of presentation'] only served to give extra credibility to the charge of lack of seriousness.... Indeed, it is tempting to speculate that generative semantic style is but a classic example of content both shaping form and dominating it."

A comment which reflects a lack of understanding of both the style and the content of GS.

conversations, intelligible only if you were a party to the talk. Or they represent the distillation of many discussions and arguments, and skip over crucial steps in the argument because they had all been through them before – orally – and it would be tedious to spell them out again. The field remained solipsistic in this way, especially since its proponents felt rejected by establishment TG. Nobody else wanted to talk to them, they thought; so they wouldn't try to talk to anybody. Hence, too, their colloquial, whimsical style: It was their personal and interpersonal mode of self-presentation; since they envisioned their writings having the same audience as their oral communication (whether on the phone or in papers at CLS), why shift the style? We knew our readers would understand. Indeed, the style itself became a kind of secret handshake. You could always tell a GS paper: by its title, its breezy style, its funny examples. You knew who belonged, who your people were. It was cozy comfort in a heartless world.

While there are philosophical and psychological explanations possible (such as the ones I have proposed here) for the GS style, it is useful to bear in mind, when we try to understand how theories are born, live, and die, that the generative semanticists themselves during their heyday didn't attempt to account for their assumptions, style, or behavior in these terms. First of all, they were too busy arguing with each other and outsiders to worry about how they were arguing, much less why they chose to do it that way. It didn't occur consciously to them that they were devising a different style, separating themselves by style as much as content from their counterparts in CTG or EST. And this, finally, is because the style (even more than the content) was not a conscious accretion: Rather, it arose out of their fundamental selves, was inseparable from them. They could not have done otherwise, being who they were, at that time. For we must also recall that GS arose as an independent theory in the late 1960s, when experimentation with lifestyle and personality style was encouraged – as it had not been in the '50s, when CTG arose, or at present. We cannot separate the theory from its time.

5. Universality and relativity

> He thought he saw an Argument
> That proved he was the Pope:
> He looked again, and found it was
> A Bar of Mottled Soap.
> "A fact so dread," he faintly said,
> "Extinguishes all hope."
>
> Lewis Carroll, *Sylvie and Bruno Concluded*

Before we take final stock of the GS contribution, it may be useful to reflect upon another point of controversy then and now: the relative importance, in a theory of language, of universals and typological diversity. Are both of these aspects of language? Significant aspects? How should a theory of language incorporate each of them?

To a degree, this was a bone of contention between CTG and EST, though even more so between generative grammar of all forms, and its immediate predecessor, structural linguistics; and between the former as the prime example of 'autonomous' linguistics and the latter with its various hyphenated relations, e.g., anthropological linguistics and sociolinguistics. CTG and its lineal descendants, as we have seen, tend to attract people whose background is introspective: humanists, philosophers and mathematicians, 'armchair' types, who see the systematicity of all human language as interesting in itself, or as a way of demonstrating Descartes' view that all human beings are basically in possession of the same system of logical thought, with language as its observable representative. The other fields are peopled by scholars who have spent time in the field, have seen with their own eyes, heard with their own ears, the diversity among human languages, and see this as the interesting fact. One of the structuralists, Martin Joos, once made a statement to the effect that, as far as he knew, languages could differ from one another in innumerable and unpredictable ways[12] – an extreme form of the relativist position, to be sure, and one the transformationalists seized upon with glee as attesting to the imbecility of the Bad Guys. On the other side, the anthropological types found amusing, or horrifying, the TG tendency to abstract 'universals' from thin air: If a phenomenon was true of English and German, it was said, TG would declare it a universal – an overstatement, but not by much. The dispute is far from dead. Recently Dell Hymes (a representative of the anthropological linguists) has written at length excoriating the universalist tendencies of current linguistics (1986), pointing out the impossibility of proving most such contentions in terms of any current theories, and the misstatements and wrong turns they have led to. His examples are in many cases cogent and sobering, but I think his arguments on the whole miss the point.

Language is neither pure surface diversity, nor pure deep identity. Both exist as vital parts of linguistic activity, and it is the connection between the two that composes grammar and indeed necessitates it. There must (empirically) be universals, or second-language learning would be impossible or tremendously difficult; there must be typological differences or else it would be trivial, or nonexistent. But even if we grant these propositions, questions still remain that divide theorists: Which of the two is more important (if

[12] Lest we be too quick to laugh condescendingly at this overenthusiastic expression of an unfashionable mythology, let us reflect upon an equally misguided more recent version – almost the reverse of Joos, yet demonstrating the same overly simple view of the nature of language and the task of the linguist. Stockwell (1977: preface xv) says, "It is my belief that language is really simpler than any linguistic theory comes close to suggesting".

either); and if there are agreed to be universals, what are they and how are they discovered?

Anthropologically minded linguists would say that the only way to prove the universality of a phenomenon is to demonstrate it empirically, for instance as Greenberg (1966) did in his work on universals of word-order. To make his case that there were statistical correlations between basic sentence word-order and other intra-sentence orderings, he and his investigators examined superficial sentences in a large number of diverse languages. So their universality claims had a firm empirical base.

Transformationalists, on the other hand, tended to reason deductively: A principle would be identified as necessary for the description of English syntax (the A-over-A principle, the cycle); arguments would be given very largely from English, though cases from other languages, genetically related or otherwise, would be supplied if informants happened to find any. (Informants were not expected to be completely fluent in the language.) If no one within telephone range of Building 20 could elicit any counterexamples within a reasonable period of time, the putative universal was established.

It should be noted that the universals in which TG took an interest were of an intrinsically different kind from the Greenbergian-typological. The latter could be determined upon surface inspection of actual data. The sorts of phenomena examined did not require of the investigator a deep knowledge of the grammar of the language – merely a knowledge of the lexicon (what was a noun, a verb, etc.); and rudimentary grammatical sophistication (the ability to determine subjects, verbs, objects, etc.). But the phenomena that TG weighed for universality could be studied only through acquaintance with underlying structure, and therefore required deep and reliable intuitions about a language, as well as considerable theoretical sophistication, since the phenomena were not superficially accessible. Hence, a typological approach was seldom feasible. The problem was that claims for formal universals (as Chomsky called these, as opposed to the substantive universals favored by the anthropologists) tended to be made on the basis of English and then applied *a priori* to other languages: the implicit argument was that, if it worked for English, it must work for other languages; if it must work for other languages, then the data of Language X could and should be fitted into the model; and if the investigator of Language X were clever enough, (s)he could find a way to accommodate recalcitrant data to theory. Exciting work, if dangerous.

Perhaps it will be useful to represent this debate in the form of a diagram, representing the two threads of modern linguistics (figure 4). Some of the points of distinction were explicit and acknowledged by both sides, others less so. One thing seems clear: This dichotomy, like the others I have discussed, is a misconstruction of the nature of language and must ultimately be resolved by a theory that incorporates procedures for responsibly eliciting and testing both universals and typological differences, and a model that relates them.

	#1	#2
Sources	anthropology	philosophy
Patron saint	Bloomfield	Descartes
Level of observation	surface/concrete	logical/abstract
Philosophical stance	empirical	rationalist
World-view	relativistic	absolutist
Professional identification	social science	'hard' science
Scientific method	quantification	formalism
Discovery procedure	empirical	intuitive
Perspective	diversity	universality
Interaction theory	sociolinguistics	pragmatics

Fig. 4. Two threads of modern linguistics.

Where does this fit into my present topic? Certainly, here was one place where GS and CTG/EST could present a united front against the forces of empiricism. But even here, there were sharp differences in the way each defined their terms. Both sides paid homage to the importance of identifying universals of grammar. But it became clear fairly early on that what each meant by 'universal' was different.

Chomsky in *Aspects* spoke of universals of form and substance, clearly indicating that the former were the more interesting. But we have seen that he was unwilling to consider alterations in his concept of deep structure so that its constructs – PS rules, lexical items – could be universal; the *Aspects* DS was in many ways English-specific, and even more so Indo-European specific. GS took the Chomsky of *Language and Mind* and *Cartesian Linguistics* at his word: If language was a window to the mind, if the mind's capacities represented logical structures shared by all human beings by virtue of their common humanity, then the deepest level of grammatical structure should be directly linked to or (as they said later) fully identified with those cognitive and logical capacities: The base of syntax (the deep, or rather, underlying or logical structure) should be semantics itself, not merely accessible to and interpreted by the semantic component. Then, the basic forms and mechanisms of syntax, including the lexicon, were the same across languages. Chomsky, of course, would not follow where these suggestions led, and the question of the universality of the base became perhaps the most divisive issue between the two camps. It is a bit ironic, seen through GS eyes, that Chomsky should be attacked for his universalist proclivities on the one hand, while on the other, he appears rather closer to the surface-typological school (rather the way so many aspects of his theory, seen through GS eyes, come closer to structuralism than to GS).

6. Why GS failed, if GS failed

Errare malo cum Platone quam cum istis vera sentire.
Cicero, *Tusculan Disputations* 1, 17, 39

In the end, we must ask: Why did GS fail – for all its hope, its optimism and its energy? If we accepted the question as valid, we would have to answer: Because it tried to do too much, dared too much. It tried to encompass what could not be formally or rigorously controlled, and this was intolerable to those who see linguistic investigation, or scholarly work generally, as taming the universe, getting things under our control. It's a ruggedly masculine image: Knowledge, like nature, must be broken, tamed, bent to our will. We cannot tolerate slippage or disorder. GS reveled in disorder, and in its ability to tolerate it lay its contribution – and, if we accept the myth, its downfall.

I have offered disclaimers in lamenting the downfall of GS. For I think there is an excellent argument that GS never died. The conventional wisdom is that the Chomskyan branch of the theory continues in a straight line, with only minor modifications separating the theories of *Syntactic Structures, Aspects,* early lexicalism, EST, and Government and Binding – and on into the future. For Chomsky's writings imply that position: Had we, the readers, but read *Aspects* intelligently, we would have understood how fully it presaged EST ... nothing really changes, it's just made more explicit by the Master, pushed by the stupid misunderstandings of the opposition. So we have a sense of growing, flourishing, success. But the GS people, as we have seen, were of a different kind. Their distaste for authority brought them together, and it also made them quick to acknowledge when some premise or formula fell apart. As Chomsky can be said to be too slow to acknowledge paradigm shifts, GS may have been too quick to despair – and to make its despair known to all, as loudly as possible (cf. Morgan (1973) and Sadock (1975)). So the legend has it that GS finally exploded, of its own impossible convolution, about 1975. But I think a better understanding is that, at that time, we began to move away from the idea that social and psychological context could be represented as the basis of syntax in a syntax-central grammar such as GS was. We understood that, to deal with the phenomena we had uncovered, the relationships between form and function that our work had made manifest and unavoidable, we needed to shift the emphasis of the grammar from syntax to semantics and pragmatics. No longer did we use pragmatics to 'explain' the central point, syntax. More and more we started from function, and saw syntactic devices as the servants of that function – so function still explained form, but form depended on function, not vice versa. Yet, there is no sharp break. I would say the difference between early-'70s GS and what its descendants practice today is less strong than the difference between *Syntactic Structures* and *Aspects*, the latter and EST, or the latter and Government and Binding. It's just the salesmanship that makes it seem otherwise. One should never underestimate the power of salesmanship – that, if anything, is the message that the history of GS has for us.

In the non-dogmatic spirit of GS, I will present here two contradictory arguments. One: GS died, and serves it right. Two, GS never died, and thank heaven for it. I believe absolutely in both.

(a) GS died, and serves it right. Seen from the CTG/EST side, GS failed because it was chaotic, and that chaos was the direct outcome of a theory that strayed too far from autonomous syntax, tried to incorporate the whole world, and choked on it. I would argue rather that GS did die: but 'GS' in its literal acceptance, in the form its practitioners specified up until the mid-'70s. GS in this sense died because it was too conservative, its practitioners too much enslaved by their early training as classical transformationalists.

The problem was not the attempt to connect syntax, semantics, and pragmatics in pragmantax; the problem was that whatever we called it, syntax was still central and syntactic modes of argumentation and proof – distribution and cooccurrence – were still the only options. But this doesn't really make sense: Why should syntactic form be what drives the grammar, if meaning and function were basic to form? We gave lip service to the concept of semantic and pragmatic conditioning for syntactic rules, but never really got away from the idea that everything had to be syntactically justified; that is, that the other two levels existed to serve syntactic form.

Then, too, although late in the history of GS we began to realize the importance of continua, we never followed through; the grammatical devices and categories GS developed remained as dichotomous and discrete as any within CTG. And as the meanings and functions we sought to capture were of course continuous, we went crazy finding more and more and finer and finer subcategorizations in an attempt to account for the complexity and richness of the data we wanted to incorporate into 'linguistics' and 'language'. Because we clung too desperately to the mind-set of our youth, we made GS untenable.

There is another way we can account for the perception that GS failed and its competitor survived, and that is by considering the role the devices of public relations and salesmanship play in the ivory tower. GS is presumed to have failed because its practitioners *said* it did: How often, publicly and privately, we bemoaned the fact that our theories didn't fit the data! That everything was getting too complex for our understanding! That the phenomena of language went beyond the realm of science and into the realm of aesthetics and even the supernatural! All this was interpreted as a statement that we gave up, it was no good. We didn't really intend it that way (Newmeyer cites such statements as evidence of our despair, but as so often he misunderstands), but that was what was understood, and eventually we began to wonder if they maybe weren't right. In any case, we never made a vigorous defense.

Newmeyer suggests that salesmanship was the style of GS, with CTG/EST peopled by earnest Young Doctor Pasteurs, concerned only for the truth, while GS'ists ran around the world making conversions and selling highly abstract snake oil. But the premise is contradictory; at least, if we were salesmen, we were bad ones. We had no journal; we controlled few departments, if any; we couldn't even keep our own gang in order.

CTG, on the other hand, had in its progenitor a superb persuader, whose buoyant optimism about his theory swept others irresistibly along. Chomsky's style (and that of his disciples) always was to downplay sharp revisions in the theory. One reads *Aspects* without encountering any suggestion that deep structure represents a revolutionary departure from the TG of kernel sentences; one reads 'Remarks on Nominalization' without a clue that the description there of the lexicon and of deep structure existed nowhere before and was developed only because GS pushed Chomsky to redefine his position – quite radically. The fissures separating early TG, Classical TG, EST, and GB are at least as deep as those dividing GS and what its developers are doing today. But the Chomskyans present themselves as coherent, united, and essentially unchanging – a reassuring position for the neophyte looking for a wing to shelter beneath; GS shows itself as disorganized, chaotic, and unfocused – not a position to attract new adherents, or even hold ones who are afraid of uncertainty.

(b) GS never died, and thank heaven for it. What GS strove to do, its ideal version of itself, is still healthy, and if anything can save linguistics and make it once again a rational field, I think it is this perspective. It is a highly ambitious program, as well as (currently) a rather nebulous one, not fully articulated by any of its practitioners and practiced by people with a wide diversity of agendas who do not necessarily see eye to eye (any more than the old-time GS'ists did). But the contributions toward which GS was reaching when it fell into crisis are still part of all of our active agendas, though we approach it in diverse ways. Basically, I think the lasting contributions GS made, and its successor(s) will continue to make, to our understanding of language are these:

First, the realization that the phenomena of language are continuous, and that this continuity extends across all the data as well as the theoretical artifacts that describe it; as well as in the metastructure, our understanding of the relation between language and what it describes. Any formal theory that relies upon dichotomous representations is doomed to fail in the long run; and until formal devices are developed that are truly non-discrete, we are wise to avoid any and all attempts at so-called 'rigor'.

Second, and actually a part of the first: the full awareness of the interconnectedness of three parts of the human experience: language, the mind, and the world. While there are aspects of reality that may not be linguistically encodable, most of what we perceive as reality is colored by the forms of the language we use to understand and describe it; and the forms language takes are determined in large measure by the functions to which it is put – the understanding and communication of reality, or perceptions of reality. Any theory of language must begin with these functions, and see linguistic form as an artifact thereof.

7. Conclusion

And the new sun rose, bringing the new year.
Tennyson, *Idylls of the King*

What are we finally to make of the upheavals of those years and the complexities of feeling and belief that still remain? For surely there is no reason to dwell at length on a moment's misguided squabbles and the bad feelings they engendered, unless there is something to learn from the experience.

It seems to me now that the 'wars' were an unfortunate outgrowth of a failure on the part of all of us to make explicit – to ourselves and others – our covert hopes, aims, and beliefs concerning language and its analysis. Not surprisingly, the unaskable and unanswerable questions festering a decade ago remain largely unmentionable. Only by bringing them out into the light, as I have tried to begin to do here, can we hope to achieve some resolution and perhaps ultimately re-unify the field, or at least make it possible for us to hold rational discourse with one another.

I see this as the major, unaskable, festering question: Is linguistics a science?; and if not, What happens? Before you clench those muscles in your jaws, dear reader, consider awhile: Do we want our field to be a 'science' because that name makes our enterprise feel prestigious and worthwhile; or are there really justifications?

Let us drop back a bit and ask: what does it mean to be a science? Does linguistics work that way? Does language fit the model of the proper object of scientific inquiry?

Some areas of understanding are, necessarily, not scientific nor scientizable, though attempts have from time to time been made to deny this. The understanding of artistic products and their effect on us, I think, is a clear case. One reason is that the function and effect of art is individual. Each person can and must respond uniquely – to *Paradise Lost* or *Les Demoiselles d'Avignon*. Critics can sharpen and define our responses, creating deeper resonances for each of us as we experience or re-experience the work. But the tools and methods of natural science – experiment, replication, quantification, formalization – are impossible or ridiculous in this arena. Who cares how many college freshmen think Satan is more attractive than God in *Paradise Lost?* All that matters is whether *I* do: the humanities are about *me*; science, about *us* or (more probably) *them*, or *it*.

The clearly scientizable modes of investigation are different: They look at objects outside of us, or at least they address objects that are not part of the very apparatus we use as the principal investigative tool – the mind. While the ideal of scientific 'objectivity' has been called into question (both as a possibility and as an ideal) in even these cases, at least it makes some sort of logical sense there. One can count stars, or determine statistically the possible interactions of elementary particles. Much of the data is descrete and objectively verifiable; perhaps even replicable. But once one is dealing with the products of the mind, the products of several minds acting together, the products of the mind acted upon by external reality, and so forth, possibilities change radically. Replicability is highly improbable when exact context can never be reproduced in each instance. Discreteness is unlikely when any form in question is the result of the interplay of an indeterminable number of factors, each with its own subtle effect. Sheer statistical probability is uninteresting, if the reasons individuals choose particular forms in specific circumstances are what is relevant. Formalism is futile if there are so many sub-cases, sub-varieties, vicissitudes that no sets or categories can be reliably differentiated, when the irregularities and counterexamples outweigh the presumably 'regular' cases predicted by the rules. And all of this, I would argue, is the case with linguistic data, more often than not.

The first temptation is to give up in despair, to assume the data are so various and complex that no systematicity could capture them, and therefore, that there is nothing of interest to be said. But this conclusion makes sense only if we agree that the methods and results of hard science are the only ones to emulate. If we see that humanistic models must be a part of our system, then we see that only part of our work can be concerned with recurring regularities for which formal rules can be written – the rest is more like exegeses of *Hamlet*. The two must operate in tandem on the same body of data.

We have tried for most of this century to force language into the Procrustean bed of 'science', and the chaos and dissension that we have experienced in the field are the result. If we are a science, we must assume that only one paradigm has access to the truth, and it had better be our own. But the impossibility of getting everyone in the field to accept a single paradigm, to settle down to Kuhnian 'normal science', demonstrates that we have been seeing things incorrectly. Just as every serious literary critic who has had something to say about *Hamlet* has added to our understanding of that work, although each sees it in a very different way, so each linguist, or each theoretical perspective, captures a different vision of the linguistic reality, and all, though incompatible as scientific theories, have something to add to our knowledge. But we can no longer require that perspectives be combinable into one single theory: We must settle for different, but equally valid, viewpoints.

We must see that our work is not, on the whole, 'objective', since we can only view language through the filter of our own individual minds, themselves working through a lifetime of diverse and unique experiences.[13] Language use is subjective, and much of our treatment of it, if it is to represent it accurately, must mirror that subjectivity: talk about how the investigator reacts to the data as a human being rather than speaking as though that unique experience were somehow that of all of us.

The introduction of pragmatics into linguistics in the early 1970s brought (without our realizing it) this subjective and mentalistic aspect of language use into focus, where syntax-based approaches allowed the problem to be glossed over. Therefore, it was at the time of the introduction of pragmatics into GS that the dispute between the two sides got most heated and bitter. Not because one side wanted to see linguistics as 'scientific' and objective, and the other didn't; but because both did, and neither could reconcile that desire satisfactorily with the data that were now turning up. Both felt frustrated with the inability of their own theories to deal with these facts fully; one turned away completely, and the other attempted to incorporate the problems within the domain of linguistic science. The internal frustrations created irritation – for which each side blamed the other.

If we now take seriously the implications of the existence of a pragmatic aspect of language, we must recognize that the nature of language is not as we had been assuming, and the way in which it has to be studied is much more complex and sophisticated than any method available to scholarship at present. We have two choices, as I see it: to continue to pretend to objectivity, discreteness, the artifacts of Newtonian physics; and as an inevitable concomitant, brace ourselves for another generation of non-cooperation, frustration, and bad feelings. Or, we must put aside all of our previous ways of looking at the linguistic universe, and carve out a new perspective: toward language, toward knowledge, toward one another.

[13] It should be noted throughout this discussion that when I talk about the necessary subjectivity of linguistic ionvestigation, I really mean to confine my comments to the domain of syntax, semantics and pragmatics. Phonetics and phonology, being grounded in instrumentally-examinable and -verifiable actualities, might certainly be scientizable.

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