# Skeptical Linguistic Essays

**Paul M. Postal, New York University**
**e-mail: Paul.Postal@NYU.EDU**

## Table of Contents

# Introduction

This collection of essays is concerned with syntactic questions, with certain general features of grammatical theory related to syntax, here and there with semantic issues and quite a bit with questions of appropriate standards in pursuing research in the previously mentioned domains. It has almost nothing to say about phonology. The immediately following remarks are to be interpreted against the background of this restricted understanding of what 'linguistic' is here intended to denote.

A prospective reader might ask 'what is a *skeptical* linguistic essay?' I would answer that it is one based on a deep and long-standing view that much nonetheless prestigious current linguistics has in fact made very restricted descriptive and explanatory progress and, in some areas where great things have been claimed, no real substantive progress at all. This lack of true progress holds, I maintain, despite the fact that the literature of modern linguistics and its ancestors is thick with boasts of at least grand-sounding accomplishments; see in particular the discussion of (2) of Chapter 8.

Consider (1) for example:

(1) Epstein and Hornstein (1999: xi):

"GB's very success, however, dramatically alters the methodological landscape. It has spawned a consensus that principles-and-parameters accounts may well answer Plato's problem in the domain of language. This general and surely tentative consensus allows the other sorts of measures of success to guide minimalist theory construction and (self-)evaluation. In effect, given a principles-and-parameters setting, simplicity, elegance, parsimony, and naturalness move from background to foreground concerns in the construction and evaluation of analyses. To put matters more tendentiously than is warranted: given that principles-and-parameters models "solve" Plato's problem, the paramount research issue becomes which of the conceivable principles-and-parameters models is best, and this issue is (in part) addressed using conventional (not uniquely linguistic) criteria of theory evaluation."

Various hedges notwithstanding, the authors of this passage clearly communicate the view that the government binding (GB) framework has been a deep success and has even in some sense solved the problem of natural language (hereafter: NL) acquisition ('Plato's problem'). This is in my view so far from reality as to be little more than a fantasy. [1] The claim that GB has been a success is not backed up, and I

claim could not be, by any citation of substantive factual results for e.g. the grammar of English. Chapters 2, 3, 5, 7 and 8 consider in fact cases where some factual success in these terms seems to have been proclaimed and I suggest that they show that the claims are groundless. Note that passages like (1) make no attempt to consider criticisms of the favored view nor do they deal with arguments, many of considerable detail and depth, that other, competing views of NL syntax are far superior to the GB view. Work in lexical functional grammar (LFG) and head driven phrase structure (HPSG), categorial grammar, etc., is unmentioned. In short, such passages partake strongly of the character of factually empty propoganda. [2]

Beyond any substantive questions, many of the essays that follow thus express even more serious skepticism about the standards which seem to govern the way much linguistic research is evaluated, justified and promoted. Lack of actual progress is unfortunate but at least remediable. But inadequate standards undermine the entire enterprise of linguistic inquiry by even eating away at a clear conception of what an actual result might be. One nonnegligeable trend supporting this damaging development is seen in remarks like (2):

(2) Chomsky and Lasnik (1995: 28):

"The shifts in focus over the years alter the task of inquiry considerably, and yield different conceptions of what constitutes a "real result" in the study of language. Suppose we have some collection of phenomena in a particular language. In the early stages of generative grammar, the task was to find a rule system of the permitted form from which these phenomena (and infinitely many others) could be derived. That is a harder task than the ones posed in pregenerative grammar, but not an impossible one: there are many potential rule systems, and it is often possible to devise one that will more or less work-though the problem of explanatory adequacy at once arises, as noted.

But this achievement, however difficult, does not count as a real result if we adopt the P&P approach as a goal. Rather, it merely sets the problem. The task is now to show how the phenomena derived by the rule system can be deduced from the invariant principles of UG with parameters set in one of the permissible ways. This is a far harder and more challenging task. It is an important fact that the problem can now be posed realistically, and solved in interesting ways in some range of cases, with failures that are also interesting insofar as they point the way to better solutions. The departure from the long and rich tradition of linguistic inquiry is much sharper and more radical than in early generative grammar, with problems that are quite new and prospects that appear promising."

The notion to be critically commented on here is that in the linguistics advocated by the authors, constructing a grammar of a NL which gets the facts right putatively 'does not count as a real result'. Rather, supposedly, a genuine result is only achieved when a successful rule system is deducible from universal grammar. While sounding impressively deep, the implications of such remarks *at this stage* threaten an elimination of the constraints on linguistic theorizing by precluding a serious factual basis for it. How much can anyone be expected to labor to actually try to account for the facts of actual NLs, internal to a point of view which proclaims at the start that success in such an endeavor is not even a result? But, as stressed long ago (Chomsky, 1957: 11), proper understanding of NL universals depends on *successful* descriptions: ''More generally, linguists have been concerned with the problem of determining the fudamental underlying properties of successful grammars.''

The thinly disguised contempt for getting the facts right manifest in a passage like (2) emerges particularly clearly when one is told that 'failures are also interesting' and 'point the way to better solutions'.[3] Such claims can only be seen as self-serving and defensive attempts to make descriptive failure seem acceptable. *Some* failures (of some kind) *might* be interesting in some way, 'interesting' being monumentally subjective, and *some* might point the way to a better solution. Others might just be hopelessly wrong, misguided, contradictory, ludicrous and hence incapable of pointing to anything positive whatever. But since supposedly even descriptive failures are valuable, then, assuming descriptive successes are of at least *some* value too, passage (2) would appear to embody the overall idea that *anything* done in this framework is progress. While this would be marvelously convenient for its supporters, it is evidently entirely illusory!

Implicit in (2) is the fantastic and unsupported notion that descriptive success is not really that hard and so not of much importance. Were such a claim true, it would be straightforward to show that it is. The authors would merely need to document the proper descriptions (since supposedly ''it is often possible to devise one that will more or less work"). Given the now *very* long period of work in the advocated terms, successful descriptions should be abundant. In fact, since their current position is explicitly a development of decades of effort by hundreds of people, with English the favored domain of study, correctness of their point of view would, one would think, almost *inevitably* have yielded some ( ?many) concrete descriptive results, at least for English. But they offer nothing of the sort and it is not that common to see such things even attempted in recent years. One is a very long way indeed from concern with realizing the earlier goal (Chomsky, 1957: 13) of a grammar which would describe all the well-formed sequences and none of the ill-formed ones. Moreover, one need only look at various syntactic areas where the authors themselves have

worked, passives, expletives, *wh* constructions, object raising constructions, parasitic gaps, control phenomena, crossover phenomena, ellipsis, negation, etc, to see that there is no subdomain, even in English, where a genuinely successful description can be said to have been achieved. I argue this in great detail for crossover phenomena and passives in Chapters 7 and 8. In such a context, talk about 'real results' which go *beyond* successful grammars is largely make believe.

Challenged here though is not the idea that there are higher level results to be sought which would involve a reduction of aspects of successful descriptions to universal principles. Rather, at issue is the merit of skepticism about the ill-supported assumption that successful grammatical descriptions are relatively straightforwardly come by, thereby rendering notions of deducing such descriptions from universal principles currently meaningful. Perhaps the most negative aspect of claims that descriptive success is not a real result and that it is appropriate at this stage of linguistics to concentrate on higher order explanatory goals is that it undermines understanding of (i) the need for truly intensive factual study and (ii) the lack so far even in well-studied NLs like English of serious descriptive understanding. If one had achieved an adequate description of e.g. English passive phenomena in the almost half a century of work in the terms the authors favor, it would make sense to inquire into the general principles from which such a description followed. But as touched on in Chapter 8, the actual descriptions offered in these frameworks so far are not only not successful but so bad as to hardly merit being taken seriously. Just so with issues of the strong crossover phenomenon, as treated in Chaper 7. Many claims of virtues for such descriptions are achieved only via a disregard for serious standards. But if there are no actual defensible descriptions, talk of deducing such from universal grammar or the like is evidently mere dreaming.

A last point. Dismissiveness with respect to the difficulty and importance of reaching adequate descriptions can only undermine the proper valuation of real work which actually advances descriptive adequacy. I am thinking here, for excample, specifically of work on negative polarity, which I have tried to educate myself about over the last half dozen years or so. Here it is uncontrovertible that really serious insights have been gained, specifically in the rich descriptive and theoretical studies partially and inadequately referenced by Atlas (1996, 1997, 2000), Beghelli (1994), Fauconnier (1975a, 1975b, 1976, 1979), Giannakidou (1998, 1999, 2001), Heim (1984), Hoeksema (1983, 1986, 1996 2001), Horn (1995, 1996a, 1996b, 2001), Horn and Lee (1995), Israel (1996, 1998), Kempson (1985), Krifka (1990, 1994, 1995), Ladusaw (1980, 1996a, 1996b), Linebarger. (1981, 1987, 1991), Wouden (1994, 1997) and Zwarts (1995, 1996, 1999). But enormous problems remain and no one who even samples the literature on this topic can, I believe, be

long deluded into shallow assumptions about the *ease* of finding viable descriptions. Anyone daring to speak in such terms should, I suggest, be faced with the simple requirement of presenting a characterization of the distribution of e.g. English negative polarity any which, as in (2), 'will more or less work. '

The fifteen essays found here, only one of which was previously published, fall into three categories. Part I contains six articles purporting to be positive contributions to the study of English syntax, with implications for general syntactic theory. While the point is not made explicitly in any of them, I do not think it possible to reflect on their content without seeing how far one remains in the year 2002 from serious understanding of English grammar.

Chapter 1 deals with the vexed question of English so-called *locative inversion* structures like (3) :

(3)a. Under the rock was lying a large snake.

b. In the sphere could be seen several space aliens.

c. At that time were invented new forms of defensive rhetoric.

This chapter, mostly written in 1994-1996, arose as a response to Bresnan (1994), which argued that the initial PPs in (3a, b) are subjects. Chapter 1 develops a range of evidence that they are not and that such clauses have invisible expletive subjects. An interesting consequence of these conclusions, if true, is that the evidence which Bresnan (1994) took to show the subjecthood of the relevant PPs actually argues that these phrases fall under the category of *noncanonical* DP behavior, known principally in the case of 'unexpected' or 'quirky' case marking, e.g. dative for subjects. The discussion reveals that it is all too easy to draw an erroneous conclusion on the basis of a few (real) arguments favoring a position P one finds pleasing because that pleasing character may deter one from seeking evidence against P. In effect, one is always balancing the pros and cons of various proposals and one must beware of prematurely concluding P on the basis of evidence for it if one has not systematically considered the question of available evidence against it.

Chapter 2 addresses a notable convergence of views among the GB, LFG and HPSG frameworks. For quite distinct theoretical reasons, all three of these positions developed during the 1980s the claim that there can be no phenomenon in NL syntax which could be informally characterized as 'raising to complement/object of P'. This *might*, for instance, be the proper analysis of cases like (4), as in fact suggested in Postal (1974: 363 note 7).

(4) Dora was depending on James to bring the wine.

It is argued, morever, that despite the theoretical convergence cited which would preclude any raising treatment for (4), that is in fact the correct analysis. If so, the theoretically 'forbidden' phenomena exists in English. While many are disturbed by controversy and lack of agreement in syntax, this essay might be taken to justify a certain skepticism about theoretical *concord*.

Chapter 3, which originated as a lecture given at the University of Maryland in May, 2000, deals with a curious set of restrictions initially visible in certain interactions with Heavy DP Shift and Right Node Raising constructions, and illustrated by contrasts like :

(5)a. I believe [the woman who is favored to win]$_1$ to have screamed.

b. I believe [the woman who is favored to win]$_1$ to be a doctor.

c. I believe [the woman who is favored to win]$_1$ to be you.

(6)a. I believe $t_1$ to have screamed [the woman who is favored to win]$_1$.

b. I believe $t_1$ to be a doctor [the woman who is favored to win]$_1$.

c. *I believe $t_1$ to be you [the woman who is favored to win]$_1$.

Here while in general the arguably raised (to object) subject of an infinitival complement with a main verb like <u>believe</u> can be the target DP of the Complex DP Shift phenomenon, not so in cases like (5c). This chapter explores the reasons for this apparent anomaly and its connections to a number of other peculiar restrictions. There emerge grounds for skepticism both about views which see Complex DP Shift and Right Node Raising as fundamentally distinct from left extractions, about those which reject the notion of invisible resumptive pronouns and also a straightforward argument for a 'non-derivational' property of these constructions.

Chapter 4 argues for the existence of a grammatical category distinction in English DPs which both traditional grammar and more modern research seem mostly not to have recognized. This distinction is taken to underlie such a priori puzzling contrasts as (7) :

(7)a. Tanya attended (in the matriculation sense) some school/*something/it.

b. Tanya discussed some school/something/it.

c. Tanya discussed some school$_1$/*something$_1$ after attending (in the matriculation sense) it$_1$.

This chapter could be said to be skeptical of the possibility of an adequate account of English syntax which does not appeal to the posited category distinction.

Chapter 5 grew out of research begun jointly with Haj Ross, and took more form as a talk "On the Grammar of Squat" given at the Yale University Department of Linguistics Reunion on November 10, 2000. The skeptical morals here are severalfold. There is the general implication that much less is understood about the grammar of even well-studied NLs than is often implied. In this case, now quite current slang forms are shown to have hitherto largely unstudied and surprising properties not only relevant to their own analysis but to that of central standard forms. Further, evidence emerges that even systematic morphological relations characteristic of a class of forms can be quite deceptive as to their implications for the grammatical analysis of those forms.

The reader will notice that inter alia the topics of Chapters 1-5, if developed as promised, will flesh out and support the claims at the beginning of this introduction that it is not serious to imagine that linguists are today in possession of, or even close to, adequate descriptive accounts of syntax, even for a heavily studied NL like English.

Chapter 6, while raising serious issues of linguistic theory, is also in a sense transitional to the concerns of Part II, as it uncovers an unpleasant feature of much common linguistic theorizing, namely, deep question begging. It is argued that a critical claim about the role of the lexicon which underlies the assumption that NLs could have generative grammars in the technical sense is supported in a literature dating to the mid 1950s only by begged questions. This situation is not what one would expect to find in real linguistics but is entirely expected internal to the topic of Part II, which is something specious masquerading as linguistics. This chapter presents arguments based on a variety of phenomena that NLs have a property called openness, which precludes the possibility of their having proof-theoretic/computational/generative grammars.

Part II contains eight essays, seven of which purport to uncover something which I claim is justifiably called *junk linguistics*. The latter is defined not by the substantive matters it touches on but by the standards, more accurately lack of same, which govern what to a significant extent only purports to be linguistic inquiry. It is one thing to lack insight, to propose defective principles, to suggest generalizations which do not stand up. All this is a regrettable but nearly inevitable part of normal inquiry. It is a quite different thing to flout minimal standards of scholarly procedure, to ignore the literature, to claim such and such generalization holds when one knows or should know it does not, to generalize to grand claims from a few selected cases, to develop techniques, rhetorical and otherwise, for avoiding falsification, to deliberately cite certain facts

which support one's proposals while deliberately not citing those which do not, to fail to respond to criticisms and to restate criticized positions as if no critique existed not because the challenges do not merit a response but because one lacks a viable response, to utilize other people's ideas without credit, etc. Combinations of various of these and other unacceptable procedures inevitably yield something which, while purporting to be linguistics, is actually junk linguistics. [4] The latter claims to advance the understanding of NL but it is a pretense and fundamentally a deception. [5]

That junk linguistics should not exist is uncontroversial, and it would be soothing and thereby quite tempting to think that it does not. Moreover, the contemporary culture of linguists is, I believe, unreceptive to any claims that it does. But facts about linguistics aside, this would be a puzzling state of affairs. For scientific fraud and fakery in general is hardly rare and whole works are devoted to its dissection; see for instance the excellent volume by Broad and Wade (1982). [6] After all, the term *junk science* on which *junk linguistics* is calqued has a well-known non-null extension. By what miracle could one expect that the field of linguistics would be free of such things? Especially true though is that contemporary linguistic culture is certainly extremely defensive about questioning the standards of the influential and powerful. [7]

To give one concrete example, Postal and Pullum (1997) documents a clear case where a major linguistic figure distorted to a biographer (the latter a person in general untutored in linguistics and its history) the history of that figure's own department, a history in which I was personally involved. The distortion, not surprisingly, made the figure appear far more positively with respect to the (remarkably marginal and minor) issues at stake than reality indicates. The validity of the historical claims in Postal and Pullum (1997) has never been challenged. But after it appeared, I received a note from the chairman of a major Department of Linguistics in the United States. Therein the chairman expressed distaste and fatigue with the sort of quarrelsome content of our article and expressed worries about its negative effect on students. No negative feeling was directed toward the dishonesty we had documented. That is, the chairman's critical attitude challenged exclusively the revelation and correction of falsehood, not the falsehood itself, although nothing addressing the conclusion of falsehood was presented. In my experience, this attitude, which sees *exposure* of misbehavior (at least that by a favored few) as in bad taste while ignoring or finding some strained justifcation for the behavior itself is not isolated. But a field which allows the pseudovalue of consensus including at least a dictated silence about misdeeds primacy over the truth, some of which may be displeasing, is all too likely to obtain a lot more consensus than truth.

Junk linguistics of course attempts to impersonate real linguistics as junk science in general attempts to impersonate real science. My view is that the best defense against junk linguistics is precisely the skeptical attitude referenced in the title of the present work. Here there are several aspects. First, one must of course intellectually challenge, at least to oneself, claims that such and such generalization or princple holds either for a particular NL or, more grandly, for all. One wants to know where this has been shown, what data sets support it, what data sets if any attack it, etc. When one is told that so and so has shown such and such, one needs to demand references, specifically page references. [8] Second, one must try to analyze as clearly as possible the logic of the purported support for various proposals. However, certain aspects of junk linguistics are more insidious than simply inadequaely supported or false claims. A recurring feature is to utilize a pretentious terminology to make grandiose-*sounding* claims which, on analysis, turn out to involve no factual propositions about the subject matter at all. Good examples of this with respect to the study of passivization are found in Chapter 8. One different example should suffice for illustration here.

Smith (1999: 1) begins his investigation of the linguistic contributions of a leading contemporary figure by stating flatly` that "he has shown that there is really only one language." This claim is then repeated (1999: 214) at the end of his laudatory volume, indicating that it is meant to represent an important accomplishment. Notable is that nowhere is there any citation of a work or even part of a work where a demonstration of this seemingly remarkable property could be found. Nor is there any even attempted analysis of what the claim of 'only one language' could mean. Evidently, it is not intended to signify that we all speak Mohawk. [9] What could show that it is false and that there are, say, two, nine, or sixteen hundred languages? Given the undeniable vast diversity among the totality of attested NLs, such a claim can only mean that there are certain similarities among them. The situation is entirely analogous to that for the totality of known human beings, which reveals a vast diversity and yet an enormous core of common features (hence e.g. the possibility of medical science). But is any biologist going to claim thereby that there is only one human being ?

Evidently then, the relevant claim is empty, mere use of an unanalyzed terminology ('one language') with presumed deep and important connotations to cover a claim no less devoid of content than would be a 'one human being claim'. Such empty puffery is typical of junk linguistics. Especially disturbing is that such vacuous 'accomplishments' can be claimed, discussed, published, reviewed, etc., all without any outcry, as if the difference between such pretense and real inquiry was not noticed.[10] This should be a warning that the rot of junk linguistics is widespread and little recognized. But another aspect of the invocation of

contentless accomplishments like the 'one language' claim deserve stress. Research which has *real* scientific accomplishments has no need to cite such empty pseudo-results. Their invocation is, first, a pathetic attempt to fill the gap left by the lack of actual results and, second, consequently, in effect an unintended admission of the lack of same. Remarkable then is the apparently widespread inability to hear the admission. [11]

Relevant to an understanding of this situation, no doubt, are the mind-clouding effects of (i) the repetition of claims; (ii) the association of claims with prestigeful figures, (iii) the creation by the process of education of interest groups whose career success depends as much or more on the conformity of expressed views with certain doctrines as on their validty and scientific grounding. Here one should, for instance, look most carefully at the recent discussion of the introduction and widespread acceptance of the so-called 'minimalist program', arguably introduced with no groundings in descriptive success whatever. [12] The fact that one finds a claim repeated in ten, twenty or one hundred works cannot, no one doubts, in itself legitimize the claim. But there is nonetheless a tendency to attribute seriousness to widely cited claims. My experience suggests though that wide affirmation of a claim is in current linguistics not even ground for confidence that it is serious, still less that it is well-supported or true. As already indicated, Chapter 2, though not concerned with junk linguistics, documents a case of broad cross-framework agreement that a certain syntactic phenomenon, arguably found in English, is universally impossible.

Chapter 7 examines a popular and widely cited purported explanation of the strong crossover effect, a phenomenon apparently first made explicit in Postal (1971). The junk linguistic character of this proposal is revealed nicely inter alia by the fact that the so-called explanatory account can be shown to demonstably fail merely on the basis of data not from some exotic NL but from English, already published (in Postal, 1971) a decade *before* the essential junk claim appeared. Ominously, this state of affairs in no way interfered with a long and happy acceptance of the junk claim. That is, for a junk linguistic proposal, having been in effect already shown to be false before it was even formulated is no current barrier to success.

Chapter 8 considers a range of transformational grammar views about English passives. It is argued that they manifest a characteristic combination of junk features including seemingly strong claims actually rendered empty by rhetorical dodges, grotesque factual incompatibility of claims if interpreted in a way with factual content and an overall deep contempt for the facts, standards of argument, the literature and readers. It is shown, for instance, that some strong (if interpreted factually) transformational claims about

passives of the 1970s were not only already known in the 1950s to be false, but known to be such by the author of the later claims.

Chapter 9, previously published in 1988, takes the form of a series of parodies of junk linguistic claims, although the term 'junk linguistics' was not used. But the parody form should not obscure the serious intent. Herein is cited inter alia what is, one hopes, the only case in the history of inquiry where an author states that a proposition A *is a theorem of* certain assumptions B, then states that he has no proof of A from B, and then adds that it is unlikely any proof can be constructed. A 21st century reader might imagine that such incoherence could, at worst, only have appeared in some marginal outlet, a very bad beginning student paper, a set of parochial working papers, or in a missupervised thesis in, maybe, a deprived or isolated third world university, etc.. Not at all; it was published in what many might consider the then (and now) most prestigious current journal of theoretical linguistics. Moreover, despite the absurdity, in the intervening twenty four years, neither the author nor the editor who approved its publication have, to my knowledge, in any way recognized the junk linguistic character of the passage, which thus remains unrepaired. [13] Except for its place in my parody and a citation in Postal (1982), I do not know that it is ever mentioned. The implication is, evidently, that even the most grotesque junk linguistic failure, one trivially discernable independent of facts and entirely on internal grounds, is cost free for its perpetrator. Anyone who doubts that junk linguistics is a real and longstanding phenomenon should try to find an alternative account for all of (i) the production and writing of the above set of claims; (ii) their unimpeded passage before the eyes of several readers; (iii) their satisfaction of the editor's standards for publication; (iv) their survival over decades without correction. A key conclusion is that, unlike the parallel situation with junk work in many other fields, junk linguistics seems to incur no sanctions.

Chapter 10 argues for the intrusion of junk linguistics into the refereeing process for the National Science Foundation. A specific referee report is analyzed in detail and shown to be a near perfect model of junk linguistic thinking disguised as refereeing.

Chapter 11 focuses on issues of ethics and honesty in a passage putatively concerned with the foundations of linguistics. Here is junk linguistics arguably at its absolute worst with an author contradicting himself internal to the passage, contradicting other nearly contemporary work of his own, contradicting his own accurate remarks of decades earlier, scorning fundamental and universally acclaimed results of modern logic, ignoring criticisms of his position he cannot answer, all in aid of a foundational position which is

trivially incoherent. The reader who for whatever reasons a priori doubts the existence of junk linguistics might well be advised to begin here.

Chapter 12 undertakes a bit of lexicographic analysis of arguably curious and obscure usages of linguists themselves, ones like 'X follows automatically from Y', 'X virtually holds' and 'X is natural'. It is suggested that these usages in themselves reveal a strong underlying current of junk linguistic thinking.

Chapter 13, the final substantive essay of Part II, investigates recent claims that such and such supposed feature of NL is (virtually) conceptually necessary. It is suggested that such jargon is a substantively empty means, hence one to be expected in junk linguistics, of promulgating claims in the absence of any serious argument or evidence for them. It is argued in detail that certain claims of this type are not only unsupported, but unsupportable, since they are false. At issue is what amounts to serious and very longstanding question-begging about the nature of NL grammars.

Chapter 14 provides a brief set of schematic personal suggestions of how one should in principle deal with junk linguistics. As signaled in the present volume's title, a key component should be a thoroughgoing skepticism, one which begins with one's own views. Those who have different ideas about how to react to junk linguistics should be urged to make them public.

Chapter 15, which makes up all of Part III of this collection, turns to a different but possibly no less emotionally charged issue than that of junk linguistics. Here the topic is a protest against the attempt by the Linguistic Society of America (LSA) to impose, via its guidelines for nonsexist usage, what is claimed here to amount to the beginnings of censorship. You, a linguist reader, might assume that you are capable of formulating the non-substantively relevant portions of your own required illustrative examples on your own. But those who have influenced the LSA to promulgate this code disagree and feel you need *their* tutoring to avoid falling into the sin of offending someone, especially them. Such codes represent, in my opinion, as argued in this chapter, misguided intrusions of political views into the research environment and anything like them should be banned from the field forever.

## Notes

1 One can make the claim in the text without in any way accepting the view that linguistic theory is a theory of NL learning. The latter view inter alia confuses NL and knowledge of NL; see Katz (1981, 1984,

1996, 1998), Katz and Postal (1991), Langendoen and Postal (1984) and Chapter 11 for relevant discussion.

Moreover, even a linguistics oriented toward the study of NL learning need hardly accept claims like those in (1). See Culicover (1999) for a contrasting position based on a highly detailed analysis of a wide array of facts.

2 It is, moreover, hard to understand why, if GB was such a great success, its inventor has jettisoned so much of its content in his transition to the 'minimalist' framework. See e.g. Hornstein (2001: Chapter 1) for some specification and Lappin, Levine and Johnson (2000a, 2000b, 20001), Seuren (2000) for relevant comment.

3 The dismissal of the importance of accurate descriptions seen here is not new and has advanced so far in the past as to yield a claim that one should *hope* one's descriptions are wrong; see Koster (1978: 566); Chomsky ( 1982: 76), Riemsdijk and Williams (1986: 320) and Chapter 9. Noteworthy is e.g. Chomsky (1981: 281), which claims: ''As theoretical work advances and proposals become more significant, we expect – in fact, hope – that serious empirical and conceptual problems will arise. That is what makes progress possible.'' Such material seems designed to make life hard for a caricaturist. The latter is perhaps reduced to imagining a journal devoted to junk linguistics (titled of course the Journal of Non-Junk Linguistics) whose call for papers specifically, in order to maximize progress, limits descriptive analysis submissions to those which their authors *hope* are incorrect.

4 Although the term was not used in any of these works, I believe it would be accurate to characterize much of the material in the two series of articles in (i) and (ii) as in effect previous revelations of, and attacks on, junk linguistics.

(i) Postal and Pullum (1978, 1979, 1982, 1986)

(ii) Postal (1981, 1983, 1984)

And an analogous remark could be justified for Postal and Pullum (1988).

The term 'junk linguistics' is used here essentially to refer to the linguistic subvariety of the term 'spurious science' of the following:

(i) Seuren (2000: 1)

"This book is a sustained argument purporting to show that Chomsky's linguistic theory, especially in the form recently presented as the minimalist programme (MP) (Chomsky 1995a, 1998) is a prime example of

spurious science. It fails to satisfy the basic criteria for sound scientific work, such as respect of data, unambiguous formulations, falsifiability, and also, on a different level, simple good manners."

5 The issue arises as to what extent *self*-deception plays a role for at least some of those involved in junk linguistics. I will have nothing to say about this hard question here. But I do not doubt its relevance in many cases.

6 In other fields, part of the professional curriculum for field X involves courses in professional ethics relevant to X. Presumably, a course in professional ethics for linguists would consider the dangers of junk linguistics and provide help in avoiding its production and uncovering it where it does exist. But in more than forty years in linguistics, not only have I never heard of the existence of a professional ethics course in linguistics, I have never heard anyone even mention the possibility.

7 A current refusal to face the issue of misconduct on the part of the highly placed is arguably particularly ostrich-like as the recent history of the United States shows that wrongdoing on the part of the highly placed is anything but unknown. The person who was the President of this nation prior to the one holding office as I write these words, a person who was a lawyer, was previously fined for contempt of court, recently disbarred in his home state, and denied the right to practice before the Supreme Court, all for well-documented violations of minimal professional standards which took place, moreover, while he was President. One would only kid oneself to imagine that the analog, the violation of standards which should define proper scholarly behavior, is not possible in linguistics, and not possible by senior and influential figures.

8 I tried this not too long ago with respect to the assertion of Smith in his foreword to Chomsky (2000: vii) that the volume's author defends a certain claim "with a series of imaginative analyses". Since I could find *no analyses at all* in the volume, I naturally did not see how there could be imaginative ones. I asked Professor Smith in two e-mails (of July, 2000) on what pages of the cited work anything characterizable as an analysis (in the sense of something acceptable in a refereed journal in linguistics) could be found. The most precise 'information' I could extract though (e-mail of July 12, 2000) was 'passim', a transparent falsehood. This incident illustrates, I believe, that, unaccustomed to demands to justify junk claims, purveyors of them do poorly when faced with such. All the more reason to relentlessly challenge them.

9 The 'one language' claim is also found in Chomsky (1999: 7), who says : "The Martian scientist might reasonably conclude that there is a single human language, with differences only at the margins." A similar claim occurs in Pinker (1994: 232), who says "According to Chomsky, a visiting Martian scientist would surely conclude that aside from their mutually unintelligible vocabularies, Earthlings speak a single

language.'' Note the 'surely'. One sees that the 'one language' claim is not an *isolated*, one time aberration.

10 So Milsark (2001) reviews Smith (1999) quite favorably and nowhere mentions the 'one language' claim.

11 The idea that the generative linguistics under discussion is lacking in real results may seem strange, shocking, even irresponsible, given the vast literature which has over time been devoted to praising it and stressing its supposed accomplishments. But one need not take my word for it. Comments of the inventor of this approach themselves are consistent with the claim. For a telling exemplification, in the course of a relatively recent interview, Belletti and Rizzi (1999/2000), carried out notably by two researchers entirely sympathetic to his approach, the inventor was twice asked to list the results :

"AB & LR Taking for granted the obvious fact that nothing is definitively acquired in empirical science, what are those aspects that you would consider "established results" in our field?
NC: My own view is that almost everything is subject to question, especially if you look at it from a minimalist perspective;"

After that, the interviewee rambled on for eight hundred and twenty four words, without every committing himself to a single established result. Apparently unsatisfied, the interviewers then in effect tried again somewhat indirectly:

"AB & LR: Sometimes speaking with specialists of other disciplines, people ask "what are the results of modern linguistics?" Is there a way of phrasing some of the results, sort of independently from the technical language that makes them opaque for the public at large?"

Again the interviewee responds at length, here for three hundred and forty five words, but once more without invoking a single actual result.

12 See Lappin, Levine and Johnson (2000a, 2000b, 20001) for extensive discussion of this point, as well as responses to a number of critics who strongly disagreed. Levine (2002) also provides a good deal of insight into the quality of minimalist claims.

13 It is hard to see as insignificant the fact that the author of these strange claims is, as of 2002, a member of the associate editorial board of the same journal.