Excerpt from "ODD MAN OUT: REPLY TO REVIEWERS"

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I:

Three very different reviews ... to which I'll reply in turn [29,47,123]. Two of them, written by Paul Thagard and Jerry Feldman, engage with my book seriously. The third, by Noam Chomsky, does not. It's a sadly unscholarly piece, guaranteed to mislead its readers about both the tone and the content of my text. It's also defamatory. But that's par for the course: I'm not the first, and I surely shan't be the last, to be intemperately vilified by Chomsky. However, more on that later.

II:

I'll start with Thagard's review, because this addresses my book as a whole, rather than considering just one or two chapters. Thagard's assessment of the book in general, and of the chapters on AI in particular (and of my discussion of Chomsky, too), is highly positive. Naturally, I'm delighted. But I'll focus here on his critical remarks. And first of all, I'll address his closing questions about the future relations between AI and cognitive science.

[[snip]]

Feldman says that the book provides "a good deal of juicy gossip about personalities and conflicts". So it does. But this material isn't mere trivia. In other words, it wasn't included--as it would be in science journalism--for titillation, or simply to lighten the narrative. It's there for a solid historiographic reason: to show that what the physicist John Ziman [140] has called "the Legend" of purely disinterested science is false (1.iii.b-d).

Personal (and sociopolitical) factors can hugely influence which ideas are favoured or ignored by the scientific community. My discussion of the Lighthill Report, for example, shows that this notorious AI *affaire* was prompted by the unusual personality of one individual, Donald Michie (11.iv and v.c). And the so-called "twenty-year famine" in research-funding for connectionism was due in part (though only in part: 12.iii.e) to a long-lasting friendship between two influential men: Minsky and Joseph Licklider. The history of cognitive science can't be properly understood unless the Legend is specifically repudiated. That's why I gave a wide variety of examples of

decidedly non-Legendary behavior.

And that, as it happens, brings us to Chomsky.

IV:

Anyone who believed in the Legend would be sadly disillusioned on reading Chomsky's review of my Chapter 9 [29]. It's a sorry contribution. Far from being a disinterested intellectual engagement, his essay displays more bile than logic. It systematically misinterprets and misrepresents my text, and also contains outright falsehoods--not all of which merit the charitable label of "mistake".

Some of his claims--for instance (p. 1096), that I chide him for having changed his mind--are so inherently incredible that a cautious reader would take them with a pinch of salt. And his sustained intemperance might also engender doubts. But many non-linguist readers, unaware of Chomsky's habitual manner of argument (of which, more below), may feel that where there's smoke there surely must be fire. Certainly, no-one reading his splenetic attack would guess that I dwelt at length on the hugely beneficial influence that Chomsky's early work had on cognitive science (and computer science, too), nor that I stressed his importance in raising certain core questions in linguistics.

Instead, his readers would infer that my chapter was an intellectual hatchet-job intended to destroy his reputation: in his words, a "campaign" motivated by "her rage and ridicule". Indeed, an American urological surgeon who'd found the review on the web sent me a very funny e-mail, saying "As a urologist, I recognize a 'pissing contest' when I see it".

The point, however, is that it wasn't a pissing contest initially. There was no rage, no ridicule. My chapter wasn't written in a polemical spirit. It wasn't even written in a negative spirit. To the contrary, it recorded--and applauded--Chomsky's crucial role in the founding of cognitive science. It contained some criticism, to be sure. And it quoted more (some polemical, some not). But that's a very different matter. How could an honest intellectual history be written without recording the negative reactions too?

One reason for the web-grazing urologist's misconception lies in Chomsky's overly defensive misinterpretations of words that I had used neutrally--or even in praise of him. For example, he takes my section-heading "That Review!" to be an attack, quoting it sarcastically on several occasions. In fact, it was saluting the huge fame of his review of Burrhus Skinner [20]--which many of my non-linguist readers would already have heard of, even if they hadn't read it. Similarly, he misreads my reference to his general political disagreement with Skinner. He says that I claimed to find political content in his review of Skinner's *Verbal Behavior* (which I did not), and that like charges are "repeated throughout" my chapter (also false). What's more, he assumes that my remark was intended as criticism--even though it occurred in the context of my commending his bravery in the political sphere, comparable (I said) to that of Bertrand Russell, who suffered prison on more than one occasion as a result of his political views (p. 641).

The key reason why the American MD was misled, however, lies in Chomsky's unscholarly strategy of continually quoting contemptuous terms picked out of my text as though I had used them myself. I had not. Rather, I had quoted them--from critiques of Chomsky written by professional linguists and psycholinguists. His inability to see the difference leads to misinterpretation over and over again. It follows from the elementary distinction between *mention* and *use* that quotation doesn't necessarily imply agreement. In particular, the barrage of negative terms with which he opens his review contains many that weren't endorsed by me.

In most cases, I didn't endorse them because I didn't share the emotion expressed by them.

(See my remarks, below, on the unfortunate within-discipline effects of Chomsky's rhetorical style.) In some cases, however, I pointed out that I was in no position to endorse them. I said, for instance, that I lack the mathematical skills that would enable me to vouch for the judgments (quoted on pp. 651 and 654) that Chomsky's argument in *The Logical Structure of Linguistic Theory* was often "maladroit" and "perverse", and sometimes "just plain wrong" [107, p. 366], or that Chomskyans often state "purported 'theorems' ... without any proof being suggested, or theorems that are given 'proofs' that involve no definition of the underlying class of grammars and are thus empty" [53, p. 14].

Notice, however, that (as I reported) the two internationally distinguished linguists quoted here had assured me that they meant exactly what they said. I'd pressed them hard on this matter because, having previously accepted the widely-held belief that he always achieved mathematical rigor in his work (see below), I'd found their claims highly surprising. This undermines Chomsky's charge that "Her conception of an argument, repeatedly, is to quote someone who agrees with her judgments. QED": in some cases, I had no such prior judgments, being agnostic on the matters concerned.

So why, you may ask, did I mention the adverse judgments of others? Why not emulate Thumper's mother, who famously advised her baby rabbit that if he couldn't say anything nice then he shouldn't say anything at all? Or, if I had to report that some people disagreed with him, why also cite their less-than-temperate words? Weren't these startling quotations simply academic muck-raking--alias a pissing contest?

No, not at all. Another widely-held belief (again, see below) is that his linguistic theory is "indisputably" correct in its essentials. In other words, that all serious linguists today accept his approach. The quotes referred to above show that they do not--indeed, that many reject it with an unusual degree of passion. Just why Chomsky has attracted such venomous opprobrium from his disciplinary colleagues is an interesting question, to which I'll return at the end of this *Reply*.

As he remarks, the historical and scientific issues central to my Chapter 9 arose within my discussion of what I called "the tenfold Chomsky myth" (p. 592). So it may be helpful if I list its components here:

(1) Besides giving a *vision* of mathematical rigor,
    he always achieved it in his own work.
(2) His linguistic theory was (or is now) indisputably correct in
    all its essentials.
(3) The nativist psychological implications he drew from it were
    convincingly argued at the time, and
(4) they are now empirically beyond doubt.
(5) His work of the 1950s was wholly original.
(6) His writings of the 1960s were, as he claimed, the culmination
    of a tradition of rationalism dating back 300 years.
(7) Without Chomsky's grammar there would have been no computer
    modelling of language.
(8) Chomsky was responsible for the demise of behaviorism.
(9) He reawakened and strengthened the discipline of linguistics.
(10) Linguistics is as prominent in cognitive science today as it
    was, thanks to Chomsky, in the late 1950s to 1970s.

Each of the laudatory beliefs in the myth, I said, is near enough to the truth to explain why it's

so widely accepted. Indeed, as remarked above, I'd never questioned item-1 myself before doing the research for this chapter. Nevertheless, interpreted strictly, each is false. The least defensible is item-10: that linguistics, especially Chomskyan linguistics, is as prominent in cognitive science today as it was forty years ago. The fact that it isn't surprises many people, and cries out for explanation--which I provided (see below). The most defensible is item-9. That he reawakened and strengthened the study of syntax (and semantics) is undeniable. But that he did this for linguistics as a whole is not. In some ways, as we'll see, he set the discipline back.

Before saying anything else, I must make one important clarification. It concerns a misinterpretation on Chomsky's part that was largely my own fault. At the outset of Chapter 9, I used a phrase whose meaning I thought was clear in context but which was in fact ambiguous. And for that, I apologize.

Specifically, I contrasted those people who, because of his courageous political writings, take Chomsky as their "political guru" (making him the most widely quoted living writer, and the eighth most-quoted of all time [5, p. 3]) with "those who uncritically [sic] take him as their scientific guru". By the latter phrase, I meant people on the outskirts of cognitive science, or students having only a superficial knowledge of it. This group, I said, accept most or all items of the myth.

That they do so has indeed been my experience. However, since the people I was thinking of were outsiders and/or beginners, citations would have been inappropriate. A similar point, relevant to myth-items 2-4, was recently made by someone else: "The general public still appears to see Chomskyan linguistics and the idea of innate cognitive structures as an unchallenged consensus" [109]. Moreover, Chomsky's arcane text *The Minimalist Program* (1995) was hailed in a British national newspaper in 1998--not in a book review, nor even a feature article, but as a quarter-page news-item. Clearly, the editor accepted, and expected his readers also to accept, item-10: the belief that Chomsky's linguistics is as prominent in the scientific study of mind today as it was many years ago.

I now realise, however, that--despite my deliberate insertion of the word "uncritically"--the second phrase could be supposed to mean *Chomskyan linguists.* That's especially likely if it were read--as Chomsky admits it was, by him--in the context of Chapter 9 alone, ignoring the rest of the book. However, a Chomskyan who did read some other chapters interpreted the phrase in the same way, saying "[she clearly implies] that Chomsky and his associates have somehow developed a largely illusory belief system" and he, too, complained about lack of references [102]. Had that been what I meant, it would indeed have been remiss not to give references. So the complaint that I didn't do so is understandable.

Chomsky's other charges of lack-of-references, however, are mostly false. Many of the 5,000 bibliographic items remarked on by Thagard were cited in this chapter. And some of those attracted multiple page-references: *Syntactic Structures (SS),* for example (notwithstanding Chomsky's claim that I "scarcely looked at" it). At one point (p. 593), I omitted a name deliberately. I mentioned a conversation with a young linguist from MIT who had never heard of an important competing theorist. I didn't identify this person: the fault wasn't his/hers, but lay rather in the standards of scholarship surrounding them at MIT.

Although I did not ascribe belief in the tenfold myth to Chomsky himself, nor to Chomskyans in general, it seems to me to raise many key concerns of his work.

He disagrees: he complains that "the topics that have been of primary interest to me in linguistics, philosophy, and cognitive science" are alluded to "only tangentially, if at all" (p. 1094). Since he coyly omits to say what these are, it's difficult to counter his remark. Certainly, I chose not to discuss his later theories in any detail, because (contra myth-item 10) they haven't

featured in cognitive science. But several Chomskyan topics which did affect it intimately are discussed in the chapter he read. Others, to which cross-references are given, are considered elsewhere in the book. These include his influential work on the competence/performance distinction, on nativism in psychology and the philosophy of mind, on modularity, and on internal representations (see especially 7.iii.a, 7.vi.passim, 12.x, and 14.viii).

Most of Chomsky's complaints about my chapter relate to myth-items 2-4. These concern science more than history, although some historical points do arise. Before considering what he says about nativism (items 3 and 4), I'll focus on what he says concerning his theory of language as such--the topic of myth-item 2.

One aspect of myth-2 has already been discussed: the mistaken notion that no serious linguist "disputes" Chomsky's approach. The other aspect--that his linguistic theory actually was/is essentially "correct"--can't be dealt with so quickly.

Whether (any version of) Chomsky's theory is correct depends, as he repeatedly says in his review, on what the "empirical facts" about language actually are. He accuses me, again and again, of neither knowing nor caring about this question. But he fails to point out that the answer depends, in large part, on a tricky theoretical/methodological problem: *what sort of thing,* in principle, an empirical fact about language is. And that is something which I did address in my chapter. I said (p. 630) that, in making judgments about grammaticality, Chomsky's reliance on the intuitions of native speakers, and especially his habit of relying primarily on his own intuitions, didn't satisfy "data-respecting critics" in the late-1950s, and doesn't satisfy them now. "Such a source," I remarked, "hardly seems reliable".

He objects vociferously, agreeing with my statement about reliability but denying that it applies to him. His self-defence is that "Boden was apparently unable to discover someone to quote" (p. 1096). Well, it's true that I didn't give a verbatim quotation. But I did refer to his tutor Zellig Harris's sage advice--published in the same year as *SS*-- on how intuitive grammaticality-judgments needed to be carefully controlled so as to avoid experimental bias of various kinds [58, sect. 4]. I didn't add any references published long after *SS,* because the point seemed so obvious. So let me now satisfy Chomsky's request for further chapter and verse.

The classic source on this question, which was largely prompted by Chomsky's continuing failure to take Harris' advice seriously, is the paper by the sociolinguist William Labov on "Empirical Foundations of Linguistic Theory" [69]. This was Harris's warning with knobs on. Labov showed conclusively that the intuitive judgments of the native speaker (although useful as a first port of call [69, p. 103]) are not a reliable indication of grammaticality. In other words, they are not a proper source of linguistic facts.

Labov gave various reasons why "the uncontrolled intuitions of linguists must be looked on with grave suspicion" (p. 102). But "the most damaging body of evidence on the weakness of intuitive data", he said (p. 104), was research showing that someone may intuitively reject a certain word-string as ungrammatical which on other occasions they are happy to use. For instance, he had observed "hundreds" of examples of a Philadelphia dialect in which *anymore* was used to mean *nowadays* (97f., 106-8). Native speakers might insist that a word-string employing *anymore* in that way is (a) never used, and (b) uninterpretable, *even though* they had been heard to use it themselves. As Labov commented (p. 107), "This puts us in the somewhat embarrassing position of knowing more about a speaker's grammar than he does himself".

His explanation was that the Philadelphians had been unknowingly influenced by faulty teaching in the schoolroom: their theoretical ideas about what their language is like had blinded them to the empirical realities of what it actually is like. Academic linguists aren't immune to this type of bias. They are subject, too, to what psychologists term "the experimenter effect",

wherein people unconsciously behave as the experimenter expects them to behave [103].

It followed, of course, that "Chomsky should not accept his own judgments on [any] issue in which he has an established theoretical position" (p. 101f.). Worse (from Chomsky's point of view), "linguists cannot [i.e. should not] continue to produce theory and data at the same time" [68]. And the killer: in the case of any disagreement on intuitions, "the judgments of those who are familiar with the theoretical issues may not be counted as evidence" [69, p. 103].

This was a damning indictment of Chomsky's methodology. Nevertheless, Chomsky continued to accept intuitive judgments (often his own, or those of his students) as his prime source of "linguistic facts". His occasional admissions that native speakers' intuitions are not "sacrosanct and beyond any conceivable doubt," and that "their correctness can be challenged and supported in many ways ..." [22, p. 939] were usually thrown to the winds. He would call his own intuitions "facts" or "data", while calling his rivals' intuitions mere "factual claims" or "interpretations" [69, p. 101]. When he (atypically) allowed that experimental tests of other people's intuitions could be relevant, this was because--so he reported, or predicted--they would align with his own [60, pp. 19 and 74].

Given Chomsky's sustained refusal to heed Labov's warnings, myth-item 2 is highly questionable. No matter what specific claims about syntax are in question, the general point about how (not) to discover linguistic facts suffices to undermine them. (Undermine, not disprove: to question myth-2 is to suggest some reason to doubt whether his theory is correct, not to try to show that in fact it isn't.)

It's relevant, here, that Chomsky's list [29, p. 1096] of alternative types of evidence for discovering linguistic facts doesn't include linguistic corpora--that is, collections of naturally-occurring utterances, culled from a variety of spoken and published sources (mentioned in my book at pp. 624, 681-683, and 1449). True, he says his list isn't "exhaustive"; and he even adds "in fact any source" at the end. But it's significant that he fails to mention corpora, because these are the most plausible alternative to intuition as a way of discovering "empirical facts" about language.

Until the early 1960s it was understandable that linguists put their trust in intuitive judgments (whether or not they also heeded Harris's or Labov's warnings), because there were no computers capable of analysing large corpora--and no such corpora available to be analysed. Indeed, in a discussion in 1958 in which structuralist linguists were recommending the use of corpora and Chomsky was denying their relevance, someone said: "Well, if I had three months of speech on tape, it would have to be carded and sorted by an IBM machine of pretty big proportions, before I could go to the corpus to answer any questions. I think such a program is unlikely in a practical world" [60, p. 78].

Chomsky wasn't worried about the practicalities, for he dismissed the very idea of relying on corpora. In this discussion, and in subsequent years, he insisted repeatedly that knowledge of linguistic facts was available from the intuitions of the native speaker.

Moreover, his own intuitions were treated as paramount: "The trouble with using a corpus is that some authors do not write the English language. Veblen, for example, speaks of 'performing leisure', and the verb *perform* cannot take such an object [i.e. a mass-word]" ([60, p. 28). In other words, any actual usage recorded in a corpus can be ignored if it doesn't fit Chomsky's theory. And this applies not just to the false starts and unintentional mistakes common in spontaneous speech, or the syntactic 'solecisms' committed by the unschooled, but even to the carefully considered constructions of experienced native-speaker writers such as Thorstein Veblen. In defence of his remark about the grammar of *perform,* Chomsky said: "How do I know [if I haven't used a corpus]? Because I am a native speaker of the English language" (p. 29). Veblen,

seemingly, wasn't. (So much for the claim that "I've never hinted at the crazed belief [i.e. that my own intuitions were paramount] Boden attributes to the Chomsky of her imagination, which is why she cites nothing": p. 1096.)

Today, there's even more reason to doubt the scientific usefulness of linguistic intuitions. Huge corpora, and the "IBM machines" to deal with them, do now exist: the British National Corpus, for example (100 million words, including 10 million of speech). These enable linguists to make objective (statistical) judgments about the usage of certain word-patterns, even of constructions that occur very rarely indeed (currently, frequencies as low as 1 in 131,302 words [109]). Admittedly, corpora aren't unproblematic. Besides the obvious danger of skewed sampling (of which there are many possible varieties), there may be theoretical problems too: some corpora aren't 'raw', but are tagged by means of syntactic labels, about which there could be principled disagreement of various kinds. Nevertheless, these utterance-collections constitute a source of relatively concrete evidence about language. (Chomskyans use corpora when they have to: in studying infants' language or dead languages, for example. But when native-speaker intuitions are available, they rely on them.)

In short, the notion that the "empirical facts" of a science (sic) of linguistics can be culled from personal intuitions is now even shakier than it was when Harris and Labov questioned it many years ago. Most linguists today would say that it's bizarre. In that--very general--respect, myth-item 2 is false.

A professional linguist might add that Chomsky's more *specific* claims about syntax are mistaken, too. I myself would not--not because I accept them, but because I am agnostic about them. My references to his A-over-A principle, for instance, were given merely to illustrate the nature of his claims, and other people's counter-claims, about the content of universal grammar (9.vii.d). Nor did I discuss any individual hypotheses within his principles and parameters, or P&P, theory (see below). So Chomsky's constantly reiterated complaint that I have no interest in the empirical facts about language meets its mark if interpreted *at that level of specificity.* As a non-linguist, I'm not concerned with the theoretical details.

More to the point, as a cognitive scientist I would be interested in them only if they were relevant to psychological studies of language--and/or to NLP, as the rival Generalized Phrase Structure Grammar, or GPSG [53], is (9.ix.d-f). Chomsky's early theories, set out in *SS,* were psychologically relevant--or anyway, they seemed to be. His "derivational theory of complexity" [82] attributed psychological reality to the specific grammatical transformations defined in *SS,* and prompted many interesting experimental studies before being abandoned. But that is no longer true. Psycholinguists today no longer hang on Chomsky's every word for theoretical/experimental inspiration, and cognitive scientists in general rarely mention his name.

That point could jump us straight from myth-item 2 to myth-item 10--and to Chomsky's complaint about my describing linguistics as "eclipsed". However, I'll postpone that discussion until later. First, I'll consider Chomsky's comments on nativism--which is to say, myth-items 3 and 4. These concern whether the pro-innateness arguments offered by Chomsky in around 1960 were "convincing", and whether the empirical evidence amassed since then has put them "beyond doubt".

Much as my chapter didn't aim to prove that Chomsky's grammatical theory is mistaken (although it did make highly favourable comments about the rival GPSG theory), so it didn't seek to prove that linguistic nativism is mistaken. The best critique of nativism that I know of is the admirably clear attack written by a Sussex colleague, the linguist Geoffrey Sampson [108]. But despite Sampson's careful, and often persuasive, arguments, I remain open-minded on the point--and I said as much on p. 594.

However, Chomsky claims in his review that to have an open mind here is absurd. It is equivalent, he says, to seeing his granddaughter's achievement in learning language--which her pet kitten, a chimp, or a songbird cannot do--as "a miracle" (p. 1095).

That argument (like his reliance on intuitions) isn't new. In 2000, for instance, he opined that "To say that 'language is not innate' is to say there is no difference between my granddaughter, a rock and a rabbit....So people who are proposing that there is something debatable about the assumption that language is innate are just confused. So deeply confused that there is no way of answering their arguments. [Except, perhaps, to accuse them of belief in miracles?]" [30, p. 50]. Moreover--and again, like his reliance on intuitions--it has already been roundly refuted. (Refuted, not just rebutted.)

For example, Paul Postal has pointed out that "Since rocks and rabbits lack general human attributes *of every sort,* any failure on their part to learn languages ... would, necessarily, entirely fail to distinguish the hypotheses at issue" [99, p. ix]. These are (in my words, not Postal's): (a) that language learning depends only on general aspects of human intelligence (some of which may be unique to *Homo sapiens),* and (b) that it depends also on inborn language-specific knowledge--for instance, on expectations about the abstract form of human languages in general: the so-called Universal Grammar (UG). Miracles, in other words, have nothing whatever to do with it.

Notice that there is no third hypothesis here, suggesting that language acquisition can be "fully explained by experience". The notion that there is *no* innate preparation for language, that the newborn mind/brain is a *tabula rasa,* is absurd. Chomsky was right about that. Quite apart from our neuroscientific knowledge, *pure* induction, with no prior guidance whatever about what patterns to look out for, is impossible--as argued in Chapter 8.vi.f. (The statistical ADIOS algorithm [44] can induce both context-dependent and context-free grammars without being given any grammatical cues; but it has to presuppose that the input sequence contains partially overlapping strings at multiple levels of organization: 9.ix.g.)

Postal's response wasn't new either. In essence, Willard Quine [100] had said the same thing: "[The behaviorist is] knowingly and cheerfully up to his neck in innate mechanisms of learning-readiness ... [and] unquestionably much additional innate structure is needed, too, to account for language learning." Empiricism (as opposed to rationalism), he said, "sees nothing uncongenial in the appeal to innate dispositions to overt behavior, innate readiness for language-learning. What would be interesting and valuable to find out, rather, is just what these endowments are in fact like in detail". Those details might turn out to confirm either one of the hypotheses (a) and (b) listed above.

Chomsky remarks that Quine, in this paper, retracted his previous methodological critique and stated that "generative grammar is what mainly distinguishes language from subhuman communication systems". He takes this to be an acceptance of his own form of linguistic nativism, i.e. a version of hypothesis (b).

The quotation is correct, but the interpretation isn't. To say that language is characterized by "generative grammar" is ambiguous. It may mean merely that language has a hierarchical structure wherein grammatical sentences can be (endlessly) generated by a finite set of syntactic rules--perhaps those specified by Chomsky. Or it may mean, in addition, that language depends in part on inherited language-specific mechanisms that determine the abstract structure (UG) of all human tongues. The former interpretation could be true even if the latter were false. There's no reason to think that Quine accepted the second, stronger, interpretation. At most, he allowed that it was an empirical possibility, whose investigation would be "interesting and valuable".

In brief, myth-item 3 was shown to be false many years ago: Chomsky's early arguments

weren't "convincing". (This is compatible, of course, with his empirical claims being true.)

As for myth-item 4, that's a mixed bag. Research on children's language acquisition, and on what Quine called "subhuman communication systems", was hugely stimulated by Chomsky's early work (see 7.vi passim). That's just one of the many ways in which he has had a beneficial influence on cognitive science. Research on other species has greatly increased our understanding of animal communication--for example, in insects, parrots, dolphins, and apes. But, despite various energetic attempts to show the contrary, it appears that language--broadly: a communication system having a generative grammar (in the first interpretation distinguished above)--is indeed confined to *Homo sapiens,* as Chomsky claimed. Moreover, various aspects of our oral and respiratory anatomy seem to have evolved as adaptations enabling speech. Or rather, they have evolved, and they enable speech: whether they are adaptations specifically for language depends on the choice between hypotheses (a) and (b), above [74,75].

On the other hand, and as I remarked in response to Feldman, connectionist models of the acquisition of the past tense (see 12.vi.e and 12.viii.e) have suggested that grammatical structures can be learnt without any reliance on internal syntactic *rules,* whether innate or not [95,96,97,105]. In particular, the transitory over-generalizations (such as *mouses, goed* instead of *mice, went)* regarded by Chomsky as explicable only in terms of rules within the infant's mind occur, for purely statistical reasons, in PDP networks. Chomskyan linguists have challenged this research, pointing out (among other things) that the behavior of the networks doesn't entirely match that of the young child [94]. My own view is that the question remains open: Chomsky's claim that we employ internal rules of grammar hasn't been conclusively refuted.

Two other things would really strengthen myth-item 4. First, if Chomsky's abstract theory of universal grammar were shown by linguists to fit all known languages. As he admits, that's not yet the case. (Some linguists argue that it will never be the case: either there is no universal grammar, or if there is then it's not Chomskyan.) Second, if non-linguists were to discover that the inborn psychological and/or neurological mechanisms enabling language acquisition include some which are clearly language-specific.

No such mechanisms have yet been found. Particular aspects of our oral/respiratory anatomy may be exploited by language although initially evolved for other purposes (see above). Similarly, mirror neurons are thought by some people to be crucial in the development of language [2], but they function in respect to motor actions in general. That is, they may support hypothesis (a), but not hypothesis (b). Moreover, they are found in some nonhuman species, so can't explain why only human beings acquire language. And some cognitive scientists argue that non-communicative (internal) "generalised languages", with structural variability and compositional semantics, are needed even for representing spatial structures and planning movements in space; already present in many animal species, these might be the evolutionary base of human (communicative) language [115].

Future cognitive neuroscience may discover language-specific mechanisms. And these may, perhaps, include mechanisms underlying a Chomskyan UG, an abstract structure common to all languages. At present, however, these questions remain open.

As the latter points suggest, part of the empirical evidence relevant to myth-item 4 might come from evolutionary psychology/neuroscience (7.vi.d-e, 8.v passim). In his review (p. 1101), Chomsky takes me to task for saying that he claimed there could be no evolutionary explanation of language. However, I stand by what I said, for I did not misrepresent him.

While he mentions my quoting him on "emergence", he fails to mention that I also quoted this: "It is perfectly safe to attribute this development [of human minds and language] to 'natural selection', so long as we realize that there is no substance to this assertion, that it amounts to

nothing more than a belief that there is some naturalistic explanation for these phenomena" [27, p. 83].

He also fails to mention that I cited a more recent passage, written in 1999 [28], in which he tentatively *("might", "Maybe")* suggests the possibility of a spontaneous self-organization such as was described by Turing. (Turing's "evo-devo" influence, triumphantly mentioned by Chomsky as though I'd never heard of it, was discussed at length in Chapter 15.iv.) To understand the *possibility* of emergence and/or self-organization is by no means to understand how a particular case actually happened. So even if the evolution of language is no longer "a total mystery" (as Chomsky put it in 1986), since it no longer appears utterly unintelligible, he seems still to be pessimistic about our ever specifying the explanation. (For what it's worth, I share his pessimism here.)

The upshot of these remarks is that Chomsky's linguistic nativism--positing innate dispositions that predispose the baby to some abstract structure that's universal to all languages--was dubious when it was first put forward, and remains so today. (The well-known Chomskyan Steven Pinker has conceded that "UG has been poorly defended and documented in the linguistics literature" [92].) That's true even though the current (epigenetic) understanding of nativism, mentioned above in reply to both Thagard and Feldman, is more nuanced than the binary 'nature or nurture?' divide favored in Chomsky's youth. In other words, and despite Chomsky's claim that anyone who doubts them is "just confused", myth-items 3 and 4 are both false.

Let's turn now to a few of Chomsky's other complaints. (Only a few: his nine pages of abuse contain far too many for individual attention.) First, concerning my discussion of item-5 of the myth: that his early work was wholly original. This attracted the charge that I made "energetic efforts (as always, without evidence) to show that [generative grammar] was all borrowed from the prevailing structuralist approaches" [29, p. 1098].

What nonsense! I pointed out some very broad similarities between Chomsky's early work and that of Otto Jespersen, citing Jespersen at length in so doing. In addition, I outlined some closer similarities between Chomsky's work and that of his teacher Harris--again, cited carefully (9.v.c-d). But I also pointed out crucial differences between Chomsky and Harris (9.v.e), and described Chomsky's generative grammar as "largely novel" (p. 595), and so new as to be "shocking" to the linguists of the time (p. 629). (That word "shocking" was of course meant positively: appreciation, not denigration.)

If Chomsky doesn't regard my many specific references to Jespersen and Harris as constituting "evidence", he might prefer to consult his own address to the Linguistic Society of America in 1975 [26]. He opened by speaking in warm terms of Jespersen, even saying (p. 161): "Jespersen's own view of the matter [i.e. the possibility of a universal grammar] is subtle and complex, and I think, generally persuasive". He also said that Jespersen's work, although it had "a great deal of merit", was "perhaps, premature" (p. 166), and closed his paper by remarking that his own work "extends and advances the program that [Jespersen] outlined" (p. 196). That was the very judgment which I argued in my chapter--making it clear that the extensions and advances here were considerable.

Having re-read his paper of 1975, he might care to turn to what he said in 1958, at the first major conference wherein his theory was presented: "[My] approach to syntax ... developed directly out of the attempts of Z. S. Harris to extend methods of linguistic analysis to the analysis of the structure of discourse" (60, p. 124). Again, a judgment for which I gave scholarly chapter and verse in my text.

But we encounter an embarrassment here. In his review, Chomsky says: "There was a tradition of something like generative grammar, later [sic] unearthed by 'Chomskyans', tracing from

classical India to Leonard Bloomfield.... But there is no hint of the tradition in the work of the structuralists she mentions, for a very good reason: it was completely foreign to their approaches to language, contrary to her unsupported assertions" (p. 1098). The embarrassment arises because, in the context of his review, this contradicts what he said in both 1958 and 1975. So it appears either that Chomsky was dissembling years ago, when he acknowledged the influence of Jespersen and Harris (perhaps to curry favour with the orthodoxy?--but that was never his style), or--very much more likely--that he is now shamelessly denying the historical facts, simply in order to grind me into the dust. In short, he cannot be relied on to tell the truth.

As for what follows from acknowledging the links with Jespersen and Harris, Chomsky misrepresents me yet again. To say that his early theory shared some of the structuralists' goals, and was even reminiscent of some of their methods, is not to say that his work was "all borrowed from" them. Creativity in general, as I've argued at length elsewhere [12], doesn't spring from nothing. There will always be some associative and/or structural links to past thinking, some seeds of later ideas. To trace those links/seeds is by no means to deny the creativity of the later thinkers. Rather, it is to set them in their historical context--which is what an intellectual history is supposed to do. (So, again: no rage, no ridicule.)

Links and seeds, of course, aren't the same as mere conceptual similarities--which may exist without there being any actual historical influence. My distinction between "predecessors and precursors" (9.ii.a) made this important point. Before applying it to Chomsky's immediate forerunners (the structuralists), and thereby to item-5 of the myth, I applied it at some length (in 9.ii.b-c and 9.iii-iv) to a number of much earlier writers--and thereby to item-6 of the myth.

Subscribers to the *AIJ* wouldn't welcome a spate of scholarly nit-picking on these matters, so I shan't defend my comparisons between Chomsky and Port Royal or Wilhelm von Humboldt against Chomsky's criticisms [29, p. 1102]. Indeed, *AIJ* readers might say that they simply couldn't care less: if myth-item 6 happens to be false, so what?

Well, there are three reasons why it matters. First, anyone who makes a point of situating their own work within an ancient, and no longer fashionable, tradition must expect serious readers to consider their claim carefully. It should not simply be taken for granted, still less uncritically repeated to others--such as the readers of my book. (I didn't claim to be a "specialist" here, by the way--a sneer occasioned by my saying that I knew from my own experience that a seventeenth-century item that Chomsky had declared to be forgotten was routinely recommended to undergraduates in the 1950s [13, p. 596]. However, one genuine specialist has buried myth-item 6 very deep indeed, saying that Chomsky's history of linguistics is "fundamentally false from beginning to end" [1].)

More important, for cognitive science as a whole, the far-from-monolithic "rationalist" tradition is largely concerned with the notoriously tricky philosophical/psychological concept of innate ideas in general, and of linguistic nativism in particular (see 2.vi.a and 9.ii). Here, problems arise with respect not only to truth but also to meaning: *just what* were the (various) writers in question intending to say? Until one knows that, one is in no position to assess the importance of rationalism for cognitive science in general.

The third reason is especially relevant for *AIJ* readers. The rationalist writers named by Chomsky said things about language that are relevant to the prospects for NLP. Humboldt, for instance, discussed the creativity of language use, its cultural (and even individual) specificity, its relation to thought (cf. the Sapir-Whorf hypothesis: 8.i.a), and the manifold difficulties of translation. Critics of NLP often describe language in similar ways. The ALPAC Report of 1964, which stalled work in machine translation (MT) for almost a generation, was one case in point (9.x.e-f and 9.xi.a).

MT, already being explored in the 1940s (and envisaged in Russia in the 1930s), is just one example showing the falsity of myth-item 7. The history of NLP in general, and of Chomsky's deep scepticism concerning it [13, p. 673], was sketched in Chapter 9 of my book (9.x-xi). I'll ignore it here, however, as Chomsky doesn't address the NLP sections in his review.

With respect to myth-item 8 (regarding the fall of behaviorism), Chomsky accuses me not of denying his originality but of ignorantly exaggerating it [29, p. 1099]. In truth, the ignorance here is his, caused by his failing to follow up, or even to notice, my cross-references.

According to him, I said that cognitive scientists, himself included, had overlooked Karl Lashley's paper on serial order in behavior [73]. Moreover, he complains that I didn't realise that the early ethologists had had much of relevance to say. He's mistaken on each count. I'd already discussed Lashley at length (5.iv.a and e). I'd even pointed out that his serial-order talk was converted from "a mini-sensation" at the Hixon Symposium in 1948 into "a genuine sensation" in about 1960 by Miller and--guess who!--Chomsky [13, p. 266]. Similarly, I'd outlined the work of the pioneers of ethology, including four of the names listed by Chomsky, explaining how their views differed fundamentally from the behaviorist orthodoxy (5.ii.c). (Other important figures in the demise of behaviorism included Miller, Bruner, Donald Broadbent, Richard Gregory, and--of course--Simon and Newell: see Chapter 6, passim.)

Chomsky is especially scathing about my rejection of myth-item 10, the belief that linguistics is as prominent in cognitive science today as it was in its first quarter-century. He has fun mocking my pithy sub-heading "Linguistics eclipsed", asking "Since linguistics is the study of human language, it is a remarkable feat to have 'eclipsed' it. How was that achieved?".

Someone with his keen sense of rhetoric should know better than to take my phrase literally. But I'll gladly answer his question (as I did in my chapter). The "eclipse" happened within cognitive science, not within academia as a whole. It concerned not linguistics in general but Chomskyan linguistics in particular. And it was achieved by Chomsky himself, through various changes in his theory.

Chomsky's early "derivational" theory of transformations, which made psychological as well as purely linguistic claims [82], excited psycholinguists (see 6.i.e and 7.ii.a-b). They found some suppporting evidence for it [6,48]. But two things happened to discourage them. On the one hand, the experimental evidence for the psychological reality of transformations started to weaken [50], and the theory was later declared defunct by some of Chomsky's closest colleagues [49]. On the other hand, Chomsky changed his theory. Specifically, *SS* was ousted by *Aspects of the Theory of Syntax* [23]. This introduced "standard" theory, which--by reducing the number of transformations in his grammar--took the first steps on the theoretical road I referred to as "Transformations trounced".

The psychologists at that time were dismayed, not to say aghast. As the experimentalist James Jenkins put it [65, p. 243], "Chomsky pulled the rug out from under us... [We] were very busy trying to find the [psychological] apparatus for a theory of linguistics that at that moment was being discredited.... [By] the time we could supply the right kind of theory, the nature of what language was believed to be had changed. The whole theory was no longer appropriate. Very grim, very grim".

Grim or not, if Chomsky's new theories (standard theory was only the first of several fundamental changes) had been as psychologically relevant as his early ones, then psycholinguists and other cognitive scientists would have had to pay the price of struggling to keep up with him. (I said this on p. 667, so his accusation that I criticized him for changing his mind is absurd: see Karl Popper's [98] account of science as "conjectures *and refutations",* cited at various points throughout the book.) But they were not. Eventually, the major problem became

the forbidding abstractness of his work, in the P&P theory and the minimalist program (MP).

Another key problem (from the late-1960s on), from the experimentalists' point of view, sprang from the competence-performance distinction (CPD). The CPD, which defined a form of psychological explanation that abstracted from the performance details, was hugely influential in the early years of cognitive science (see 7.iii.a).

It was welcomed enthusiastically by Marr, for example, who applied it in his theory of low-level vision [78]. And, as Chomsky rightly says, it soon led to "rich and important" work in developmental psychology--detailed in 7.vi.a (which disproves the slur that I omitted even "the most casual investigation of the literature" [29, p. 1101]). So it didn't instantly put an impenetrable firewall between theoretical linguistics and experimental psychology. Nevertheless, it tempted psychologists to ignore performance details that could not be fitted into the abstract linguistic theory. The characteristics of anxiety-ridden speech [36], for example, wouldn't have been studied by Chomskyans (see 7.ii.c).

As the abstraction continued to increase, the firewall became even stronger. It became very unclear just what experimental evidence, as opposed to the data available in intuitions and/or corpora, could confirm or disconfirm the linguistic theory. It was also unclear just what new lessons his later theories had to teach us about the nature of mind in general. That's why I said very little about P&P, and even less about MP.

In discussing P&P, I pointed out that it is potentially relevant to the recent revival of interest in nativism as epigenesis. As I said in my reply to Thagard, this concept is prompting exciting work in developmental psychology, neuroscience, and biology, and has also influenced A-Life--and even robotics (7.vi.g, 14.ix.c-d, 15.viii.a-b). Piaget had been expounding epigenesis for many years, with scant support from Chomsky [91] (5.ii.c). But only relatively recently has the concept surfaced in cognitive science. Even so, the possible relevance of P&P is rarely, if ever, remarked.

The details of P&P (assuming that they can be confirmed by linguists) could be interesting to cognitive scientists in other ways, too. For instance, one Chomskyan has tried to couch empirical evidence about the acquisition of different languages in P&P terms, including suggestions as to why this or that parameter is chosen in a particular case [56]. Another has applied P&P to historical linguistics, seeking to explain why certain patterns of grammatical change occur repeatedly [101].

Because of this untapped potential, I had intended to give illustrations of one or two suggested parameters in my chapter. But I found, when drafting, that this would take up more space than would be warranted. What's more, I wasn't at all sure that I could understand these highly abstract matters well enough to do so. (For helpful recent discussions, see [4,133].) Perhaps other non-linguists have the same problem. Whatever the reason, the fact remains that there is no mainstream cognitive-science interest in P&P.

As for minimalism, this too has been pretty much ignored by non-linguists, newspaper articles notwithstanding (see above). In principle, MP would be of greater interest than P&P to *AIJ* readers, because it aims to show how general considerations, such as computational efficiency and optimal search, could lead to UG being like *this* rather than *that* [102]. In the terms used above, it seeks to minimize (sic) the domain-specific UG component, so as to push nativism as far as possible from hypothesis (b) towards hypothesis (a). But in practice, it is far from being a shopping-list for work in computer science, NLP, or psycholinguistics.

Had Chapter 9 been intended as a history of Chomskyan linguistics as such, my very brief treatment of P&P and MP would have been a serious fault. (Similarly, a history of cognitive psychology as such would have discussed reaction-time techniques, and Bayes' theorem too: see my response to Thagard, above.) But it was concerned, rather, with how Chomsky's various

theories have influenced cognitive science. In these two cases, they haven't.

The falsity of myth-item 10 has been reluctantly acknowledged by the Chomskyan Ian Roberts [102]: "Unfortunately, this [i.e. my phrase 'Linguistics eclipsed'] is a largely correct observation". It's conceded also by Chomsky's ex-pupil Ray Jackendoff, who has allowed that the discipline is now "arguably far on the periphery of the action in cognitive science" [64, p. 651]. Admittedly, types of linguistics that aren't focussed on syntax remain, and are much further in from the "periphery". I've already granted, in reply to both Thagard and Feldman, that my discussion of cognitive linguistics in Chapter 9 was far too brief (even though I explored some relevant theories in other chapters: 7.iii.d, 8.vi.e, 12.x.g). Linguistics may regain its place in cognitive science--if not as a *root* of theorizing in the other disciplines, at least as an equal, and collaborative, partner. If that happens, however, it will be a very different enterprise from that engaged in by Chomsky. In short, "eclipsed" seems about right.

And yet, and yet.... If Chomsky's name is rarely mentioned today by non-linguists (except perhaps by those working on nativism/evolution), and if his current theories are addressed even more rarely, his general contribution hasn't been eclipsed. To the contrary, his early lessons have been well learnt. Cognitive scientists no longer doubt the importance of the themes that Chomsky highlighted--even if a minority (for example, proponents of situated robotics and/or dynamical systems) are highly sceptical about some of them. Indeed, these themes are taken for granted, and not thought of as specifically Chomskyan. They include the definition of generative grammars (for many kinds of behavior); comparisons between the computational power of different systems; the existence and nature of mental representations; hierarchical structure in minds and behavior; the CPD; formal description/explanation in psychology and the philosophy of mind; and the plausibility of some form of nativism/epigenesis in language (and in other areas, too).

In short, cognitive science owes a huge intellectual debt to Chomsky, even though he is less of a name to conjure with now than in the 1960s/1970s. That's what my Chapter 9 was saying--so the urologist's description, albeit highly amusing, wasn't well-aimed. (Yes, the pun is intended!)

Finally, a word about the highly unpleasant tone of Chomsky's piece--which led the e-mailing MD to come up with his memorable description in the first place.

Among the countless personal sneers scattered within it is one that I find very funny. I'm minded to get out my embroidery silks, to sew it as a richly-colored sampler for my study wall. He says (p. 1096): "But none of this matters. The phenomenologist has spoken." His scorn here is directed at me, although his word ("phenomenologist") is possibly aimed also at a Finnish linguist whom I'd quoted. The barb is laughable: as my remarks (above) about Continental philosophy show, the last approach that I'd follow uncritically is phenomenology. Well, perhaps not the *very* last: "post-modernist" would have been even funnier.

The other insults in Chomsky's piece are less amusing. They don't merit individual attention, and nor do they merit publication in a serious journal. But such discourtesies have been part of his intellectual armoury for years. The contemptuous tone, the systematic misrepresentations, and the many falsehoods that characterize his review are all typical.

Others, too, have received the catch-all accusation (p. 1094) that "virtually every reference to me ... is fanciful". Compare, for instance, "I see no reason to try to trace the various confusions that [the philosopher Gilbert] Harman develops, none of which have any relation to the views I actually hold" [24, p. 154]. Or consider this, aimed at the (then) generative semanticist Lakoff: "[he has] thoroughly misunderstood the references he cites...presents a hopelessly garbled version...[and] has discussed views that do not exist on issues that have not been raised, confused beyond recognition the issues that have been raised and severely distorted the contents of

virtually every source he cites" [25].

No dissident can escape his arrows. Even being a Finn is enough to prompt a jeer from the master. (And possibly a libel: saying that the Chomsky quotation I cited from my Finnish source [63] is "presumably invented", he cannily leaves it ambiguous as to which of us is being accused of such rampant dishonesty.)

This intellectually shoddy treatment of anyone who dares to disagree with him is not merely non-Legendary, but positively harmful. It has contaminated the discipline--hence the doubt about myth-item 9 (that he reawakened and strengthened linguistics as a whole). His combative rhetorical stance has helped to lower the standing of other sub-areas, such as sociolinguistics or comparative philology, since he often implies that anything he isn't interested in is trivial (see below). And within the study of syntax, it has raised the emotional temperature considerably. His followers are tempted to imitate. (Fortunately they don't always do so: my book received an entirely courteous review from one of them [102]). And his opponents are often so enraged as to reply in kind.

That's largely why non-Chomskyans are passionate in their opposition (and why I, as an outsider to the field, didn't feel a need to endorse the vitriol in their remarks). And it's why I didn't have to do much digging to "unearth" (Chomsky's word) the highly-charged critiques quoted at the opening of his review. Anyone who doubts that has only to look at the aptly-named survey of *The Linguistic Wars* [57], or at Postal's blistering critique of Chomsky's "often disturbingly unserious, indeed irresponsible" style of argument, with its many "outrageous" and "grotesquely untrue" remarks [99].

Other fields, of course, have their feuds and schisms. Several of these are described in my book--including five intellectual scandals relating to AI: MT versus ALPAC, the Dreyfus and Weizenbaum affairs, the attack on perceptrons, and the Lighthill Report. My AI chapters therefore mentioned some "juicy gossip" about conflicts, as Feldman puts it. So did the sections dealing with disputes about the Science Wars (1.iii.b), the analytic/Continental divide in philosophy (1.ii.c-d, 11.ii, 16.vi-viii), and the proper nature of psychology--empirical or hermeneutic (6.i.d, 6.ii.d). The fight between cognitive and hermeneutic anthropologists was especially nasty (8.i.d, 8.ii.a-c). Disappointing though this must be for Thumper's mother, cognitive science is not all sweetness and light.

But no other discipline within it is so notoriously ill-tempered that I felt bound to open my chapter with a health warning ("beware of the passions that swirl under any discussion of Chomsky": p. 591) and a minatory section headed "A non-pacific ocean" (7.i.b). In short, the disciplinary situation in linguistics is dire. It goes way beyond the robust argument--and yes, the catty infighting--endemic in other areas of intellectual life. And, although I didn't explicitly say so in my book (because I wasn't engaged in a pissing contest), it is Chomsky's habitual manner of treating his critics which is the key to this sorry state of affairs.

Roberts [102] agrees about the direness, but blames the anti-Chomskyans. He says that the "meme" of Chomsky as an intellectual dictator "performs a useful rhetorical function for Chomsky's critics: it poisons the well for his defenders. It means, for example, that all of the points I [Roberts] am trying to make in this article can be safely ignored, since I can be viewed as either a terrorised apparatchik or as a mindless acolyte". That may well be true--and if so, it's highly regrettable. But *why* did this lamentable state-of-affairs come about? Roberts suggests (p.c.) that it is a generational thing, a hangover from the "linguistic wars" of the 1970s/1980s, and that the youngsters will be less obstreporous. I hope he's right. But even if he is, we're still suffering the direness now. And it's clear that Chomsky's own lack of collegiality and good manners is largely responsible.

Even as a young man, he could use both invective and ridicule as weapons. His review of Skinner [20] achieved its notoriety partly for that reason. Words such as "pointless", "confused", "gross", "absurd", "delusion", "arbitrary", "useless", and "empty" leapt out from the pages, and Skinnerian art-appreciation was said to be best conveyed by shrieking "Beautiful!" repeatedly, without ever pausing for breath. (I don't say this as a criticism: in the context of his careful and intellectually powerful critique of Skinner's theory of language, these rhetorical flourishes were acceptable--indeed, enjoyable.)

But at that time, fifty years ago, he could practice courtesy too. In 1958 he was given a rough passage by the about-to-be-ousted structuralist linguists, at a conference held soon after the publication of *SS* [60]. Chomsky's own presentation was inoffensive enough [21], but the post-talk discussions that appeared in the *Proceedings* were robust. And some remarks suggest that the atmosphere wasn't always congenial: speakers complained that "At this point communication seems to have failed" (174), or referred to "the danger that communication would break down completely" (p. 182) and the need that "if we are going to live together, we must do our best to keep our wars strictly intellectual" (p. 177). The organizer, Archibald Hill, closed the meeting diplomatically by saying: "I am afraid we are not quite fair to each other, since I do not believe that any of us makes a serious attempt to work it out as the other fellow has done it. We should, and we should have respect for each other, and study what each of us has done" (p. 186). In light of that sage remark, I suspect that many heated discourtesies had been exchanged in the smoke-filled rooms.

For all that, the published discussions were scrupulously polite--Chomsky's included. At one point (p. 161), he even said: "[If] I have given the impression that anything that I am not personally interested in, is trivial, I apologize."

Anybody finding those two little words in one of Chomsky's more recent publications might need smelling-salts to help them recover from the shock. Despite his unparallelled professional success since those early days, he has long forsaken courtesy for insults--and even calumnies. Dissenters regularly receive abuse, rather than critique.

(That's assuming that he deigns to reply to them at all. His insistence that he had to be asked "several times" to write his review for the *AIJ* is consistent with a recent report--with ample evidence provided--that Chomskyans use "sulking as an intellectual strategy" [109]. They typically refuse to give their opponents "the oxygen of publicity" by citing them, or by agreeing to speak from the same platform. One prominent Chomskyan, namely Pinker [93, p. 171f.], even failed to cite an opponent [108, p. 128] whose argument he was specifically rebutting, and whose--highly unusual--examples he was borrowing.)

A remarkable case of unrestrained abuse on Chomsky's part was noted by William Bright, an expert on indigenous American languages and the key founder of sociolinguistics. As editor of *Language* for the previous twenty years, Bright had written to Chomsky in 1984 urging him to submit a paper. It was a pity, he felt, that the discipline's leading journal hadn't published anything by its leading theorist for many years. Indeed, MIT-trained linguists in general had virtually stopped sending items to *Language,* favouring two newer journals instead (one founded only in 1983).

Chomsky's reply was not your usual polite brush-off: *Sorry: too busy, too many deadlines....* To Bright's amazement, he wrote (alongside other abuse) that he would never consider publishing in a journal that had published "flat lies ... couched in a rhetoric of a sort that might be appropriate to some criminal, but that one is surprised to find in a scholarly journal" [61, p. 636].

Truly, the mind boggles. Compared with invective like that, his derisive "phenomenologist" is mild indeed. (Perhaps that's my loss: "criminal" would have inspired an even more delectably

ridiculous sampler for my wall.) This extraordinary incident, if nothing else, suggests that a linguist colleague may have been right when he said to me recently, "To be savaged by Chomsky is a badge of honor".

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