Chomsky's Methodological Fakery

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Section 1: History

Like my article 'Two Studies of Chomsky's Play Acting at Linguistics", previously posted at LINGBUZZ, the present article was originally written in 2004-2005 and has been only modestly updated for posting on LINGBUZZ. This updating does *not* involve any extension of the references. Like the previous article, when originally written, this one was also intended to be merely one chapter in a full length book devoted to the (lack of) quality and standards in Noam Chomsky's (NC's) linguistic work. That work, listed as Levine and Postal (in preparation) in the references of Levine and Postal (2004), was abandoned. The updating mentioned here includes a few deletions and rewordings intended to permit the work to stand alone, specifically by eliminating crossreferences to other intended chapters of the ultimately merely hypothetical book.

Section 2 Goals of Linguistics

If one thinks of the goals of the field of linguistics in business terms, one can ask what product(s) or services does linguistics aim to yield. In general, the answer is simple. Linguistics seeks to determine and disseminate the basic truth(s) about natural languages in the widest sense.

Given that, methodological discussion of linguistics should, in fact must, be based on directing linguistic research along paths which maximize its efficacy at yielding truths and minimizes its danger of yielding and spreading falsehood. In this respect, linguistics is not different from other disciplines, biology, chemistry, history, mathematics, etc. To serve the ends of truth maximization and falsehood minimization, many fields have explicit codes of conduct

governing the proper behavior of those engaging in, or purporting to engage in, research in those fields. Notably, linguistics in America has never had such a code.

That situation, which is arguably highly undesirable, does not mean, however, that linguistic work can be, or is, in general pursued in a kind of ethical anarchy. Basic ethical assumptions which linguists can doubt no more than other academics determine e.g. that it is unacceptable in an academic discipline to knowingly advance falsehoods, to use other people's work without credit, to pretend that some claim which is baseless is a known fact, to invoke some methodological principle P to reject a critic's conclusions and then (even in the same work) to violate P oneself, etc. Thus the absence of a *formal* code of conduct for linguists cannot in any way preclude ethical evaluation of the professional conduct of linguists.

Section 3 Methodological Discussion

NC's linguistic writings contain a significant body of statements concerned not with the nature of natural language as such but with his views about the nature of proper research into the subject matter. He has thus advanced a good many claims about methodology. What follows considers critical ethical aspects of some of his particular methodological claims and statements.

Section 4 Methodological Constraints Which Only Others Need Follow

Chomsky (1977) represented in the history of generative grammar a frequently cited descriptive and theoretical article; see e.g. (Postal, 1998: 1) and Hukari and Levine (1991: 97-98) among others. In it, NC inter alia strongly rejected as an instantiation of a *fallacy* certain criticisms (irrelevantly, by the author of the present work), and in so doing enunciated a general methodological principle:

(1)a. Chomsky (1977: 74)

"A proposed condition on rules, such as subjacency, cannot be confirmed or refuted directly by phenomena of this (or any other) sort. A condition on rules can be confirmed or refuted only by rules, which observe or violate it, respectively. If the rule of NP-movement that yields (9) applies successive cyclically, as often assumed, then the rule will observe subjacency. If, as I have argued in the references cited, the rule of *wh*-movement applies successive cyclically, then it too will observe subjacency, giving (10). To find evidence to support or to refute a proposed condition on rules, it does not suffice to list unexplained phenomena; rather, it is necessary to present rules, i.e. to present a fragment of a grammar. The confirmation or refutation will be as convincing as the fragment of grammar presented. This is a simple point of logic, occasionally overlooked in the literature."

b. Chomsky (1977: 127 note 10):

"Postal's discussion of alleged counterexamples to the SSC is a good example of the fallacy I have noted several times: *phenomena do not bear directly on conditions on rules; only rules do*. In no case does he propose a rule that violates (or confirms) these or any other conditions." (Emphasis added: PMP)

I am unconcerned here with the validity or accuracy of the criticism, only with its dependence on NC's unequivocal, twice-given invocation of the general procedural claim:

(2) Citing sentences with certain properties as counterexamples to conditions on rules is a

fallacy; one must cite rules.

Regardless of the methodological validity of claim (2), *minimal* honesty obviously requires that anyone proposing it and then appealing to it to defend some position of his own is especially under the obligation to follow (2) themselves.

Significantly though, as already noted in Brame (1984), NC refused to conform to his own methodological constraint even in the very article where he stated it.

(3) Chomsky (1977: 113)

"A different approach to these questions is suggested by Bach and Horn (forthcoming). They propose a general constraint that they formulate as follows:

 (185) The NP Constraint. No constituent which is dominated by NP can be moved or deleted from that NP by a transformational rule [apart from free deletions, if such exist].

The NP Constraint differs in its empirical consequences from the modified subjacency condition in that it excludes all movement from NP, whereas the subjacency condition excludes only those movement rules that extract an element from S as well as NP; just *wh-movement*, if the foregoing analysis is correct. The NP Constraint is *immediately falsified by such examples as (182).*" (emphasis added: PMP)

Evidently, Bach and Horn's proposal represented as NC's (185) was a condition on rules. While it mentioned constituent types, it also referred to transformational rule operations and any precise version of it would have had to quantify over rules. Thus to criticize their proposal merely by citing examples would unquestionably fall under NC's proclaimed fallacy type. But despite that, NC's (182), which he claimed falsified the Bach and Horn claim, was:

(4)a. [$_{Sbar}$ COMP [$_S$ [NP a review t_1] was published [of Bill's book]$_1$]]

   b. [$_{Sbar}$ COMP [$_S$ [of the students in the class]$_1$ [$_{NP}$ several t_1] failed the exam]]

Notations (4a, b) represent (structured) examples not rules although, perhaps not insignificantly, as pointed out to me by Geoffrey K. Pullum, NC *refers to them* just above (4) as rules. However, on the following page they are correctly referred to as examples. NC's varying usages here are

linguistically and methodologically irrelevant. One cannot alter the logical connections between a black swan and a claim that all swans are white by pasting the label 'crow' on the black swan.

Moreover, suppose for argument, but contrary to standard practice and terminology, that an example counted as a rule simply by virtue of receiving some labeled bracketing, as in (4). It is a given that whenever an analyzed example is offered as a contraindication to a rule (system) R, the claim is that its real structure is inconsistent with R. But under the supposition, examples offered *without* labeled bracketings could then not constitute a category error of the sort NC complained about, that is, a violation of (2). Rather, under NC's own seeming assumptions about what e.g. (4a, b) consist of, they would at worst represent insufficiently explicit rule (systems). Consequently, there would be no fallacy and none of the factually based objections NC complained about as fallacious could be dismissed on that ground. So NC's bizarre talk of (4a, b) as rules might reasonably be construed as an attempt to have it both ways.

One marvels initially at the sheer flagrancy of the hypocrisy involved in the existence of (1) and (3) in the same article. Particularly noteworthy is the presence in the last line of (3) of the word *immediately,* used in serious discussions of arguments to register the lack of any need for intermediate steps, which would here be the presentation of a rule inconsistent with Horn and Bach's constraint. NC's defensive principle (2) required the existence of such a rule but he presented none. Although having claimed that a condition on rules could only be confirmed or refuted by a fragment of grammar, he took Horn and Bach's condition to be refuted merely by some data; there was no relevant grammar fragment in sight.

While such a performance surely merits the designation 'play acting at linguistics', a notion whose applicability to NC's work is argued in my article 'Chomsky's Play Acting at Linguistics', it is considerably worse. For, in 1977 either (i) NC believed (2) or (ii) he did not.

If one assumes (i), as the default academic assumption about anyone's work requires and as is of course supported by the sheer existence of (1), then one could accept that NC's rejection of Postal's criticisms of his own position was honest. But his attack on Horn and Bach's proposal then could not have been. If (ii), NC's rejection of Horn and Bach's proposal could have been legitimate, but his dismissal of Postal's criticisms could not have been; and of course, in that case, his dual statements (1a, b) had to be dishonest. No matter what one assumes then, NC was, with respect to at least one conclusion he wished to establish, *dishonestly utilizing a principle whose validity he did not accept*.

The foregoing reveals that in this article NC in effect *demonstrated* his own contempt for scholarly standards by *attacking and rejecting* a proposal of other linguists via the employment of an argument type he himself declared---when *defending* proposals of his own---to be fallacious. These remarks presume, of course, that NC was conscious of the conflict between his general principle (2) and his clear violation of (2) in the Bach-Horn discussion. But how could one seriously assume otherwise? That is, how could it be assumed that a professional linguist who had then been active in his field for almost a quarter of a century and who had been widely praised as the world's most illustrious linguist, could be unaware of the obvious conflict between (1) and (3).

It would be a mistake to imagine that the unconscionable methodological application I have documented about Chomsky (1977) was a one-time matter, an anomaly. In fact, the formulation of *principles like (1)* is a recurrent feature of NC's linguistics, where by the highlighted phrase I mean ad hoc principles invented de novo to insulate his own ideas from disconfirmation. And, as shown for (1), such principles are consistently not applied by NC evenhandedly.

One notes for instance

(5)a. Chomsky (1981: 281)

> "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."

b. Chomsky (1982 :76)

> "Suppose that counterevidence is discovered --as we should expect and as we should in fact hope, since precisely this eventuality will offer the possibility of a deeper understanding of the real principles involved."

Such remarks, already made fun of in Postal (1988, 2004, Chapters 1, 9), could rightly be taken to suffice in themselves to show that the author had more than three decades ago already gone way beyond play acting at linguistics into utter fakery.

What professional linguist would, for example, accept the content of (5) as part of a student's reaction to rejection of claims in a paper, thesis or examination, e.g. as a defense of falsehoods like 'every language has labial consonants' (Mohawk doesn't) or 'every language allows nonpronominal prenominal possessors' (French doesn't) or 'no language has passives of nonagentive clauses' (English does)! But if such sophistry is not acceptable for students, how in a world free of double standards would it be acceptable for NC?

Moreover, imagine if in 1957 at the beginning of his first major and arguably most influential work, Chomsky (1957), NC had said:

(6) It is intended that this study facilitates theoretical advances and significant proposals which, I hope will lead to serious empirical and conceptual problems and counterevidence to those proposals.

How many linguists could have taken seriously whatever followed such an opening? One is, with respect to such claims, never told why, for example, one should not, on the contrary, hope for the immediate resolution of any factual and conceptual problems arising in the attempt to formulate a grammar of some NL .

And never has NC referenced any serious philosophy of science work or cited anything from a real field of science in which such a view has been maintained. I therefore see in such remarks nothing beyond NC's fearful recognition that whatever descriptive or grammatical analysis he might be able to provide could not stand the light of ordinary scrutiny. And the latter judgment has often been validated; see e.g. Matthews (1980), Borsley and Pullum (1980), Borsley and Horn (1981), Bresnan (1982), Postal and Pullum (1978, 1982, 1986, 1988), Pullum and Postal (1979), McCawley (1982, Chapter 1), Pullum (1983) Levine (1984a, 1984b), Ruwet (1991, especially Chapter 5), Sag and Fodor (1995), Pollard and Sag (1992, 1994), Postal (1998, 2004: Chapters 7 and 8), Levine and Sag (2003) among many others.

An assessment of remarks like (5) not predicated in advance on special privileged assumptions about NC should, I believe, see them as an attempt to claim validity for whatever is being proposed, whether it turns out to be right or wrong. If right, then of course it is valid, and if wrong, then valid as a stepping-stone to the ultimately correct solution. Such a perspective is, I believe, unknown in real science, where results shown to be predictively incorrect in crucial tests are typically dismissed from further consideration and regarded as casualties of scientific progress, not as valuable pointers to superior models.

That statements like (5a, b) are fakery is supported by NC's reaction in (3). There he of course says nothing about how wonderful it is that Bach and Horn's proposal satisfies the supposed hoped for problem manifestation. Far from being taken to facilitate progress, factual difficulties

in *other people's* proposals simply lead (if, as in this case, NC doesn't like them,) to immediate rejection.

What emerges beyond reasonable doubt is that many of NC's methodological injunctions like (1) are not principles designed to properly direct inquiry in profitable directions but are just devices to protect NC's personal ideas from falsification. This defensiveness is seen in NC's formulation of even grander protective shields like that in:

(7) Chomsky (1979: 187-188)

"Serious questions arise concerning the attitude one should take toward apparent counterexamples. At what point must they be taken seriously? In the natural sciences apparent counterevidence is often ignored, on the assumption that it will somehow be taken care of later. That is quite a sane attitude. Within reasonable limits, of course, not to excess." "At a given moment one has to cut short the questions that arise. One must try to assess the relative importance of the phenomena or rules that contradict one's hypotheses, as compared with the evidence supporting them. Then, one will either put aside counterevidence to be dealt with later, or else decide that the theory is inadequate and must be reconstructed. The choice isn't easy. There is no algorithm. And as this kind of problem arises constantly in the course of research, it is an intuitive judgment whether or not one should persevere within a given framework-because of the positive results and in spite of the apparent counterexamples. In general, there has been considerable progress in linguistics, if one considers the positive results-even if innumerable problems remain at each stage. 'Methodologists' sometimes assert that a counterexample serves to refute a theory and shows that it must be abandoned. Such an injunction finds little support in the practice of the advanced sciences, as is well known, virtually a truism, in the history of

science. The willingness to put aside the counterexamples to a theory with some degree of explanatory force, a theory that provides a degree of insight, and to take them up again at a higher level of understanding, is quite simply the path of rationality. In fact, it constitutes the precondition for significant progress in any nontrivial field of research."

I do not think this account, supported, one observes, by no references to actual science or to serious work in the philosophy of science, provides a proper depiction of the way real science deals with apparent counterevidence to principles and claims.

For instance, NC's remarks appeal to unnamed and unreferenced methodologists who assert that a counterexample (i) serves to refute a theory and (ii) to indicate that it must be abandoned. It is doubtful any serious student of science accepts tout court the conjunction of these propositions. Certainly, anyone must admit that if a theory T consisting of a set of distinct propositions entails a false proposition P, P being the proper description of the putative counterexample, then T is false…as such, it is refuted if the counterexemplification is bona fide. But it is absurd to suggest that this simple logical conclusion, representing nothing but the truism that a true theory cannot entail a falsehood, requires T to be *abandoned*. It requires only that at least one of the propositions composing T either be dropped or modified in such a way that the modified set of propositions formed from T does not entail P. In short, NC's (7) sets up a straw man as an alternative to a serious approach to dealing with conflicts between theoretical proposals and facts, a straw man whose function is to illegitimately rationalize ignoring unpleasant data.

It is relevant to compare obscurantist remarks like (7) with NC's earlier statement of the importance of rigor, strict application of principles to linguistic material and the need to exclude the protection of unacceptable conclusions by ad hoc adjustments.

(8) Chomsky (1957: 5)

> "I think that some of those linguists who have questioned the value of precise and technical development of linguistic theory may have failed to recognize the productive potential in the method of rigorously stating a proposed theory and applying it strictly to linguistic material with no attempt to avoid unacceptable conclusions by *ad hoc* adjustments or loose formulation."

In short, fakery like (7) violates NC's own earlier strictures.

Further documentation that baseless attempts to defend his own personal views from falsification are a persistent feature of NC's writings is easily found:

(9) Chomsky (1980a: 2)

> "If some remarkable flash of insight were suddenly to yield the absolutely true theory of universal grammar (or English), there is no doubt that it would at once be 'refuted' by innumerable observations from a wide range of languages."

As with (7), no support at all was supplied for (9). And, as noted in Postal (1983: 365 n6), the quotes on *refuted* do all the rhetorical work in this typical defensive statement whose dishonest subtext is clearly: 'If what I say is impugned by counterexamples, that is not a real problem.' That follows given that a true theory *cannot be refuted*, since by definition a refuted theory is not true.

The same make-believe found in (9) shows up in:

(10) Chomsky (1980b:10)

> "As for the matter of unexplained apparent counterevidence, if someone were to descend from heaven with the absolute truth about language or some other cognitive faculty, this theory would doubtless be confronted at once with all sorts of problems and

'counterexamples', if only because we do not yet understand the natural bounds of these particular faculties and because partially understood data are so easily misconstrued."

And again in:

(11) Chomsky (1982: 5)

"Even if somebody came up with the absolute truth, it would be easy to 'demonstrate' that it is wrong because there are all kinds of data around that do not seem to fit, there is too much that we just don't understand."

Passage (10) was rightly taken in Brame (1984: 346) as a notable instance of NC's illegitimately attempting to preclude falsification of his ideas. The basic claim was entirely unsupported. The reader is given no reason whatever to accept that the *absolute truth* would be faced with counterexamples. The quotes on *counterexamples* hedge in such a way as to make no genuine testable claim. But the subtext is clear: if even the *absolute* truth would face 'counterexamples', it is no surprise that my (= NC's) ideas appear to face problems, which doesn't mean they are wrong. But this methodological grace does not of course hold for disfavored individuals, like Bach and Horn.

Suppose, contrary to what I have said, if only for argument, that NC's principles like (7) do give a proper account of the proper method for treating counterexamples. Then since NC purports to accept it, one should find him applying it across the board to the work of others, and not only to his own pet ideas. But (3) shows nothing of the sort. Where is any argument that the putative counterevidence he cited to reject ('immediately') Bach and Horn's proposal was not of the sort that should be 'put aside'? Where is there any hint that NC even considered such a possibility? Where is there any reason to think then that (5a, b), (7), (9), (10) and (11) were not just a continuing sham, ideas to be invoked as protection only for NC's own ideas and those of

his favorites, but certainly not for those of e.g. his critics or even others showing some independence of thought?

Section 5 Getting Away with It

NC's repeated invocation of fake methodological principles and his refusal to be bound even by the principles he advances is surely extraordinary. But what is more extraordinary is that his repetitive involvement in the relevant kind of fundamental dishonesty has, as far as I can determine, had absolutely no negative consequences for him in his career in linguistics. One might speculate that this situation internal to a self-characterized science could only have been due to the fact that prior to the present remarks, no one had grasped the degree of deontological violations in what he was doing.

But that surmise is entirely wrong. First, the subterfuge involved in (1) and (3) above was originally noted more than three decades ago::

(12) Pullum and Borsley (1980: 101, note 21)

"The assertion, incidentally, represents another piece of Chomsky's advice that Chomsky himself seems to ignore. Thus while we find him asserting and reiterating that 'a proposed condition on rules ... cannot be confirmed or refuted directly by phenomena' on p. 74 of Chomsky (1977a), on p. 133 of the same paper he quotes the NP Constraint, a condition on rules due to George Horn, and says that it 'is immediately falsified by such examples as (182)', where (182) contains two schematic labelled bracketings of English sentences, On the other hand, when he turns (pp. 126-27, footnote 10) to discuss Postal (1976:172-179), where twenty-two similarly schematic proposed counterexamples to the Specified Subject Condition are presented, he returns to the precept, and accuses Postal of committing 'the

fallacy that I have noted several times'."

One notes the criticism was brief, mild, marginal to the overall topic of the article and restricted to a footnote. So it is perhaps unsurprising that NC never reacted to the remarks in (12), and more generally, perhaps not noteworthy that there was a general failure to pick up on the significance of such a comment.

But the conclusion that NC was able to violate basic principles of scholarly conduct with impunity, in particular, with flagrant utilization of double standards, is more solidly supported. For my remarks above about Chomsky (1977) in effect largely elaborate part of one *already made in Brame (1984)*. Moreover, Brame noted properly that the particular violation recapitulated here was only a special case of a trend in NC's then previous activities. That is, Brame's remarks were much more central to his overall thesis than Pullum and Borsley's and not at all dismissable as a mere footnote. His discussion was, moreover quite lengthy. It nonetheless merits quoting in full:

(13) Brame (1984: 319-321)

"Now compare the two extracts and answer the following question: How can Chomsky's 'phenomena' bear crucially on Bach and Horn's condition which is a condition on rules, while Postal's 'phenomena' cannot bear on Chomsky's condition?

Turn the clock back a couple of decades. In 1964 we find that Chomsky was extolling the virtues of data that favored his idea including those examples provided below.

(3)a. John is easy to please.

  b. John is eager to please.

(4)a. John's eagerness to please,

b. *John's easiness to please.

In connection with such examples. Chomsky wrote: "data of this sort are simply what constitutes the subject matter for linguistic theory, - and he continued, "We neglect such data at the cost of destroying the subject" [15:79]. Here we are told that data are crucial when Chomsky is the purveyor of the criticism; however, data are beside the point when the roles are reversed. For example, Winter [421 in 1965 adduced the following "subject matter" as evidence against the transformational derivation of prenominal adjectives.

(5)a. a second Chomsky ≠   b. a Chomsky who is second

To these data Chomsky responded: 'As has been pointed out innumerable times, exceptions such as those which Winter notes are of interest only if they lead to subsidiary or still deeper generalizations'[16:119, fnl7]. Now, the naive reader who is unschooled in the subtleties of the Galilean philosophy of science may well wonder: Why are Chomsky's data in (3)-(4) 'the subject matter for linguistic theory' whose 'neglect' would 'destroy . . . the subject', whereas Winter's data can simply be neglected since they are alleged not to lead to deeper generalizations?

Consider another of Chomsky's earlier idealizations-extrinsic rule ordering. Chomsky imputed to Lamb 'apriorism' and 'dogmatism' with the remark that 'there can be no question that [such ordering principles, MB] are factual hypotheses, and that empirical evidence can be used to confirm or disconfirm them' and that 'it is mere dogmatism to refuse to judge factual issues in terms of empirical evidence and simply to assert what the answer must be, independently of what any facts may suggest' [16:107].

By this line of reasoning can we not conclude that Chomsky's style is 'mere dogmatism' and 'apriorism' when he excludes the data in (2) 'in principle', and that his approach is

'to refuse to judge factual issues in terms of empirical evidence', when he a priori denies Postal's and Winter's facts, and that he simply asserts 'what the answer must be, independently of what any facts may suggest', such as the facts in (2), Postal's facts, and Winter's facts? It would seem, then, that by his own reasoning Chomsky dogmatically and aprioristically embraces the asymmetry hypothesis and with it deep structure, and by inference, transformations.

We begin to see how the so-called Galilean style has been employed to license the court linguists with special privileges. Counterevidence to the court theory can be eschewed as irrelevant by drawing on the Galilean style; counterevidence that favors the court theory and argues against others, however, is 'the subject matter for linguistic theory'.

What about the record? What about Lamb's claim that rules are not extrinsically ordered-that dogmatic position of his? What about Winter's claim that transformations must not be utilized to derive prenominal adjectives, that uninteresting claim of his? What about Postal's evidence that Chomsky's conditions on transformations, and in particular his SSC, is empirically disconfirmed by empirical considerations? What does hindsight tell us? In answering these questions, we note that years later Chomsky has laid claim to a number of breakthroughs; in [14:489] he and Lasnik claim that rule ordering in syntax is not needed; in [19] he has discovered that many transformations previously postulated do not in fact exist; more recently he has noticed that SSC is misguided. These 'discoveries', however, had to come through the Galilean style, with no recognition of priority."

That is, Brame had in 1984, like Pullum and Borsley before him, not only laid bare the unacceptable conduct in Chomsky (1977) I have noted but more generally had shown it to already represent part of a persistent strand of hypocrisy in NC's work of long duration.

Moreover, each of Brame's charges was unequivocally supported by an evidence base consisting entirely of NC's own statements. He had merely assembled some of NC's own claims, permitting readers to recognize their undeniable moral inconsistency. This essentially represents my own method in this article as well. Arguably, which one of various incompatible methodological principles NC invoked depended only on which one served his rhetorical purposes at the time it was appealed to.

One would naturally assume that someone accused publicly in print of this sort of unprofessional conduct, and moreover accused *by one of his own students*, would have replied and tried to defend himself. But it never happened; NC successfully treated Brame's criticisms (like that of Pullum and Borsley) as if they did not exist.

But the fact NC could both (a) behave in the disreputable ways documented and (b) suffer no general opprobrium internal to linguistics has broader implications. It tells us, does it not, that much of modern linguistics, at least in the United States, has developed in such a way that standards of academic conduct are simply of no relevance as far as NC's behavior is concerned. The linguistic community has for decades implicitly made it clear that he could trash basic standards in flagrant ways with only the most intermittent protests on fringes of the field. And it is impossible to believe that NC did not long ago grasp that his linguistic statements were, as far as the existent social structure in linguistics goes, subject to no ethical constraints.[2]

Given that linguistics represents only a tiny fragment of the intellectual and academic world, sooner or later the true nature of NC's unethical behavior will in all likelihood become common knowledge. And then the dominant silence of so many members of the linguistic community will have to be answered for in one way or another. How is it, many will ask, that

such a huge portion of a whole field was willing to avert its eyes for decades from the demonstrably corrupt behavior of its most prominent practitioner?

Notes

1 Sadly but significantly, such ludicrous remarks have been aped by others under NC's influence:

> (i) Koster (1978: 566)
>
> "Interesting theories do not avoid conflicts with the data, but rather create
>
>   clashes on purpose"
>
> (ii) Riemdijk and Williams (1986: 320)
>
> "[B]ut this is exactly what one wants of a strong proposal: that it lead immediately to
>
>   a great deal of empirical difficulty."

2 My own experience in this regard sheds some light on the situation. Over time, a number of linguists have reacted to various documentations I have offered either publicly or privately of NC's unethical professional conduct, by taking such criticisms to show something unseemly about MY conduct. The idea was clearly transmitted that the very idea of raising such issues in connection with NC was more or less reprehensible. It has been suggested (ignorantly and falsely) that I hold some personal grudge against NC, that I am obsessed with criticizing him, that I am consumed with anger, that I would better spend my time on other activities, etc. Suppose for argument some or all of these claims were true. So what! Suppose I am the most

rotten individual ever to have been involved in linguistics. Even loathsome creatures like me might still somehow nonetheless assert something which is true. But the sort of reactions I received and others in the same vein address everything but the truth of the assertions about NC's conduct. They amounted essentially to urgings that I go away and stop criticizing NC's ethical failures. In my view, the relevant responses thus represent part of the linguistic world's dominant refusal to confront NC's dishonesty.

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