TOPIC . . . COMMENT

The Revenge of the Methodological Moaners

In 1978, I published in *Language* a review of a rather dull book about linguistic argumentation, filled with uninspiring papers that fruitlessly raked over the ashes of arguments long gone cold. I thought it was regrettable to see linguists spending their time mulling over the logic of tired old arguments when there was so much linguistics to do, and I said so quite bluntly, using phrases like "self-indulgent methodological agonizing".

This brought down on me a certain amount of abuse. *Language* took the unusual step of publishing a letter to the editor about the review – a letter in which ugly phrases such as "ostrich-like" and "avoid facing up to foundational problems" are to be found (Kac 1980). And there were much angrier responses elsewhere. The angriest I know about was from Bruce Derwing of the University of Alberta, who in 1979 published an article in which my name appeared alarmingly close to a rash of phrases like "failure to recognize the nature of the problem", "pure sloth and accompanying ignorance", "arrogance", "narrow and inflexible mind", "thoroughly anti-scientific", and "disreputable and isolated". All I had said about Derwing in the review was that he represented an excellent example of the way the linguists who make the most noise about the coming methodological disaster seem also to be those who do the least linguistics. Derwing quotes this with the gratuitous insertion "[formal]" in front of my word "linguistics", but I wasn't singling out work that offers a mathematical definition of its claims. I meant any sort of linguistics at all. I was thinking of Derwing's book *Transformational Grammar as a Theory of Language Acquisition*, which contributes nothing to transformational grammar, and every bit as much to the study of language acquisition. It has essentially no linguistics in it. That seems a waste to me. Even if linguistics *is* in troubled waters, what we need is all hands to man the pumps. What we don't need is the likes of Derwing striding around the heaving deck shouting that we're all doomed.

Even people I did not mention at all have come forward obligingly to argue that they aren't guilty when I never said they were. This happens sometimes. In 1848 a factory in Portlaw, Ireland brought a libel action against a newspaper for accusing "a certain factory in the south of Ireland" of cruel labor practices. The owners argued, most revealingly, that readers of the paper would be likely to assume it referred to them. (Incidentally, they won their case, and a lot of money, the judgement being upheld on appeal to the House of Lords; see Carter-Ruck (1972, p. 69).) One person who did something similar with my tirade against the methodological moaners is Geoffrey Sampson, who quotes the phrase about methodological agonizing in his book *Making Sense* (1980, p. 205) and attempts to defend himself against the potential charge that he is engaged in this activity. (Too disgusted to mention my name in the text, he refers to me by a contemptuous epithet, "One enthusiastic British proponent of Postal's theories" (!), revealing my identity only in a footnote on p. 206.) Well, I am not aware of having had Sampson in mind at the time, but if Sampson thinks the cap fits, he is clearly welcome to insert his head. Certainly, he does seem to be another example of an erstwhile linguist who has deliberately decided not to do any more linguistics, but merely to stand around and kibitz.

In 1978, I had no idea of the direction that the next half-decade's developments would take. I merely hoped to discourage linguists from engaging in philosophy of science and encourage them to do something they are good at. I failed, as ever. Since I wrote the 1978 review, we have witnessed a new flowering of methodological moaning and self-serving cracker-barrel philosophy of science in the work of people who actually *do* produce publishable work in descriptive and theoretical linguistics. Many linguists now refuse to let their linguistic work stand on its merits. They garnish it with epistemological homilies, and serve it with a side salad of little sermons on the essence of scientific inquiry. While handing you their linguistic hypotheses they take the opportunity to stuff a few tracts on the philosophically correct view of falsifiability into your pocket, in case you should be so misguided as to suggest a counterargument, or to fail to see that what they are doing parallels precisely what Einstein and Newton did.

Jan Koster's 1973 article 'Conditions, Empty Nodes, and Markedness' is a fine example, and introduces to *Linguistic Inquiry* readers a useful term of abuse: "naive falsificationism" (p. 566). There is a substantial amount of serious and interesting linguistic argumentation in the article. But along with the grammar we get homilies on how to live as a Good Scientist, dressed up with references to Dijksterhuis, Feyerabend, Feynman, Holton, and Moscovici on the philosophy of science. And the drift of most of these didactic excursi into general studies of scientific method is purely defensive. Here are a few interpreted examples:

Text	*Interpretation*
One can hardly imagine the development of an explanatory science without the discovery of entities that are unknown in prescientific experience...	Don't be alarmed if I introduce some pretty weird little invisible doohickeys to get my explanatory payoff; it's O.K., real scientists do it.
It is necessary for the growth of a theory to work out several alternatives...	I may seem to be disagreeing with Chomsky here, but don't worry, I really am a good guy.

Interesting theories do not avoid conflicts with data, but rather create clashes on purpose... Idealization involves counterfactual representation, by definition. In general, improvements in the structure of theories may lead to a (temporary) loss of descriptive adequacy...	I'm saying some things that look as if they're completely wrong, but what you've got to understand is that real scientists do this all the time, and it's completely kosher. My account is so much nicer, it's just mean to try and show that it's wrong about the mere facts.
It is entirely pointless to list arbitrary data from arbitrary languages in order to refute principles...	Don't waste my time bringing me your weird data from languages no one ever heard of when I'm trying to do some theory, O.K.?
Mathematical order is not directly reflected in the common sense classification of any domain of reality...	If you look for my beautiful constraints to leap out at you from your grubby field notes, you're in for a disappointment.
Classical mechanics... *makes assertions which not only are never confirmed by everyday experience, but whose direct experimental verification is impossible.*	In physics they say things that no one can possibly check up on. I just don't see why you trust those nerds in their white coats more than you're prepared to trust me.

What is so hilarious here is not the anodyne views in the left column. It is seeing them defensively plugged in as interlinear glosses in an actual research paper. It's quite true that real working physicists ignore facts incompatible with their theory, operate with idealizations (like perfect vacua) that render their claims untestable, refuse to consider certain phenomena relevant because of deliberately imposed limits on scope of theories, and so on. But what they don't do is comment self-consciously on this in their actual technical publications, or drone on about how wrong it would be for anyone to come along and try to say their hypotheses were not correct.

There are far wilder examples than Koster's article. For a really splendid one, look at Carlos Quicoli's 'Some Issues on [sic] the Theory of Clitics' (1982). This is a reply to Postal (1980), which itself was a critique of Quicoli (1980). The reader will have to be rather alert to keep straight about what is going on in the empirical dimension. Quicoli (1980) was supposed to be describing the standard French dialects discussed in such work as Kayne (1975), but cited some data from speakers who permit two dative clitics in one clause (e.g. **Je te le lui laisserai donner** 'I will let you (dat.) give it to him/her (dat.)') and thus do not instantiate the dialects Kayne was talking about. Quicoli did not initially appreciate that his informant was not giving him standard French, and thus was operating under a misconception in devising his analysis (see p. 231 of his article). Postal's allegation is that Kayne wrongly predicts *no* French speakers accept double-dative sentences, and Quicoli wrongly predicts that *all* French speakers should accept them. Quicoli's (somewhat baffling) response is to accuse Postal of holding the view that grammars should be able to describe mutually inconsistent dialects simultaneously. But before this, the reader has to suffer a whole section, nearly eight pages, about "conceptual issues". All Postal is saying is that Quicoli's paper is a wretched piece of descriptive linguistics, wrong at many points in its account of the syntax of French, and thus a very poor exemplar of the alleged merits of the theory in whose terms it is couched. He wants to get down to facts. But Quicoli leaps straight to the philosophy of science shelf, not the French grammar shelf, as if he had got out of the library elevator on the wrong floor. The quotation at the head of his section 3 reads

> There is no falsification before the emergence of a better theory (Imre Lakatos).

and the following section, about the data, has another quote, from Gerald Holton:

> Not only do brute facts alone not lead to science, a program of enthusiastic compilation of facts *per se* has more than once delayed the progress of science ... As the scientist-educator J. B. Conant has pointed out, "Science advances not by the accumulation of new facts... but by the continuous development of new and fruitful concepts".

Again one wonders who would doubt the truth of these platitudes. Why are they dragged out, and heavily embroidered upon, by Quicoli, who is only supposed to be responding to the charge that his analysis of French grammar is a crock? Does Quicoli really think Postal wants just to amass facts and not develop concepts? Surely not. Postal spends his life devising theories. (Sampson reports me as an enthusiastic proponent of them, remember?) Who can Quicoli be preaching to? Why is he rummaging through philosophy of science paperbacks to flesh out his protest? Linguists might listen if he presented a succinct account of the French facts that not only described them accurately but also revealed principles of some generality underlying that account. But I for one do not want to wade through eight pages of Quicoli raving about verification, theoretical constructs, quantum physics, raw data, falsifying experiments, conceptual voids, and so on (which has led, I noted with alarm more recently, to a further ten pages of philosophical discussion in a 56-page counter-attack by Postal (1983)). I want to see linguistic research in the journals I subscribe to, not philosophy term papers.

It wouldn't be so bad if it were good, creative philosophical analysis. But in fact the term papers one finds embedded in the work of the methodological moaners would in many cases get a B-minus at best. Many linguists have a rather uncertain grasp of philosophy.

I discern three main factions in philosophy of science. The first contains the logicians. They study topics like the logic of confirmation, the empirical status of counterfactual conditional claims, and so on. They cite Hempel and Popper, and their examples are about swans being white. The second faction contains the sociohistorians. They study issues like the emergence of scientific revolutions and the sociological preconditions for acceptance of new theories.

They cite Kuhn and Lakatos, and their examples are about brave physicists and chemists struggling on despite recalcitrant data and the disapproval of friends and relatives.

The third faction consists of Paul Feyerabend. What Feyerabend offers is not so much philosophy as guerilla theater for philosophers. His work is marvellous reading: bubbling wit, boiling invective, deep erudition, a constant twinkle in the eye – to read Feyerabend is to experience an intellectual analog of what dogs seem to enjoy when they get a chance to roll on their backs in a patch of fresh, crisp grass. But make no mistake: reading Feyerabend without appreciating that he is sending the whole business up is like mistaking Monty Python's Flying Circus for the Ten O'Clock News. In his celebrated book *Against Method*, for example, Feyerabend offers, tongue in cheek, a recipe for the destruction of science. Deadpan, he presents a purported methodology for modern scientists that will allegedly take them in the footsteps of their great heroes such as Galileo: develop theories that are in conflict with known facts; lie about the observational support for them; maintain them stubbornly in the face of objections; defend them by means of dishonesty and bluster. Feyerabend seems to be alternately amused and disgusted to see that there are people who read his satirical proposals as if seriously put forward (see e.g. his 'Marxist fairytales from Australia' (1978)). He would really get a kick out of seeing how linguists are solemnly citing him (see Hornstein and Lightfoot 1981, p. 29, note 5, for a wholly serious reference to *Against Method*), and how some seem to be actually trying to live by his ironically proposed principles.

If linguists understood a little more philosophy, we might be spared such things. And we might be spared the sight of Quicoli solemnly quoting Feyerabend's old friend Lakatos on the impossibility of "falsifying" (Lakatos really means "overthrowing") a theory without devising another theory to put in its place, confusing utterly his own job (defending hypotheses about the structure of French) and the job of future historiographers of linguistics.

Tomorrow's historians of linguistics might conceivably be interested in unraveling the psychological and sociological mystery of why Quicoli and Postal held on to their theoretical preconceptions so tenaciously, finding not one atom of agreement in the course of the 54 pages of Quicoli's original article and the 168 printed pages (so far) of debate about it. But they will not fall for the nonsense that more and more fierce transformational grammarians are prepared to dish out, about it being a "conceptual error" to suppose that the observation $\sim P$ falsifies a theory that predicts P. Of course it isn't a mistake to think that if you discover that $\sim P$ you have falsified a theory that entails P. It is, however, a mistake to suppose that anyone other than your enthusiastic proponents will listen to you.

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