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ON EXPLAINING LANGUAGE CHANGE

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FOR JAIME
‘We have got to the deductions and the inferences,’ said Lestrade, winking at me. ‘I find it hard enough to tackle facts, Holmes, without flying away after theories and fancies.’

‘You are right,’ said Holmes demurely; ‘you do find it very hard to tackle the facts.’

_The Boscombe Valley Mystery_

Deus enim omnibus providet secundum quod competit eorum naturae: est autem naturale homini ut per sensibilia ad intelligibilia veniat: quia omnis nostra cognitio a sensu initium habet. Unde convenienter in sacra scriptura traduntur nobis spiritualia sub metaphoris corporaliuum . . .

St Thomas, _Summa Theol._ Ia, q.1, a9R

Let us now assume that for certain remarkable facts I have no alternative explanation. Of course, that alone does not dictate acceptance of whatever theory may be offered; for that theory might be worse than none. Inability to explain a fact does not condemn me to accept an intrinsically repugnant and incomprehensible theory.

Nelson Goodman
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Any discipline whose main concerns are explanation and the justification of hypothesized unobservables must make its points by argument, not ostension. For this reason the quality of its argumentation crucially determines the quality of the knowledge it delivers, and the validity of its claims on the credence of outsiders. I take it that this is a reasonably uncontroversial, even trivial, point. I also take it that linguistics (synchronic or historical) is one of these argument-based subjects, and that just about anything interesting that linguists come up with is the result of a complex interaction between argumentative strategies and (ultimately largely theory-defined) ‘data’. And that because of this, the data are largely neutral with respect to most interesting conclusions – in the sense that they in no way ‘determine’ them, but at most, in conjunction with accepted argument types, suggest directions in which one might look for adequate solutions to problems.

Given this, it would seem that questions both about ontology in the widest sense (the nature of data and theoretical constructs) and methodology (the force and epistemic status of arguments) should be of pressing concern to linguists. But there are, within the profession, two attitudes toward this. One is that ‘metaworries’ (to use a term of Kiparsky’s, 1975: 204) are clearly a central concern, and that we are in something of a state of ‘crisis’ until we dispose of some of the worst ones. The other is that we should forget about them, rejoice in the ‘paradigm’ that a superficial reading of Kuhn (and generally none of late Popper, Lakatos, or Feyerabend) has convinced some of us that we have, and get on with the serious business of doing linguistics. Let the philosophers (if any are interested) worry.

In what might be called the ‘mainstream’ of linguistics (a place I seem, happily or not, not to inhabit), the second attitude prevails: at least this is a reasonable inference from the general apathy that seems to
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have greeted the work of Botha, Derwing, Anttila, Jon Ringen, Itkonen, and others concerned with these things (though the number of 'practising' linguists involved in the symposia reported in Cohen (1974) and Cohen & Wirth (1975) is a hopeful sign). I suggest that this attitude is unacceptable: if we don't identify and get rid of some of the worst metaworries, there will be precious little serious linguistics to do.

But linguists by and large are a rather inbred lot, and often seem not to worry about much except their intradisciplinary self-images, their positions vis-à-vis current orthodoxies and heresies, and the like. This is (apparently) what enables many to claim, for instance, to be 'empirical scientists', to pay lip- (or footnote-) service to philosophy of science (an occasional approving reference to Popper, or a self-congratulatory one to Kuhn) – all the while cheerfully practising axiomatics (Ringen 1975; Itkonen 1976b), hermeneutics (Itkonen 1974, 1976a, b) or some other non-empirical discipline (Lass 1976a: epilogue). It is also what enables them (and here I do not exclude myself from 'them') often to base major claims on arguments so weak, shoddy or confused that they would surely not pass muster outside the hermetic confines of the subject (cf. the discussions in Botha 1971; Botha & Winckler 1973; Linell 1974).

Since I am a practising linguist by profession, this book is to be taken as an insider's critical report: specifically on the status of some aspects of argumentation in one area of current historical linguistics – attempts at the explanation of linguistic (mainly phonological) change. I here disarm at least one type of criticism in advance by admitting that I have been guilty, in print, of most of the errors I attempt to anatomize. (Though except for a few relatively brief references I will stick, through modesty and a distaste for festivals of recantation, to the work of others. I see no reason why sinners should be denied the pleasure of casting the first stone, provided they identify themselves in advance.)

My conclusions will in general be both unfavourable to current practice, and pessimistic – at least for the positivistically inclined. I will try to show that the hitherto most popular explanatory argument types in this area are empty or fallacious, or at the very least seriously flawed; and that certain domains that are generally assumed to be amenable in principle to positivist (in this case deductive) explanation are in principle not so; and that certain weaker but still 'respectable' explanatory strategies (notably probabilistic explanations and explanations by 'natural tendency') are not in fact explanatory. And because of this, that most (if not all) of the objects that currently pass for...
explanations are not—in the light of standard metascientific orientations anyhow—acceptable. I will further try to show that the failures mentioned above lead directly to the collapse of what has recently (and at various times in the past) looked like a promising avenue for research: the provision of ‘functional’ explanations for language change. The discussion will also lead to a sharper focus on the vacuity of notions like ‘naturalness’ and ‘markedness’ (a point I have made elsewhere in a more preliminary way: Lass 1975).

I conclude that given current approaches to scientific and even semi-scientific epistemology, there are at present no intellectually respectable strategies for explaining linguistic change: the supposed explanations reduce either to taxonomic or descriptive schemata (which, whatever their merits—and they are considerable: cf. §5.6—are surely not explanations), or to rather desperate and logically flawed pseudo-arguments. Whether this is due to our (contingent?) shortcomings as linguists (‘gaps in our knowledge’) or to the nature of language and language change is of course debatable; though I will suggest that the latter is the case, and outline some tentative paths toward an alternative epistemology, and some rather different goals from those we usually accept.

A number of my colleagues have suggested, along the lines of the second of the two attitudes toward ‘metaworries’ detailed above, that the kind of thing I am trying to do here is counter-productive, obscurantist, and inappropriate for anyone who actually ‘does’ linguistics. But this separation between ‘real’ linguistics and ‘metalinguistics’ (the latter being a distraction) is surely untenable. To put it at its simplest, how do you go about practising your discipline if you’re convinced that much of the practice rests on shaky foundations? (Imagine a doctrinaire generative phonologist trying to give a convincing lecture on the validity of taxonomic phonemics.)

On a more sophisticated level, this attitude is rather Philistine. Surely it can only be healthy (in an amateur but not dilettante way anyhow) to be one’s own ‘philosopher’. That is, all the while one is thinking about language, to be thinking about thinking about language... etc. (Cf. Collingwood 1946: 1 on the value of this attitude for the historian: his remarks apply here as well.)

This seems to me a reasonable position. At the very least it can grant an occasional parole from the prison of one’s own conceptions and metaconceptions. It can also help deflate some forms of pretension and
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high seriousness, and provoke a bit of self-satire and scepticism about the whole enterprise of knowing things. This is often (though of course not always) a good cure for the worst kinds of humourless pomposity and the hubris of expertise. I would even assert programmatically that one of the major obstacles to ‘progress’ in any field is taking oneself or the things one is most interested in and committed to fully seriously, and getting ego-involved in one’s own theories. Whatever the substantive results of this enquiry, I take the informing attitude as one of its justifications.

Most of the more trenchant critics of current linguistic argument and theory approach the subject from one of two points of view: (a) as positivists (good old-fashioned, neo-, or crypto-) trying to show linguists how to be ‘good’ empirical scientists (Botha 1971; Derwing 1973; Ohala 1974; Sampson 1975b); or (b) as non-positivists trying to show that positivist approaches are either failures (Anttila 1976a, b) or frauds (Itkonen 1974, 1975; Ringen 1975): i.e. those crypto- or neo-positivists who say they are empirical scientists or whatever are hermeneuticians without admitting it, or mathematicians or analytical philosophers.

My approach here will be somewhat different. I will try to avoid the ‘what is linguistics?’ gambit, which is by now rather a bore (the right-minded know, and few of the Heathen are likely to be converted). I am more interested in looking at assertions and arguments, especially central and subject-defining ones, and trying to determine their logical and/or empirical validity, and their epistemic status. Or at least in raising questions for others to try and answer.

I would like to thank all of my colleagues whose work and conversation has deepened or otherwise interfered with my understanding of the issues I treat here. In particular, John Anderson, Richard Coates, and Jim Hurford, who commented in detail on an earlier draft of this work; as well as the following, all of whom have read parts of this or other work of mine bearing on similar themes, or have listened to oral presentations, and have given freely of their time and energy to discuss things with me: Kristján Árnason, Gill Brown, Paul van Buren, Gaberell Drachman, Steve Harlow, David Lightfoot, Lachlan Mackenzie, H. H. Meier, Jim Miller, Suzanne Romaine, and Nigel Vincent. I am also indebted to Cambridge University Press’ anonymous reader for helpful comments, and for sharpening my perception of some major philosophical issues, and to Mary Hesse for a
useful and pleasant afternoon of tutorial assistance. All these and the many I have forgotten are to be congratulated on whatever share they may have in anything good in this book, and absolved from blame for the rest: I am after all responsible for knowing when my friends and colleagues talk nonsense.

And finally, my deepest gratitude to my wife, Jaime, who put up with the long periods of abstraction and surliness that seem to go with writing, and is still willing to speak to me.

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