

Sociophonetic variation and generative phonology: the case of Tyneside English*

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Two criticisms of generative phonology are made by Docherty et al (1997). (1) The data on which generative analyses are normally based are unreliable, because they are not collected in a sociolinguistically sensitive manner. (2) Generative approaches to phonological analysis are less reliable than variationist ones because the former are theory-led, whereas the latter are data-led. I argue that the first claim is valid but that the second is untenable. I also raise some doubts as to the scientific status of variationist enquiry.

Docherty et al (1997) formulent deux critiques concernant la phonologie générative. (1) Les données sur lesquelles se fondent généralement les analyses générativistes ne sont pas fiables parce qu'elles ne sont pas rassemblées en tenant compte des faits sociolinguistiques. (2) Les approches générativistes de l'analyse phonologique sont moins fiables que les approches variationnistes parce que les premières sont guidées par une théorie alors que les secondes sont guidées par des données. Je montre ici que la première critique est valide mais que la seconde ne l'est pas. Je mets également en doute le statut scientifique de la recherche variationniste.

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1. Data and variation in generative phonology

A variationist approach to phonological investigation is defended by Docherty *et al* (1997), who argue that there are two problems with the non-variationist, generative approach. The first problem concerns the status of the material which generative phonologists typically present as representations of their data, namely sets of IPA (or other) phonetic symbols enclosed in square ('phonetic') brackets, such as [fɪʔt̚ɐ] ('fitter') vs [fɪɹɐ] ('fit her'), said by Carr (1991) to be pronunciations representative of the speech of Tyneside English speakers. One of the main claims made in Carr's paper is that the glottalised realisations of /t/ in Tyneside English are in complementary distribution with those (such as [ɹ] and [r]) which result from the application of the 'T-to-R' rule (what Carr calls Weakening), i.e. /t/ → [r]/[ɹ] (Wells 1982). Glottalised variants ([ʔp], [ʔt], [ʔk]) are said by Carr to occur in the following environments:

Morpheme-internally	Across morpheme boundary	Across word boundary
stupid	clipper	clip her wings
temper	clamber	clamp her down
pulpit	pulper	pulp it
pretty	fitter	*fit her
winter	chanter	chant it
alter	halter	halt her
reckon	wrecker	wreck her
hankie	thinker	think her strange
welcome	milker	milk her

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In the asterisked case (*fit her*), the T-to-R rule, rather than Glottalisation, is said to apply.

The main claims made by Carr (1991) with respect to the T-to-R rule are that:

(a) it occurs foot-internally in postlexically-formed feet (postlexical derived environment), but not in lexically-formed feet (cf *fit her* vs *fitter* above);

(b) it is sensitive to syntactic category:

it does not occur in nouns (with the exception of *lot*), adjectives or prepositions, even when they are monosyllabic and followed by an unstressed syllable;

it occurs in certain words belonging to non-lexical categories (*not, but, what, that*) and in verbs (*put, hit, met, get, got, thought, fit*). Even then, it is lexically variable, and:

(c) it is quite possibly undergoing lexical diffusion (the set of lexical items is expanding).

With respect to the application of Glottalisation in Tyneside English, Carr claims:

(a) that it applies foot-internally, and is blocked by presence of foot boundary; it does not apply, he claims, in any of the following sorts of case:

* appear, *attack, *accuse, * a peer, * a tack, *a cake, *up here, *at Easter, *suck oranges

(b) that it is an across-the-board postlexical process which applies *after* the postlexical process of T-to-R, via the Elsewhere Condition

Thus, T-to-R, but not Glottalisation, is said to apply in cases such as *fit her*, and the [r]/[ɹ] and [ʔ] realisations of /t/ are said to be in complementary distribution

These claims, if justified, have implications, Carr suggests, for certain claims made within Lexical Phonology concerning the sets of properties attributed to lexical *vs* postlexical rule application; it is those claims which constitute the focus of Carr's paper.

But Docherty *et al* point out that one is entitled to ask what the status is of data such as that provided above by Carr, which, they say, is typical of the sorts of data presented by generative phonologists when seeking to defend or criticise a given generative framework. Such data are typically not presented, in generative work, as phonetic transcriptions of specific, spatiotemporally unique utterances. Instead, they are generally taken to be representative in some way of the speech of the members of some speech community (as they are in this case). Docherty *et al* object that, in much work in generative phonology, such data are not collected by means of sociolinguistically sensitive data collection techniques, that no indication is given as to the number of speakers who acted as informants (or even whether there *were* any informants), their age, sex or social class. The worry is that such data may thus fail adequately to represent the actual speech of the members of the communities in question.

By contrast, their own approach does use such techniques, and, in the case of the two Tyneside English variants in question ([ʔ] and [ɹ]/[r]), reveals a quite distinct picture of their occurrence from that given by Carr (1991). Specifically, their research reveals the following:

(a) There are cases where Glottalisation *does* apply foot-initially (Carr's analysis claims that it never occurs in this position).

(b) There are cases where it applies *instead of* the 'T-to-R', rule, i.e. there is intra- and inter-speaker variation, with, e.g. [ʔt] ([ʔ] ?¹) in *got a nice jacket* but [ɹ] in *got a little bow*.

(c) Word list data do not exhibit the patterns suggested by Carr (1991), which are more appropriate, but still not accurate, for conversational data.

(d) There appears to be a 'lexical conditioning factor' for the occurrence of the [ɹ] realisation (292) (although this was, in fact, already suggested by Carr (1991), as shown above, and this was one of the main points of that paper).

(e) Sentence stress is possibly also a factor governing the application of Glottalisation and the 'T-to-R' rule, with 'T-to-R' more likely to occur when the main phrasal prominence is not located on the syllable where the /t/ is the rhymal consonant. Thus 'T-to-R' more likely in *get*¹*up* than in *fit*²*her*.

(f) 'T-to-R' has a much more restricted social distribution than Glottalisation.

(g) Glottalisation is blocked in utterance-final ('and other pre-pausal')³ positions.

¹ It is not clear from Docherty *et al* (1997) whether the variants in question are [ʔ] or [ʔt]. They at times (291-294) distinguish glottal from glottalised variants, but at other points (291-294) do not. It is crucial to the claim being made that we know whether the data from Hartley (1992) cited by Docherty *et al* shows glottal or glottalised variants: on the one hand, data from Hartley (1992) are said to show 'glottalised' variants (291), while those same variants are described on the same page as 'glottal'. I make it quite clear in Carr (1991) that I am discussing glottalised realisations, rather than glottal ones. If Hartley's data show glottal variants, they do *not* show, *pace* Docherty *et al*, the application of glottalisation, as discussed by me, although they are nonetheless problematic for my analysis.

² It is moot which syllable the /t/ occurs in here. If 'T-to-R' does indeed apply postlexically, then it is arguably in the second syllable, so the 'descriptive point' needs to be reformulated. This is a clear case of the relevance of 'theoretical' concerns for 'descriptive' claims.

³ It is not clear that pre-pausal position can be anything other than utterance-final, if a spoken utterance is a stretch of uninterrupted speech. Some other definition of 'utterance' may be intended, but if so, its definition is not offered by Docherty *et al*.

(h) In Tyneside English, Glottalling and Glottalisation, viewed from a sociolinguistic perspective, cannot easily be placed on a lenition scale, with Glottalling as the most lenited form. Thus Tyneside speakers might not be said to be implementing a process of 'lenition' (Note that this observation does not undermine the general idea of glottalling and glottalisation as lenition: Tyneside speakers may have borrowed a product of a lenition process).

(i) It is necessary to distinguish between the [r] and [ɹ] realisations, rather than categorise them together as Carr (1991) and Wells (1982) do, since [ɹ] is favoured by working class females, particularly those in the older group, but is rare in younger middle class speakers. The [r] realisation is more widely distributed socially.

Most of these points provide new and interesting data, and falsify the claim made by Carr (1991) that the glottalised realisations and 'R' realisations are in complementary distribution.

This first methodological point made by Docherty *et al* is a fair, and important, one: adoption of sociolinguistic methodology in phonology is likely to allow a more accurate picture of the speech practices of real speakers, whose speech is known to be inherently variable in several respects.

2. 'Theory-led' and 'data-led' approaches to phonology

Docherty *et al* (1997) raise a second objection to work in generative phonology. They argue that the kind of work they criticise is 'theory-led', rather than 'data-led', the implication being that 'theory-led' work is of more doubtful empirical (and thus scientific) status than 'data-led' work. However, the proposed distinction between 'theory-led' and 'data-led' work does not seem to be sustainable, precisely since all worthwhile scientific investigation must be simultaneously 'theory-led' and 'data-led'. Any attempt to apply Docherty *et al*'s distinction seems to run into immediate difficulties. Consider the paper by Paradis & La Charité (1997), published in the same volume of the *Journal of Linguistics* as Docherty *et al*'s paper. It is based on the Theory of Constraints and Repair Strategies. That would appear to indicate that it is 'theory-led', in Docherty *et al*'s terms. On the other hand, it is also based on a corpus of loan word pronunciations, which would seem to suggest that it is 'data-led'. Examples of work such as this abound in the phonological literature, and little of it seems clearly (or insightfully) characterisable in terms of the proposed 'theory-led' vs 'data-led' dichotomy.

One might, of course, argue that the distinction concerns the *extent* of the role played by theory as opposed to data in a given piece of work, but there appears to be no way of assessing this, and it is unclear what would be gained if one could engage in such an assessment. The distinction therefore seems to have no obvious practical application or conceptual use, and anyway appears

to be based on a conception of science (science as purely data-driven, not theory-driven) which is unsustainable and fails to fit with what is known about the history of science. As Popper (1959) pointed out, evidence from the history of scientific practice shows that there is no such thing as theory-free observation. Oddly, Docherty *et al* themselves acknowledge that this is so, an acknowledgement which sits uncomfortably with their proposed distinction. Further, the issue for Docherty *et al* is surely not whether phonological investigation should be 'theory-led' or 'data'-led, but whether the theory in question should be autonomist (factoring out variation) or variationist; this, in turn, will determine what is to count as reliable and relevant data for the theory in question.

It seems to me that the variationist case against autonomous phonology would be strengthened were the proposed distinction between 'theory-led' and 'data-led' approaches abandoned. Additionally, the distinction between autonomous and variationist phonology is surely not characterisable as one between a methodology based on systematically collected data in the latter case and non-systematic data in the former case, as Docherty *et al* suggest: *all* phonological data is necessarily systematically selected. What is at issue is the methodological and conceptual basis of the selection applied in the collection of data; that is where the strength of the variationist case lies.

One further problem with variationist work is that appeal *is* made to theoretical constructs made available by 'theorists', but the constructs in question are plucked, willy-nilly, from currently available theory as if they were not embedded in a theory, as if they were unproblematically observational terms. See, for example, Docherty *et al*'s use of the notions 'foot', 'sentence stress' and 'main phrasal prominence'. These are not unproblematically theory-free observational terms. Take the notion 'foot'. There are those who deny that the construct 'foot' is required in the description of English, and, among those who think it is, there is disagreement as to the structure of feet in English and their relationship to other postulated objects, such as 'the phonological word' (see, for instance, Fudge 1999). Docherty *et al* thus make crucial use of theoretical constructs without any apparent interest in, or expression of their commitment to, the theories from which those constructs are taken. The result is that the theoretical underpinnings of their use of such terms remain vague.

3. The scientific status of variationist phonology

Acceptance of the variationist objection to the sorts of data typically used in generative phonology, and thus of the importance of variationist studies, need not lead us to conclude that variationist phonology is not, like generative phonology, open to worries about its scientific status. It is arguable that variationist linguists, just as much as generative linguists, seek scientific status.

Evidence of the underlying concern felt among sociolinguists about the scientific status of their discipline comes from some of their uses of terminology. For instance, the expression 'second-order network contact', commonly appealed to by sociolinguists, and borrowed from social theory, corresponds exactly to the ordinary, everyday phrase 'friend of a friend': it contains no more conceptual content than the latter phrase. If we define 'jargon' as specialised terminology which does no more than recapitulate everyday terminology, then this use of terminology is properly described as mere jargon. Interestingly, this kind of usage stands in stark contrast to much of the terminology used in autonomous linguistics, such as, say, 'clausal complement to a transitive verb', which has no counterpart in everyday speech, precisely because social network membership is directly accessible to conscious awareness, and thus everyday discourse, whereas almost all of syntactic, semantic, phonological and morphological structure is not.

The question arises why such terminology is used at all in social theory (and thus in sociolinguistics), since it serves no scientific purpose. The answer to that question seems clear: it serves the purpose of making the discipline appear objectively scientific in status, and thus divorced from ordinary everyday discourse about the world. But therein lies a paradox for the sociolinguist. On the one hand, there is a desire, on the part of social theorists and sociolinguists, to belong to a properly scientific discipline, whose discourse is, of necessity, distinct from the realm of everyday discourse. On the other hand, there is a desire to regard sociolinguistic enquiry as somehow more connected to everyday reality than autonomous linguistics. My point here is not just that this constitutes a major source of intellectual unease for the sociolinguist, and for the social theorist, but that, if sociolinguistics is to turn to any discipline for scientific respectability, social theory is about the last place to look. It is, however, a field which sociolinguists *must* turn to.

Variationist phonology exhibits this problem. It must appeal to notions such as social class membership as theoretical constructs, but the scientific status of the notion 'social class' is even more open to question than the notions (such as 'syllable' or 'foot') appealed to in autonomous phonology. Consider the statement that speaker X is middle-class. Is this an observation statement? Apparently not, since it is not difficult to conceive of the claim being debated by X's acquaintances. Is it a falsifiable hypothesis? If so, what data would count as counter-evidence to it? I do not argue that there is no such data, and my objection here is not to the necessary idealisation involved in speaking of social class membership (or indeed of 'Tyneside English' or 'Parisian French'); what I do suggest is that it is at least arguable that there is no very clear-cut sense of relevant counter-evidence, and that this is worrying, given a conception of science in which falsifiability is taken to be the hallmark of

scientific hypotheses, especially since such statements appear to have to serve as observation statements in variationist work.

It is also worth noting that there is no guarantee that the notion 'middle class' means the same thing when applied to sociophonetic variation in Tyneside English, as opposed to, say, Midi French (Armstrong & Unsworth 1998). Nor is it clear, in real-time case studies which involve examining the speech of a given community at, say, fifteen year intervals, that the *meaning* of 'middle class' or 'working class' has not changed over time. For instance, it is likely that what it meant to be middle class in Norwich in 1968 was distinct from what it meant to be middle class in Norwich in 1983 (See Trudgill 1974, 1988). And if this is so, that undermines the presumption that categories such as 'middle class' may be taken to be held constant over time, so that the results of the passing of time can be observed, as distinct from class factors.

An additional worry stems from the fact that any discussion of social class membership must entail appeal to the notion of personal identity: whether a given speaker is to be viewed as middle class will depend partly on how the speaker views him/herself. But the notion 'personal identity' is a concept whose scientific status is, to say the least, open to question. The same remarks apply to categories such as age (is a given 35-year old individual middle aged or not? What kind of scientist could tell us? A sociolinguist?) and sex/gender (is my gay, cross-dressing friend male or female? What *scientist* might one ask?), and since these three categories are central to the sociolinguistic enterprise, it seems clear that some of the central concepts in that enterprise are problematic as far as their scientific status is concerned. If the scientific status of autonomous phonology may be called into question, the same is true, *a fortiori*, of sociolinguistics, and thus of variationist approaches to phonological phenomena. Paradoxically, then, autonomous phonology is, in one sense, the best contender for a phonology as a properly scientific discipline. But a fully autonomous phonology is unsustainable, since the data to be accounted for cannot be divorced from social context and are inherently variable.

Variationist work also appears to lack any interest in a coherently worked out set of assumptions about the nature of human cognition or the place of language within it. This is surely unsatisfactory in a discipline which stresses the importance of studying real speakers in real societies, since such speakers *must* be possessed of mind-internal representations which constitute their socio-phonological knowledge. The risk run by a sociolinguistics which is divorced from a coherently worked-out version of mentalism was well put by Fodor (1981: 282) in another context:

'If, then, the notion of internal representation is not coherent, the only thing left for a linguistic theory to be true of is the linguist's observations..... Take the

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notion of internal representation away from linguistic metatheory and you get positivism by subtraction.'

The same point is surely true of any variationist approach to phonological phenomena. And a positivist approach to scientific inquiry has been justly discredited for over half a century.

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