

On the Limitations of Linguistics Applied

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Linguistics has changed a good deal over the 20 years since this journal was founded, and this raises questions as to what implications this has for applied linguistics as, in some degree at least, a dependent area of enquiry. One obvious change is that linguistic analysis appears to have got closer to language experience in that it has broadened its scope to take in the externalized language of actual use. Since applied linguistics is concerned with language problems as experienced in the real world, it might appear that the two areas of enquiry in effect converge into one. This article examines two examples of such apparent convergence, corpus analysis and critical discourse analysis, where linguistic description makes claims to be directly relevant to 'real world' problems in language use and learning. It argues that in both cases what we have is linguistics applied whereby such problems are reduced and resolved by the imposition of necessarily partial linguistic account on the reality of language experience. This, it is argued, needs to be distinguished from applied linguistics, a mediating activity, more ethnographic in character, which seeks to accommodate a linguistic account to other partial perspectives on language so as to arrive at a relevant reformulation of 'real world' problems.

Applied Linguistics, the journal, has been going strong for 20 years, and this calls for some celebration. Apart from anything else, it would, on the face of it, seem to indicate that applied linguistics (the enquiry) is well established. Certainly institutional status has been comprehensively conferred upon it by any number of associations, courses, conferences, and, of course, journals. There are past achievements to celebrate and future prospects to look forward to as we move confidently into the new millennium, etc., etc. . . . And yet.

In spite of all this, there is a persistent and pervasive uncertainty about the nature of the enquiry. Its institutional establishment as a name does not correspond with any very stable definition of just what it is. It is a phenomenon, one might mischievously suggest, a little like the Holy Roman Empire: a kind of convenient nominal fiction. This may be no bad thing, of course; indeed it is perhaps not in spite of, but *because* of this uncertainty that applied linguistics has flourished. The persistence of religion shows that nothing is more enduring than mystery, and it may be that applied linguistics flourishes in like manner precisely because it is so conceptually elusive. At the same time, there is a niggling dissatisfaction that one might be engaged in activities which are so indeterminate as to defy definition. When this journal was founded, its editors were careful not to circumscribe its scope too specifically on the grounds that this might inhibit developments in the field (whatever that was) and in the hope that in time it would define itself as

a function of the contributions it attracted. No such clear definition has emerged, and so it is that from time to time the underlying uncertainty about the scope and status of applied linguistics breaks surface. The most recent example is the collection of papers in the *International Journal of Applied Linguistics* (Vol 7/1 1997), and the debate it gave rise to in the following volume (8/1 1998).

I was myself engaged in this debate, and so the question of the nature of applied linguistics as a field of enquiry is still fresh in my mind. And as the debate makes clear, the issue is a highly contentious one that raises quite fundamental questions about academic identity. So it would seem to be an appropriate one for me to deal with: a basic issue in applied linguistics well suited to the anniversary issue of the journal that bears its name. One might take the view, of course, that people who call themselves applied linguists should stop agonizing about the nature of their enquiry, and just get on with it. The difficulty about that is that the enquiry then either risks becoming a matter of pragmatic and unprincipled expediency, something that anybody can turn their hand to, or an appendage to another discipline and subject to *its* principles. So there does seem to be a point in trying (yet again) to get clear what, if anything, is distinctive about the kind of enquiry that this journal, nominally at least, claims to promote.

The main source of the uncertainty I have referred to lies, of course, in the relationship between the terms in the name itself. There is a presumption of dependency to begin with: it seems obvious that linguistics is primary: you have to have it first before you can apply it, and as linguistics changes its scope of enquiry, so the scope of application will (presumably) change accordingly. In this view, it is linguistics that calls the tune. So long as linguistics was defined, along traditional and formal lines, as the study of abstract systems of knowledge idealized out of language as actually experienced, the task of applied linguistics seemed relatively straightforward. It was to refer such abstract analyses of idealized internalized I-language (to use a term from Chomsky 1988) back to the real world to find ways in which experienced, externalized E-language could be reformulated so as to make it amenable to benevolent intervention. It all seemed straightforward enough: linguistics decontextualized language from reality, and applied linguistics re-contextualized it, and reconstructed reality in the process. In this respect, linguistics was the science (like physics) and applied linguistics its technology (like engineering).

Over the past 25 years or so, however, things seem to have become increasingly less straightforward. The dominant orthodoxy has been challenged and linguistics has breached its traditional formalist limits (and some linguists, of course, never accepted them anyway) and has extended its scope to take in aspects of Externalized language (E-language). There is now a fairly widespread conviction that linguistics should concern itself not with idealized constructs but with the reality of language as people actually experience it: as communication, as the expression of identity, as the means

for the exercise of social control. The term *real* indeed is often used freely as a general stamp of commendation. So long as linguistics dealt with idealized remote abstractions, there was a role for applied linguists to play in referring them back to the reality of lived experience. But if linguistics now accounts for that reality, what, one wonders, is there left for applied linguists to do? It would seem on the face of it that, as with Othello, its occupation's gone.

There is, of course, no point in protecting an occupation that has no purpose. One might argue that applied linguistics is necessary as a complementary activity to compensate for the limitations of formalism, but quite naturally becomes redundant once a more comprehensive view of linguistics is in place. If it is the case that linguistics now does indeed incorporate the concerns of applied linguistics, then this is all to the good. But does it? I want to argue that although the scope of linguistic enquiry has extended to take in the *data* of E-language, the *mode* of its enquiry is such as to give only a restricted account of experienced language. The perspective that it adopts can provide only a partial view. This does not invalidate it. On the contrary, its very partiality yields insights which would be impossible otherwise. But it does set limits on its claims to represent reality in the round and to provide an authoritative basis for intervention. Difficulties arise when these limits are not recognized and when its partiality is imposed: in other words, when linguistics is directly applied.

In this article I want to explore some of these difficulties and to argue that they point to the continuing relevance of the distinction (which I discussed in the first volume of this journal: Widdowson 1980) between linguistics applied and applied linguistics. Both are involved in intervention, in the referring of linguistic insights of a theoretical or descriptive nature to some language problem in the real world. And the reference will always involve some reformulation of the problem as perceived by those who have it, the claim being that this thereby makes it more amenable to solution. The difference between these modes of intervention is that in the case of linguistics applied the assumption is that the problem can be reformulated by the direct and unilateral application of concepts and terms deriving from linguistic enquiry itself. That is to say, language problems are amenable to linguistic solutions. In the case of applied linguistics, intervention is crucially a matter of mediation. Here there is the recognition that linguistic insights are not self-evident but a matter of interpretation; that ideas and findings from linguistics can only be *made* relevant in reference to other perceptions and perspectives that define the context of the problem. Applied linguistics is in this respect a multilateral process which, of its nature, has to relate and reconcile different representations of reality, including that of linguistics without excluding others.

If this distinction holds, then it follows that no matter how extended the scope of linguistics into the real world its unmediated application can never 'become' applied linguistics because it will always represent that reality linguistically, on its own terms and in its own terms. If it is to retain its

character as a discipline of enquiry, it can hardly do otherwise. But there is the danger that the extension of linguistics into E-language, and therefore of its range of potential applicability, may lead us to suppose that it does indeed incorporate applied linguistics, and has the authority to propose solutions to language problems which can only be properly formulated by mediation.

I would argue, then, that linguistics applied is, in effect, misapplied linguistics. And I want now to give some substance to this argument by giving detailed (and critical) consideration to two developments in E-language description that have become extremely influential in our field over the 20 years since this journal was founded. One of these is corpus linguistics: the quantitative analysis of text *en masse*. The other is critical linguistics: the qualitative analysis of particular texts. Each claims to have something quite radical to reveal about language use: corpus analysis about the language that people actually produce, and critical analysis about what they really mean by it. And each also makes claims for the relevance of their analyses to the formulation of problems as experienced in the real world which I believe to be questionable. In this respect, both are, to my mind, examples of linguistics applied. They warrant close attention because an examination of their analyses and the significance claimed for them seem to me to bring out the issues I have raised in clear relief. Furthermore, the identification of shortcomings (as I see them) at the same time, more positively, points out where applied linguistics might come in.

Corpus linguistics first. There is no doubt that this is an immensely important development in descriptive linguistics. That is not the issue here. The quantitative analysis of text by computer reveals facts about actual language behaviour which are not, or at least not immediately, accessible to intuition. There are frequencies of occurrence of words, and regular patterns of collocational co-occurrence, which users are unaware of, though they must be part of their competence in a procedural sense since they would not otherwise be attested. They are third person observed data ('When do they use the word X?') which are different from the first person data of introspection ('When do I use the word X?'), and the second person data of elicitation ('When do you use the word X?'). Corpus analysis reveals textual facts, fascinating profiles of produced language, and its concordances are always springing surprises. They do indeed reveal a reality about language usage which was hitherto not evident to its users.

But this achievement of corpus analysis at the same time necessarily defines its limitations. For one thing, since what is revealed is contrary to intuition, then it cannot represent the reality of first person awareness. We get third person facts of what people do, but not the facts of what people know, nor what they think they do: they come from the perspective of the observer looking on, not the introspective of the insider. In ethnomethodological terms, we do not get member categories of description. Furthermore, it can only be one aspect of what they do that is captured by such quantitative analysis. For, obviously enough, the computer can only cope with the material products of

what people do when they use language. It can only analyse the textual traces of the processes whereby meaning is achieved: it cannot account for the complex interplay of linguistic and contextual factors whereby discourse is enacted. It cannot produce ethnographic descriptions of language use. In reference to Hymes's components of communicative competence (Hymes 1972), we can say that corpus analysis deals with the textually attested, but not with the encoded possible, nor the contextually appropriate.

To point out these rather obvious limitations is not to undervalue corpus analysis but to define more clearly where its value lies. What it can do is reveal the properties of text, and that is impressive enough. But it is necessarily only a partial account of real language. For there are certain aspects of linguistic reality that it cannot reveal at all. In this respect, the linguistics of the attested is just as partial as the linguistics of the possible.

Problems arise when this partial description is directly applied to determine language prescription for pedagogic use, when claims are made that this provides the only language worth teaching. Now that we know what real language looks like, the argument runs, we expose learners to it and rid our classrooms of contrivance. This follows the common tradition of dependency whereby the language subject is designed in reference to, indeed in deference to, developments in the linguistics discipline. So it was that previously structuralist linguistics defined language content in terms of the formal units of the possible. So it is now that corpus linguistics defines language content in terms of the authentic patterns of the attested. Linguistics applied in both cases. For in both cases, what is not taken into account is the pedagogic perspective, the contextual conditions that have to be met in the classroom for language to be a reality for the learners. Whether you are dealing with the possible or the attested, you still have to make them appropriate for learning. And it is just such conditions that applied linguistics has somehow to take cognizance of.

There are two points (at least) to be made about the direct application of linguistic description of the kind that corpora provide, and both are fairly obvious. The first is that the textual product that is subjected to quantitative analysis is itself a static abstraction. The texts which are collected in a corpus have a reflected reality: they are only real because of the presupposed reality of the discourses of which they are a trace. This is decontextualized language, which is why it is only partially real. If the language is to be realized as use, it has to be recontextualized. The textual findings of frequencies and co-occurrences have to be contextually reconstituted in the classroom for their reality to be realized, and this reconstitution must obviously be based on very different contextual conditions than those which activated the texts in the first place. The contextual authenticity from which textual features originally derived cannot be ratified by language learners precisely *because* they are learners and do not know (yet) how to do it. It is sometimes assumed to be self-evident that real language is bound to be motivating, but this must depend on whether learners can *make* it real.

The first point, then, is that however the language is to be contextually abstracted, as units of the possible or the attested, they have to be recontextualized in the classroom so as to make them real for learners. And effective for learning. This is the second point. All language is realized as use in respect to some purpose or other. The purpose of language use in the classroom is to induce learning, and it is appropriate to the extent that it fulfils that purpose. There is a widespread assumption that the classroom is of its nature an unreal place and that this has to be countered by having it replicate the world outside as closely as possible. In the foreign language classroom, this world is taken to mean that of the native speakers of the language concerned. But there seems no good reason why the classroom cannot be a place of created context, like a theatre, where the community of learners live and move and have their being in imagined worlds, purposeful and real for them. To conceive of the classroom in this way is to acknowledge that what is being taught and learned is something designed as a *subject*, not the language as experienced by its native speaker users but something that native speakers cannot experience at all, namely a foreign language. And its foreignness has to be locally accounted for by the devising of appropriate contexts in the classroom which have to activate the process of learning.

This design of the subject is the concern of applied linguistics, whereby descriptive findings are pedagogically treated to make them appropriate as prescription. And the findings of corpus descriptions are potentially highly serviceable to this purpose. It would be a grave mistake to disregard the attested, as it would be to disregard the possible. After all, for many learners at least, the language as realized by its users is the goal to which they aspire and to which they will seek to approximate by the process of gradual authentication. But it would be equally mistaken to suppose that what is textually attested uniquely represents real language and that this reality should define the foreign language subject. A number of people concerned with foreign language pedagogy have expressed reservations along similar lines about the assumption that the findings of corpus linguistics should determine the content of the language subject (Cook 1998; Owen 1993; Widdowson 1991).

It is important to stress that the expression of such reservations does not amount to a denial of the pedagogic potential of corpus description, particularly, perhaps, in the *process* of analysis, in the use of concordancing to develop discovery procedures for learners (see Tribble and Jones 1998; Wichmann *et al.* 1997). Nor do these reservations betoken a conservative allegiance to outmoded ideas or a stubborn refusal to countenance change, as has sometimes been suggested. Rather it is an effort to refer these descriptive developments to applied linguistic principles by subjecting them to critical appraisal, so as to establish criteria of relevance. It is, of course, important that we should take new modes of description, and their findings, into account in the design of language instruction, but that is very different from accepting them on trust and without question.

To make the point more clearly, let me refer to a particular example. It has been John Sinclair's innovative vision more than anything else that has been the impetus behind developments in the corpus description of English, and he speaks with unique authority as a linguist. He has recently offered a number of precepts for language teachers, the first of which is: *Present real examples only*. These precepts are, as he himself acknowledges, based entirely on descriptive data: 'They are not concerned with psychological or pedagogical approaches to language teaching' (Sinclair 1997: 30). But it seems obvious that if they do not take pedagogic considerations into account, they cannot reasonably be taken as pedagogical precepts. As proposals informed, and so limited, by a linguistic perspective, they may well be worth thinking about, but that is another thing. And to be critically cautious in this way is not at all to confirm teachers in the belief that they know everything they need to know about the language they teach. But Sinclair thinks it is:

A few leading figures in applied linguistics (e.g. Widdowson 1992¹) effectively endorse this complacency by casting doubt on the relevance of corpus findings to the process of teaching and learning languages (Sinclair 1997: 30).

But to cast doubt is to express uncertainty about a claim, not to reject it out of hand. It can only be construed as negative if you assume the self-evident validity of linguistics applied. But from an applied linguistic point of view, casting doubt on the relevance of linguistic description for pedagogic prescription is, I would argue, precisely what we should be about. And this is particularly so in this case. Since, on Sinclair's own admission, the claim for relevance is not informed by pedagogic considerations, it seems only reasonable to entertain some doubt. Indeed, I would argue that the value of such proposed precepts is precisely *because* they provoke a critical response. The alternative is to accept the relevance unilaterally as a self-evident fact, and this means to fashion pedagogic reality to fit the descriptive findings: a clear case of linguistics applied (see also Aston 1995).

I have argued that corpus linguistics provides us with the description of text, not discourse. Although textual findings may well alert us to possible discourse significance and send us back to their contextual source, such significance cannot be read off from the data. The factual data constitute evidence of the textual product: what evidence they might provide of the discourse process is a matter for further enquiry. The same is true, I think, of the other area of description I want to consider. In spite of its name, critical discourse analysis is, I would maintain, also an exercise in text description. And it, too, has a way of assigning discourse significance to textual facts. The supposed area of relevant application is here, however, much broader and of much greater moment. Whereas corpus descriptions have been brought to bear on matters of language pedagogy, critical discourse analysis is concerned with education in a more general sense: it is directed at making people more sociopolitically aware of the way language is used to manipulate them. The

purpose is to be applauded. It is hard to think of one which is of greater social significance or more squarely within the scope of applied linguistics. But the question needs to be raised again as to what kind of enquiry this is, and whether here, too, we should take the relevance of its findings on trust, or cast a doubt or two.

I propose to cast a doubt or two. But again I should make it clear that I do not do this in any complacent or captious spirit. The study of the ways in which language is used to construct convenient realities and persuade opinion is of enormous social significance. And it should be acknowledged that work in critical discourse analysis has had the effect of alerting us to this significance, and provoking us to enquire more closely into the institutional use and abuse of language in public life. But enquiring more closely necessarily also means raising questions about the findings of this analysis and the procedures employed in getting them. It would be particularly perverse if critical linguistics of all things were to protect itself from criticism. As with corpus linguistics, my view would be that its value is precisely that it provokes appraisal.

What critical linguists typically do² is to make inferences about ideological intent on the evidence of textual features. But what is the nature of this evidence? The recognition that a piece of language *is* a text is a matter of inferring that it has a discourse origin: that it is the product of a process. In spite of this, linguists still routinely talk about text as being stretches of language *above the sentence*, and, as I have pointed out elsewhere (for example, Widdowson 1995) this is to define it in formal terms, as a unit of the same kind as the sentence but bigger in extent. Apart from anything else, this leaves out of account a whole host of texts that take the form of single sentences, or single constituents thereof, for a text can comprise a single word or even a single letter. The point is that what is attested in a text is not a formal manifestation of the code but a functional realization of some appropriate contextual use. As such, it carries implications of discourse and you recognize it *as a text* when you recognize there is some intention to make a contextual connection.

But text recognition is not the same as text realization, for this latter involves interpretation, that is to say the deriving of a discourse from it. Here you do not just recognize intentionality, but you realize particular intentions, inferring what the first person might have meant by the language. The question is: on what basis do you make that inference? What textual evidence can you reasonably adduce in the assignment of intention? That is one question, which involves consideration of first person perspective. There is a second: on what grounds can you reasonably assume that your assignment of intention is in accord with the way the text is discursively realized by others? This is a matter of interpretation and brings in considerations of second person perspective: it concerns not what can be read from a text, but what can be read into it; not what first persons might have meant by it, but what it might mean to second persons.

These issues are obviously crucial ones for anybody concerned with assigning significance to text, and especially if, as with those committed to a critical approach, your purpose in so doing is to discover ideological intentions which are deliberately disguised to persuade opinion. For to do this you have to be confident that the perspective you adopt will indeed provide privileged access to covert significance, and is to be preferred to that of other people of less informed perceptions. The commitment of critical linguists carries a weighty responsibility with it. It is all the more important that their analysis should be well-founded on the careful (indeed critical) consideration of these issues. But is it?

I want to consider this question by making specific reference to an article by Michael Stubbs which first appeared in *Applied Linguistics* (Stubbs 1994) and subsequently, in a revised version, in Stubbs (1996). I take this article not only because it is, I think, representative of the critical position I want to examine, but also because the very explicitness of the analysis that Stubbs provides brings out the issues I have mentioned with particular clarity. Furthermore, the expressed aim of the article is to remedy certain weaknesses in critical analysis (also discussed in Stubbs 1998) and putting it on a more secure empirical footing by linking it with corpus analysis, thereby bringing into conjunction the two developments in language description that I am concerned with. For all these reasons, this article is of particular relevance to my theme.

Stubbs presents an analysis of the language used in two school textbooks with a view to identifying textual traces of ideological bias. This is how he puts the assumption upon which the analysis is based:

Much recent text analysis, especially within 'critical linguistics', starts from the Hallidayan assumption that all linguistic usage encodes representations of the world. It is always possible to talk about the same thing in different ways, and the systematic use of different syntactic patterns encodes different points of view. As in chapters 4 and 5, I again start from this assumption, and will test whether the difference of ideological stance in the two books is expressed in their different use of the grammatical resources of English (Stubbs 1996: 130).

One obvious difficulty about basing your analysis on this assumption is that it is not practically possible to scrutinize every feature of a text for its ideological significance. This is true even of small texts, let alone the extensive ones that Stubbs is dealing with. So in practice what you have to do is to select certain features and disregard the rest, and this is what Stubbs does. He does not deal with the use in these books of the grammatical resources of English as a whole, but of one particular grammatical feature. This seems a reasonable thing to do on the grounds of feasibility, but it has the consequence that it appears to contradict the principle upon which the analysis is supposedly based. For in selecting a particular feature for special attention, you in effect accept that the disregarded parts of the text are redundant in that they are

irrelevant to the identification of ideological stance. But how does this square with the idea that ‘all linguistic usage encodes representations of the world?’ How do you know what other ideological significance might be lurking in features other than the one you have selected?

One answer to this might be that you have advance notice of which features are *likely* to carry the significance you are looking for. The selection of a particular feature is not random: it is informed by semantic considerations. Thus Stubbs identifies ergativity as a key indicator of ideological stance because ergativity is a grammatical device for encoding agentless action. So if, for example, you want to describe something as just happening on its own accord, thereby avoiding reference to any specific cause, the ergative is at hand to enable you to do so (*The vase broke* (ergative) vs. *Somebody broke the vase* (transitive)). If, therefore, it turns out that a text on, say, global warming makes disproportionate use of this form in describing the phenomenon, one might infer that the author’s intention is to avoid specifying who is responsible (it is just happening) and from this we can further infer ideological stance. But things are not so straightforward.

To begin with, the ergative, obviously enough, does not occur on its own: it necessarily contracts relationships with other grammatical features in complex ways. When ergativity is manifested as an intransitive form, it cannot encode meaning unaided but must necessarily be lexically embodied, and combined with tense and aspect.

For example, many of the instances cited by Stubbs exemplify ergative verbs combining with the perfective, as in:

1 *factories have closed* (Stubbs 1996: 133)

The question naturally arises as to how far the significance assigned to expressions such as (1) is a function of the perfective combined with the ergative rather than of the ergative itself. If the significant feature is indeed ergativity, then this would presumably be equally significant in association with other grammatical features of the verb phrase. Now, as grammarians tell us, the perfective signals completed action relevant to the present. In other words it allows us to focus attention not on the process but on the result. If we want to focus attention on process, we have another grammatical aspect available in English, namely the progressive. Thus we can contrast, for example:

2 *factories have closed* (ergative perfective: result)

3 *factories have been closed* (passive perfective: result)

4 *factories are/were closing* (ergative progressive: process)

5 *factories are/were being closed* (passive progressive: process)

What it would be interesting to know is whether there is a tendency for ergative verbs to occur more commonly in the perfective rather than the progressive, and this is just the kind of textual fact, of course, that corpus analysis can readily provide. But it is not provided here. One might speculate that the ergative effect that Stubbs identifies may have something to do with

the fact that the intransitive perfective (2) and its passive counterpart (3) have a formal resemblance, in that the auxiliary *have* and the past participle are common to both: *factories have (been) closed*. Indeed, one might suggest that there is some correspondence, that the intransitive perfective is a sort of shortened version of the passive, so that it is easy (deceptively easy) to slip from one to the other. The intransitive progressive bears no such formal resemblance. We might expect, therefore, that if people wanted to avoid the implication of agency associated with the passive, they would, wherever possible, use their ergative verbs actively in combination with the progressive rather than perfective aspect. Do they? I have no idea. This is something, of course, that could be investigated by elicitation, thereby yielding what I have called second person data. But even the analysis from a third person perspective of the kind we are presented with here might provide us with some evidence to go on. But this is ruled out if ergativity is isolated as a feature.

This matter of formal resemblances brings us to another grammatical distinction which bears on ergativity. As already indicated, the progressive is used to focus on process, the perfective to focus on a resulting state of affairs. But (again as grammarians have routinely pointed out) we have another means in English for signalling a state of affairs in dissociation from any process which might have brought it about, thereby avoiding implication of causation or agency, namely the stative:

6 *factories are closed*

Since the stative, in common with the passive and perfective, makes use of the past participle, it is not always easy to distinguish, and is prone to ambiguity. Again, it depends on how it combines with other linguistic features, including the semantics of the lexical verb. Thus example (6) here can be understood as a passive construction, and so as implying the possibility of an absent agent (*factories are closed (by unscrupulous managers)*), or as a stative construction, with no such implication (*factories are closed (on Sundays)*). In the first case, *closed* can be said to contrast with the verbal *opened*, but in the second the contrast is with the adjectival *open*. Precisely the same point can be made about Stubbs's own example (2) (*factories have been closed/opened (by unscrupulous managers)* vs. *factories have been closed/open (for years)*). But it is clear that the stative needs to be distinguished from the passive since what it encodes is a state which is not a result, and which carries, therefore, no implication of process at all.

If we are to talk of grammatical usage as encoding different 'representations of the world' these are surely crucial distinctions. But Stubbs conflates them, and indeed, he admits as much:

I studied just three patterns:

(T) transitive verb (VERB+NP)

(P) passive (mainly BE+VERB-ed)

(I) intransitive (VERB)

(Stubbs 1996: 134–5).

But this allows for VERB in (T) and (I) and BE in (P) to be realized in any tense or aspect form, thereby disregarding any semantic effect different co-selected features might bring about. Furthermore, in relation to the distinction just discussed (and whatever the term 'mainly' might cover) there would be no way of knowing whether the occurrence of BE+VERB-ed signalled a stative or passive function. The difficulty is, of course, that in this kind of analysis, you cannot indulge in semantic subtleties if you cannot program your computer to identify them, and so you end up with those which are manifest in forms that can be counted. So it is that even BECOME+VERB-ed, which one would suppose could always be unambiguously assigned a stative meaning, gets counted as (and so counts as) a passive. Stubbs acknowledges this in an endnote to his paper as published in *Applied Linguistics* (though omitted in the version which appears in Stubbs 1996):

Passive included constructions with BECOME and whiz-deletions (both infrequent). Further any form with BE plus -ed (plus, of course, other endings on irregular verbs, and plus phrases intervening between BE and past participle) was coded passive, with no distinction between stative and dynamic passives. For example, the following were both coded passive:

during World War 2, aircraft factories were dispersed,
the clothing industry is now dispersed (Stubbs 1994: 220).

It seems clear then that priority is given to the kind of formally marked criteria that the computer can readily recognize. So the distinction between passive and stative is ruled out as irrelevant, it seems, because it is inconvenient to manage.

But it is hard to see how it can be irrelevant if ergativity is to be associated, as it explicitly is in this analysis, with matters of blame and responsibility. One might argue that since the ergative freely combines with both the progressive and perfective aspect, it always carries implications of process and result, and so to some degree raises the question of causation. But since the stative encodes a state, it is at a further remove from causation, since you have first to infer an intervening process. If that is so, the stative might be said to encode a representation in which there is an even greater avoidance of implications of blame and responsibility. It goes beyond ergativity in the avoidance of causation or agency: it is not that this is the way things happen, but this is the way things are. It focuses on the status quo. It would appear that the very principle upon which this analysis is based, namely the assignment of significance to the occurrence of specific linguistic features, logically requires that the category of stative should be recognized, even if the computer cannot easily be programmed to recognize it.

In conflating stative and passive, Stubbs disregards the syntactic environment which activates one sense of the form rather than another. But it is just such an environment, we are told (in the 1994 version) which enables us to assign ideological significance:

the syntax co-selected with ergative verbs is likely to be ideologically significant, given its relation to the expression of topicality, agency, causality and responsibility. Such facts (sic) are often discussed in critical linguistics (Stubbs 1994: 207).

Which facts? Even if one restricts oneself to the syntax, and leaves lexical collocation out of account, it is precisely the effect of co-selected features that the analysis does not reveal, so we cannot take as given what 'its relation to the expression of topicality, agency, causality and responsibility' might be. The only fact we have is that certain selected formal features occur with a certain frequency. But it is not a fact that they are an index of ideological significance. It is true that critical linguistics often does discuss this as if it *were* a fact. That is just the problem.

The main source of the difficulties I have tried to identify in the Stubbs analysis can, I think, be traced back to the quotation considered earlier, and (applying the approved procedures of close textual analysis) we might submit it to careful scrutiny.

Much recent text analysis, especially within 'critical linguistics', starts from the Hallidayan assumption that all linguistic usage encodes representations of the world. It is always possible to talk about the same thing in different ways, and the systematic use of different syntactic patterns encodes different points of view. . . . I again start from this assumption, and will test whether the difference of ideological stance in the two books is expressed in their different use of the grammatical resources of English (Stubbs 1996: 130).

It is true that critical linguistics does, like Stubbs, start with this assumption, and indeed believes that Hallidayan grammar is especially well suited to its purposes. But what is this assumption exactly? It is expressed in this passage in three different ways:

- 1 all linguistic usage encodes representations of the world
- 2 the systematic use of different syntactic patterns encodes different points of view
- 3 different use of the grammatical resources (of English) expresses difference of ideological stance.

Halliday's view that usage encodes representations of the world is at one with his concept of code as social semiotic (Halliday 1994). Its forms reflect the functions it has evolved to serve, and his grammar is a functional one in that sense: it is informed by the history of its semiotic development. There are a number of things to notice here. First, representation in Halliday's conception is only one component of the grammar: it consists of the transitivity systems which formally encode the ideational function, that is to say, the first person's conceptual relation with third person reality as this has been semiotically sanctioned by the community concerned. Now if by point of view is meant first person perspective on third person reality, it seems

reasonable to associate this with representation. But ideology would normally be associated also with an interpersonal effect on the *second* person, and it is certainly this effect that concerns critical discourse analysts. But the interpersonal function of language is accounted for in a quite separate set of systems in Halliday's grammar (the systems of Mood as distinct from Transitivity) and so if one uses Hallidayan grammar as a frame of reference, ideology as an interpersonal phenomenon cannot be a matter of representation.

And this brings us to a second point. A grammar, no matter how functionally informed, is an analytic device. Thus its systems are abstract constructs which separate out the underlying elements of language and assign them separate semantic signification. Texts can be used to exemplify them, so you can point out, for example, that here you have a certain kind of process clause from the transitivity systems, and here you have another. But texts do not realize these abstract constructs in isolation. You may, and indeed you have to, isolate features, divide and rule, to make the kind of formal generalizations that constitute a grammar. But in texts, as I have argued earlier, the features combine and act upon each other in all manner of ways. It is one thing to say that syntactic patterns semantically encode reality, but quite a different thing to say, as Stubbs does, that their pragmatic use does the same. *Using* such patterns does not seem to me to be a matter of encoding at all.

As I have argued elsewhere (Widdowson 1997) a grammar, of its analytical nature, cannot account for the way language functions synthetically in text. The difficulty is that Halliday's functional grammar appears to claim that it can. There is indeed a part of the grammar which is said to account for the textual function of language. This deals with thematization, the various ways that the constituents of syntactic structure can be sequentially ordered. But the very fact that a textual function is singled out for separate treatment would normally be taken to imply the non-textual functioning of the other components of the grammar. And we should note that the textual function in Halliday's sense is not what Stubbs is concerned with in his textual analysis: he is looking at the significance of transitivity, a formalization of the ideational function which appears as a quite separate component of the grammar.

It seems to me that the difficulty about his textual analysis is that it is done on (and in) grammatical terms. What he does is to demonstrate how a particular grammatical feature is manifested in the texts he is dealing with, but he does not show how this is realized in association with other co-selected features. He makes the assumption that the semantic meaning that the grammarian identifies as having become inscribed in a particular grammatical category is carried intact into the text, so that textual meaning is a sum of the parts rather than a set of mutually modifying relations.

And this assumption is indeed, it would seem, one that he shares with Halliday. For Halliday, too, apparently makes no distinction between

grammatical and textual units of meaning. The functions which are formally encoded in grammar simply get reanimated when used in texts. So the grammar 'is at once both a grammar of the system and a grammar of the text' (Halliday 1994: xxii). It is not surprising then that critical discourse analysts should find Halliday's grammar so well-suited to their purposes. It is tailor made for them: 'The aim has been to construct a grammar for the purposes of text analysis' (Halliday 1994: xv). This would seem to suggest that if you can identify part of a text as manifesting a grammatical feature of whatever kind, all you need to do is to read off the meaning it encodes. There would in this case be no difference between semantic signification and textual significance. The text is thus taken to be a static patchwork. The dynamic inter-relationships which grammatical features contract with each other are disregarded as irrelevant.

The same point applies, of course, to lexical features too. O'Halloran, in a careful critique of critical discourse analysis (O'Halloran 1999), demonstrates how Fairclough applies the patchwork principle in his analysis of a television documentary about world poverty. Fairclough claims that the programme represents the poor as passive in that expressions that refer to them figure grammatically as Patients and not Actors. Such an assignment of significance to a grammatical feature is very much along the lines we have discussed in Stubbs's analysis. But at one point in the text, the poor apparently cease to be passive, and, inconveniently, take on the semantic role of Actor:

the poor people flock to the city . . .

Fairclough gets round this difficulty by abandoning syntax as a criterion for passivity and turning to lexis instead:

Interestingly, the Action here is one more usually associated with sheep—notoriously passive—than people, so the exception does not really contradict what I have said so far (Fairclough 1995: 113).

Fairclough assumes here that the usual association of the word *flock* with *sheep* will transfer intact into the text and prevail over any association it might contract there. But the word *flock* also denotes other groups: birds for example, and indeed people, which are not at all 'notoriously passive', and since the word *people* is actually co-selected here, as Stubbs might put it, it is surely this association which would be realized in normal reading. As O'Halloran points out, 'the lexical environment effects closure on the possibility that *flock* refers to a collection of sheep.' Now of course there will be occasions when a meaning which is imported into a text will prevail over textual influence, as in the obvious case of metaphor. And if it really were the case that the action of flocking is 'more usually associated with sheep' then one could argue that this counteracts the effect of the immediate lexical environment. As we have seen, the claim for 'usual association' cannot be sustained by appeal to the denotation of the word since this covers groups other than sheep. But it may be, of course, that one denotation is more commonly attested in texts than

another, and strength of association might then be measured by reference to actual collocational co-occurrence. And this, as Stubbs argues, is where corpus descriptions come in. If Fairclough's claim of 'usual association' is supported by evidence of collocational frequency, then we might concede on these grounds that he has a case: the strength of habitual lexical association can be said to override the textual effect of a particular lexical environment.

With this in mind, I consulted the British National Corpus and was provided with a random selection of 150 occurrences of the lemma FLOCK (from a total of 759), of which 124 give clear indications of meaning. Of these, 107 are nouns, and 50 are used in reference to sheep, although in no case does the collocation give any sign of an association with passivity; 39 of the nouns refer to birds. These quantitative facts do not really provide overwhelming justification for an ovine (as distinct from avian) reading of the word in this text. The remaining 18 nouns all refer to people: but of these 12 are in the figurative sense of 'congregation' (*The vicar wants his flock . . .*), which I suppose might suggest some degree of passivity, but others (referring to pressmen, fans, female relatives) decidedly do not. So far, the actual descriptive evidence gives no very secure warrant for the idea that the word *flock* is usually associated with passive sheep.

It might be argued, however, that this is not the relevant evidence since the 107 instances of the lemma so far mentioned are all nouns, whereas in the text that Fairclough is talking about it figures as a verb. Now as has been frequently demonstrated, different morphological forms and syntactic categories of a lemma may enter into different collocational patterns, so what is really relevant as evidence for the Fairclough assertion is the occurrence of FLOCK as a verb. The BNC sample gives 17 instances of FLOCK as a verb. And every one of them describes human actions. Sheep do not flock (nor do birds for that matter) but people do. So on the evidence of this BNC sampling of actually attested occurrence, the action of flocking is not usually associated with sheep—it is usually associated with people, and pretty active unsheepish ones at that: railway enthusiasts, journalists, golf spectators, gold diggers all flock. There is no textual evidence whatever for the usual association that Fairclough invokes, and on which his interpretation depends. On the contrary, it is the association with people that is usual: the expression *the poor flock to the city* is in actual (textual) fact collocationally entirely normal. So it does not constitute an exception and so *does* really contradict what he has to say.

This example of what can only be called tendentious interpretation is not, regrettably, just a momentary lapse, for it seems to be endemic in a good deal of critical discourse analysis (for further examples and discussion see Widdowson 1996, 1998a). It is one of the purposes of Stubbs's work to correct such partiality by relating particular textual features to the more general patterns of co-occurrence that the computer analysis of text can readily provide. Of course, as I have argued earlier, the computer can only reveal what is actually attested as overt behaviour, so we should be cautious

in our conclusions. Fairclough could still quite legitimately argue that whatever the print-out may show, people *do* nevertheless associate flock with sheep in their minds. To take this line is to invoke the Rosch notion of the prototype (Rosch 1975) and this, as I have argued myself (Widdowson 1991) does not necessarily find expression in actually attested language. But then we need to shift from a third to second person perspective whereby we seek to establish the prototypical association empirically by elicitation, as Rosch does. It will not do to assert the association on the basis of unsubstantiated first person response. It is curious to note (in passing) that in doing this, Fairclough follows the same procedure as the formal linguists: he consults his own first person intuition as if it were representative of everybody else's. So although the concordance cannot provide definitive proof, it can act as some kind of corrective to the kind of unfounded assertion that I have been criticizing in that it would require *some* evidence at least for impressionistic claims. As Sinclair has repeatedly pointed out, the great advantage of computer analysis is precisely that it can reveal the unreliability of the kind of intuition about language that Fairclough relies on. So it would seem only a sensible precaution to check your intuitions, affected as they are bound to be by your ideological bent, against the factual data of a concordance print-out.

But paradoxically enough, the use that Stubbs makes of the computer in his own analysis also leads, as I have indicated, to a similar inference of significance from features in isolation. What he deals with is the quantitative occurrence of a particular grammatical form across texts, but not the co-occurrence of this form with others or the qualitative effect of their interaction. It is curious that he should not do so, since he makes extensive use of concordance evidence elsewhere in his work (for example, Stubbs 1996: ch. 4). But in this analysis, it appears that like Fairclough he makes the assumption that significance can be read off from separate linguistic items, be they lexical or grammatical, without regard to textual modification. One can perhaps, unkindly (but taking a leaf out of the critical linguist's book) read significance into the very title of his paper (in its original version): 'Grammar, text, and ideology'. You read attitude or point of view off from the text which itself consists of grammatical units and so the point of view is encoded in the textual features which are the same as grammatical ones. That is to say, grammar encodes text encodes discourse.

So far, I have been discussing the need to take internal co-textual relations into consideration if we are to account for how texts mean. But as I have already indicated, a text is only a text at all if one infers intentionality and so recognizes its discourse implications. You cannot read off significance from text as if it were a simple projection from textual features, and you could not do it, even if you managed to account for their intratextual relations. For with discourse we have to consider not just co-textual but contextual relations, and these too, of course, have a modifying effect.

To take one rather simple example: one might agree that in certain

circumstances one can read evasion of responsibility into the intransitive use of the ergative verb in an expression like

7 *Industrial premises and shops were closing*

because in this case there is a passive alternative available which encodes causation, namely:

8 *Industrial premises and shops were being closed*

But if it were the intransitive alone which encoded such significance, then it would apply equally to an expression like

9 *The shops in Oxford Street close at six*

It seems to me that an utterance such as (9) is less likely than (7) to be pragmatically interpreted as evasive and an example of ideological stance. This, one might suggest, is because in the case of (7), prompted by the lexical collocation, and perhaps by the co-selected features of continuous aspect, we are likely to take the term *shops* as being pragmatically cognate with industrial premises, and knowing the world as we do, we therefore infer that *close* means *close down*. But such meanings are surely less likely to be activated in the case of (9) because the expression invokes quite different contextual associations. Here *shops* are places where you buy things which have normal opening and closing hours.

Or (still with *shops* in mind) consider the case of a text consisting of a single word, like the notice *CLOSED*. Here there are no intratextual relations which might lead us to assign it a stative or passive sense. But its extratextual location on the shop door leads us to understand it as a stative: it is the encoded contrast with *OPEN* that is contextually activated, not that with *OPENED*. The shop is in a closed state, and nobody in their right mind surely, no matter how critically disposed, would enquire into process or causation. The shop is closed and that's that. A *closed shop* as a collocate, however, is another thing altogether. It has to do not with a shop door but a shop floor. Here we might be inclined to read *closed* as a passive and to invest it with ideological significance: a closed shop has been closed by somebody as a matter of deliberate action, and it might be relevant to ask who it was and why. The text *CLOSED* on the door of a shop activates no such enquiry. And we might then suggest (if we were of a suspicious cast of mind) that the term *closed shop* was used precisely to invoke an analogy with the stative use on the shop door so as to 'naturalize' the concept and deflect any possible implications of agency: a closed shop is just a shop which is closed. But a reading of *closed* as a passive with ideological import depends on our knowledge of the particular trade union practice the phrase denotes, and on our own political values. Of course, in saying these things I am making appeal to plausible pragmatic uptake based on the notion of 'normal' contextual connection. And again, one would need to see whether second person elicitation would provide corroboration of these readings. But the point is that

we need to take note of how these other perspectives come into play in interpretation. The significance is not there in the text.

And indeed this is what Stubbs himself discovers. He discusses work done by Gerbig (1993) subsequent to his own 'an identical analysis of ergative verbs on a different corpus' (Stubbs 1996: 145). Gerbig's corpus consists of texts about ozone depletion. Some of these originated from an industrial source, where we might predict that responsibility would be evaded by using the ergative to imply that ozone depletion 'just happens' and other texts originated from environmental groups, where one might expect the use of explicit causative expressions to figure more prominently. But it turns out that 'contrary to expectation' it is actually the environmental groups that exhibit more ergativity in their texts, so its occurrence did not in this case provide corroborative evidence of ideological stance. Why not? 'The explanations might lie in both what is taken for granted in the texts and in the different meanings expressed by ergativity' (Stubbs 1996: 145). It is to Stubbs's credit that he should keep his enquiry open in this way rather than foreclose on premature conclusions. But his comments here do not just indicate minor procedural flaws or incidental shortcomings which might be remedied the next time round. They seem to me to indicate major conceptual problems in the very nature of the analysis.

Of these explanations, the second is co-textual and relates to the points I have been making about the way grammatical features act upon each other. But, again, it is not a matter of identifying different meanings that are encoded by ergativity itself, as accounted for in a grammar, and then analysing them out of the data: it is a matter of recognizing the dynamic interplay of ergativity with other grammatical and lexical features that are 'co-selected' in the textual process.

The first explanation is contextual, and takes us beyond what a computer analysis can account for. All texts, as the trace of a discourse process, leave things unsaid which are assumed to be taken for granted, and so do not need to be said. But of course, things might also be left unsaid with the deliberate intention of keeping the second person in the dark. In the first case, the first person is being co-operative in Gricean fashion, and presumably no implicature as an ideological effect is to be inferred. But in the second case, the absence is indeed significant, and warrants critical enquiry. The difficulty is to know which is which, for the text itself is not reliable as evidence. You need to enquire into the discursive conditions of its production and reception. It should be noted in all fairness that Fairclough himself recognizes this and admits in the introduction of his own collection of papers that:

The principle that textual analysis should be combined with analysis of practices of production and consumption has not been adequately operationalized in the papers collected here (Fairclough 1995: 9).³

But, as I have argued elsewhere (Widdowson 1998a), this is a crucial principle. Without it you are not dealing with discourse at all but only with its

textual trace. Neglect of it, therefore, must invalidate any inferences of ideological intent based on textual evidence alone.

What it seems to me is exemplified in the kind of critical work I have referred to is linguistics applied. Texts are analysed in reference to grammatical and lexical categories without regard to the way they are discursively realized: the signification of linguistic forms is carried intact into text to be recovered as significance. The process whereby these forms interrelate co-textually with each other and contextually with the circumstances of their use is left largely unexplored. Text is treated as a kind of static semantic patchwork, existing as an object for analysis in its own right. But as I have argued earlier, texts only exist as such by the implication of discourse. The general (and obvious) point is that writers generally design texts on the assumption that the Gricean co-operative principle is in force: that people do not just consume texts unthinkingly, nor subject them to linguistic analysis, but process them in normal pragmatic ways, inferring meanings, which have not been explicitly spelled out, by reference to what they have already read and what they know of the world.

But this, the critical linguist might retort, is just the problem: those in power count on this co-operation to insinuate their hegemonic influence. It is precisely because people read texts normally that they get deceived so they have to be directed into reading them abnormally by withdrawing their co-operation. But if people are to make sense of texts, they have to make co-operative connection with some context or other. What critical linguists do in effect is to provide contexts of their own devising which then regulate the interpretation of textual features as appropriate. And an acceptance of this interpretation depends on the reader being co-operative on their terms. Thus one partial reading is replaced by another. It would seem more sensible to look at how different contextual and co-operative conditions give rise to alternative discourses derived from the same text.

To do this would be to take empirical ethnographic considerations into account and locate texts in their sociocultural settings. In such an ethnographic approach, how non-analysts go about their normal pragmatic business would be the central focus of study. Rather than discount their understanding as naive and ideologically uninformed, it would help them to an awareness of the contextual conditions that give rise to different discourse interpretations and of the essential indeterminacy of meaning. If critical discourse analysts were to explore discourses along these lines using their own partial interpretations as a stimulus for such enquiry rather than claiming a privileged status for them, their work would indeed be of considerable significance since it would be relevant to an understanding of, and intervention in, everyday uses and abuses of language and so fall within the scope of applied linguistics.

In both corpus and critical linguistics what we find, I would argue, is an analysis of text which is then given unwarranted discourse significance in disregard of crucial contextual factors. In reference again to Hymes, we might

say that corpus linguistics assumes that the appropriate can be derived from the attested, and critical linguistics assumes that the appropriate can be derived from the possible. Both claim that they reveal the reality of experienced language, hitherto inaccessible, and that this gives grounds for intervention in human affairs, pedagogical and sociopolitical. But neither of them does in fact engage with the reality of language as experienced by users and learners: indeed they distance themselves from it and produce an analytic construct which then effectively projects reality in its own image. In both cases we get linguistics applied.

So long as we recognize that this is what they are, and discount the claims for unmediated relevance, no harm is done. All enquiry is partial, and each partiality has things of interest to reveal. Both kinds of text description that I have been discussing are of enormous interest precisely because their findings are partial and conditional on a particular perspective. Such a perspective has its own validity but this is relative and not absolute, and it is of value because it provokes us to consider how it relates to others. From the applied linguistic point of view, their importance lies not in the answers they provide but in the questions they provoke: if the textual facts that are revealed by corpus analysis cannot directly determine the discourse that needs to be enacted in classrooms to engage learning, then how do we evaluate their relevance in the light of other, contextual, factors that we need to take into account? If they do not represent real language for the learner, then what *does*? Similarly, if you cannot directly assign ideological effects to specific lexical and grammatical elements in a text, then what *are* the textual constraints on interpretation that people should be aware of? What is it that pragmatically motivates the intra-textual and extra-textual modification whereby people derive diverse discourses from the same text? These are questions for applied linguistics to consider, and the value of the efforts of linguistics applied, as I have reviewed them here, is that they prompt us to pose them.

If applied linguistics is to have any occupation it must, to my mind, avoid and indeed resist, the deterministic practices of linguistics applied. Its only claim to existence as a field of enquiry must rest on its readiness to enquire critically into the relevance of linguistic theory and description to the reformulation of language problems in the practical domain. Such an enquiry has to be linguistically informed without being linguistically determined, for these problems are inextricably bound up with other conceptions of reality, embedded in different discourses which have their own legitimacy, and these we have to somehow come to terms with. The business of applied linguistics in this view is to mediate between linguistics and other discourses and identify where they might relevantly interrelate.

This means that legitimacy has to be accorded to the discipline of linguistics as a distinct discourse as well, for otherwise no mediation is possible. And as a discipline, it is bound to be an abstraction in some degree, its theories and descriptions at some remove from the reality of experienced language. This is not something to be deplored or corrected. On the contrary, the whole point

of having a discipline is that its concepts and procedures reveal aspects of language which are *not* evident from experience. That is why it provides a perspective worth considering: if it simply replicated everybody else's reality, it would have nothing whatever to contribute. As noted earlier, linguistics over recent years has extended its theoretical and descriptive scope to account for aspects of E-language. The discipline has thus extended the range of data which it seeks to deal with, but although this will necessarily involve the development of new concepts and procedures, these will still conform to some set of abstract principles which define the specialist discourse of linguistic enquiry and constitute the criteria for its validity. And this will still keep linguistics at a remove from the language of lived experience. Although the extension of scope is likely to enhance its potential applicability in that its broader coverage will make it easier to establish correspondence with other perspectives, this correspondence has still to be established.

The discourse of linguistics, then, provides a version of reality which in certain essentials is different from that of other discourses. And to recognize its difference is not to defer unthinkingly to it. It is, as I have argued here, a mistake to suppose that it is the only reality worth considering, and so impose it on others, as linguistics applied tends to do. But, as I have argued elsewhere (Widdowson 1998b), it would be equally mistaken to absorb it into other discourses and deny its distinctiveness. The developments in description that I have considered in this paper are of interest not because they comprehensively capture the reality of experienced language, but because they do not. Herein, I think, lies their value: they provoke critical questions which would not otherwise be raised as to what relevance their particular partiality might have for the reconsideration and reformulation of other perceptions of reality. Linguistics applied of the kind I have criticized in this paper thus poses a challenge to applied linguistics, and in this respect is an important influence. And taking up the challenge might even make us more certain about what we are supposed to be doing in applied linguistics (the journal and the enquiry), as future prospects beckon and we move into the new millennium . . . etc., etc.

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NOTES

- 1 The work referred to here is actually Widdowson 1991.
- 2 Typically: a necessary rider. For it has been pointed out to me (by both Malcolm Coulthard and Ruth Wodak) that not all scholars working in critical discourse analysis would subscribe to the approach that I am criticizing here, and that some would indeed

take ethnographic factors into account in their interpretation.

- 3 This principle would of course bring in just the ethnographic factors that some recent developments now take note of. But it is more honoured in the breach than in the observance in Fairclough's work.

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