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**Don't throw out the baby with the
bathwater:
in defence of theoretical eclecticism in
Stylistics**

by
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Don't throw out the baby with the bathwater: in defence of theoretical eclecticism in Stylistics

1. Introduction – the arguments

There have been a number of public arguments in recent years, about the relative merits and demerits of various approaches to textual analysis, including stylistics, CDA and relevance theory. At the risk of setting myself up as a perpetuator – and perpetrator – of similar arguments, I would suggest that there has sometimes been an unnecessary virulence in the tone of some of this discussion which is detrimental to progress in the discipline and actually impedes useful thought processes by upsetting people and causing unproductive personal animosity. In the original version of this paper I quoted an example of what I mean at this point. On reflection, the inevitable result of this quotation would have been to single out a writer and risk behaving in the way that I am condemning.

It is true that the commercialisation of academia confuses the picture and since people have careers to build, they are sometimes under pressure to espouse causes (or theories) that will be advantageous in the jobs market. Indeed, Toolan (1996) alludes to this process (albeit symbolically) in his discussion of the inadequacies, from the integrationalist point of view, of seeing language as code:

The corrected account threatens the job-creation schemes of all textual theorists (Toolan 1996:129)

This argument, it seems to me, could be turned on anyone who appears to be attacking others' work more as a defence of a particular 'corner' than as a genuine attempt to discover more about the phenomena under scrutiny. Whilst Western academic pursuits are based on an Aristotelian notion of adversarial argument, there is a fine line between posing alternatives as a method of refining knowledge, and building barricades and fortresses between the different viewpoints.

Before proceeding with my argument that eclecticism is both theoretically sound and more useful than the alternative, let me summarise briefly some of the arguments that have occurred recently in the vicinity of Stylistics:

- Keith Green (1997 and 1998) and Michael Toolan (1996 and 1998) have criticised Relevance Theory, particularly in the form of Pilkington, MacMahon and Clark (1997). Pilkington and Clark (1998) and MacMahon (1999) have defended it.
- Billig (1999) and Schegloff (1999) have debated the relative merits of Conversational Analysis (CA) and Critical Discourse Analysis (CDA).
- Henry Widdowson (1995, 1996 and 1998) and Michael Toolan (1997) criticised CDA for theoretical eclecticism and lack of prescriptivism respectively. They both also criticise its practice for a lack of methodological rigour and openness. Defence of the earlier criticism has been offered by Norman Fairclough (1996).
- Mackay (1996 and 1999) has rejected claims about the scientificness and objectivity that he sees as being made by the stylistics of the post-linguistics era. The defence in this case came from Short et al (1998) and Short and van Peer (1999).

Though it might be of some interest, I do not propose to review these arguments in any detail here. Rather, I wish to use them as examples and stimuli for some of the points I wish

to make. Not all of these debates – nor all of any single debate – is/are personal in tone, and there is much valuable discussion to be found within them. However, I would like to suggest that the rhetorical strategies (such as answering a different question to the one being asked) to be found within some of them to some extent undermine their usefulness.

Billig and Schegloff, for example, both write sensibly and convincingly on their chosen methods of linguistic analysis (CDA and CA respectively), but there is a sense, in reading some of their articles, that they are (whether deliberately or not) missing each other's points. Schegloff does not cope at any point, it seems to me, with Billig's implication that no practice can be without 'foundational principles' – by which he means assumptions or premises. Instead, Schegloff simply argues that CA is more objective in its analysis than CDA, almost implying that it is founded on no unproven assumptions at all. Billig, by contrast, though he acknowledges that CDA lacks some of the rigour and objectivity of method that is one of the strengths of CA, spends his time attacking Schegloff when he could have been looking for points of possible mutual advantage:

The number of times that many of the writers in the various debates cited above feel the need to claim that they have been misrepresented is an indication of all not being well in the communicative processes that are going on. Here are just two examples:

But to pluck my sentence from its context, which is a simple set of suggestions about how students can use collocation and lexical patterning in order to begin to make sense of a poem, and treat it as articulating my theory of stylistics and poetic interpretation is unwarranted. (Toolan 1998:69)

Green's article dramatically misrepresents both relevance theory in general and the articles in the special issue in particular. His unsubstantiated assertions are not merely off-target; they are aimed at a target which does not exist. (Pilkington et al 1997: 139)

Whilst it is tempting to start analysing the texts themselves, to expose the rhetorical tropes and more importantly the miscommunications that seem to arise, I would prefer to take a constructive approach and simply suggest that some of this disagreement is simply unnecessary, and anyway untheoretical. In short, then, though the adversarial style of both politics and intellectual debate has a long and respectable history, its practice sometimes obscures the theoretical basis of academic investigation, one which I believe sanctions eclecticism, and ought to welcome multi-theoretical viewpoints as illuminating the undeniably complex data we're trying to elucidate – namely human communication.

2. 'Scientificness' in principle and in practice

When I was an undergraduate in linguistics we were taught both structuralist and functional methods of describing language as well as Transformational-Generative grammar. We were also encouraged to see synchronic linguistics (which was what we were largely doing) as different to (rather than necessarily better than) diachronic linguistics, which had been the main tradition in linguistics until the early 20th century. Having been taught the basics of scientific method and modelling in the first year, we were left in no doubt that all of these approaches were somehow necessarily partial as well as being insightful; that synchronic descriptions were by definition out-of-date as well as lacking the historical dimension.

I find it odd, then, over 25 years later, that people argue with great vehemence against models that in my view and memory never claimed to be true in any absolute sense. Toolan (1996), for example, goes to some lengths to deny the existence of literal meaning:

although a conceptualisation of literal meaning as the basic, determinate, and context-free meaning of words and sentences is necessary for standard linguistic treatments of the semantics and pragmatics of a language, in practice no such domain of context-free meaning exists. (Toolan 1996:125)

A similar problem afflicts the claim that linguistics is a science. It is clear that all scientific models are relative and are not susceptible to absolute proof. Short et al (1998) point out that no scientist – and no stylistician – would claim that the word ‘objective’ means ‘true for all time’. They add:

The point about empirical work is that it is open for all to see and find fault with: understanding is always provisional, and can in principle be revised and improved. (Short et al 1998:42)

The image being presented here is of a continual search for better and better models, as the old ones are challenged by new empirical testing of the old. To some extent this is an accurate picture, though I would argue that even in the ‘hard’ sciences, and certainly in social and human sciences, theories are not always completely discredited, even when the next generation of models and theories seems to have replaced them. This is because the particular strength of these theories is not always improved upon by the next generation of theories. It is usually the flaws that are targeted for improvement.

A simple sketch of what happens in practice is that a refinement (or a replacement) is proposed which answers some of the problems which emerged after the application of a previous theory to a range of data. Often this refinement will add to, rather than replace, the earlier theory, but even where replacement takes place, the earlier theory may retain insights and uses that are not invalidated by later work. One non-linguistic example of this is Newtonian physics, which was superseded by Einstein’s theory of relativity. The Newtonian model still works for calculations within a certain range of space and speed, and to that extent is still used for practical purposes like aeroplane flight and even getting rockets to the moon. What Einstein did, however, was create the model needed for calculations of speed nearer to the speed of light and useful, therefore, for longer journeys into space.

So, while theories might turn out to be wrong in some details, or later improved upon by different theories or refinements, it is rare for any serious theory of the data to be wrong in all respects, or to supply no insight whatsoever into the problem under investigation. At the risk of acquiring a reputation for weirdness, I would even suggest that we treat the Earth as flat for some practical purposes, like map reading. The fact this model is in some sense ‘wrong’ does not mean that the ‘Earth-as-sphere’ model has triumphed absolutely and for all time. Neither are such earlier and superseded models always only useful in some trivial sense that undermines them as theories.

Another basic insight that I remember from my early brush with scientific modelling is that no useful theoretical model can be as complex as the data that is being described. One of the strengths of theoretical models is precisely their aim of simplifying the complexity of the mass of data. What worries me about the rejection of some linguistic models we have seen recently is that they are not replaced by another equally simple model, or even by a more complex but more explanatory model. Instead they are replaced by models whose complexity equals that of the data and whose explanatory value is therefore very limited.

One of the criticisms of code-based models of language that Toolan (1996) puts forward, first of all describes the position of Lyons, who argues that

despite their undoubted importance, a full account of these contextual features is impossible in practice, “and perhaps also in principle,” and [he]

notes that such considerations cast doubt on the possibility of ever being able to construct a complete theory of the meaning of utterances. (Toolan 1996:6)

This seems to me a good summary of the position I am taking, and which I would claim is inevitable, that if we try in any one model to take into account the full complexity of the communicative situation, we will fail to adequately explain anything. This does not mean that models should not focus on different aspects of the context as well as the text, as indeed they do, but that it is often counter-productive, and anyway theoretically nonsensical to aim for a fully integrated or comprehensive theory.

The final aspect of scientific method that I wish to discuss is the importance of basic premises. It is an unavoidable fact that all scientific reasoning is based upon some assumptions that have been accepted rather than proven. Whilst different researchers may decide to adopt different premises, and anyone's assumptions may be questioned and/or replaced, no theory can keep returning to basic principles without risking the whole enterprise. One might also expect a gradual build-up of the assumption-base of any field or theory, and though any individual assumption may be queried, on the whole these will be taken for granted.

The Billig/Schegloff exchange is characterised by a problem over basic principles. Billig accuses Schegloff of making claims to objectivity that are untenable, and of failing to notice, therefore that his assumptions will reflect his ideologies:

So ideology might stalk the unnoticed and the taken-for-granted assumptions of intellectual inquiry, especially the sort of social inquiry that overtly claims to be ideology-free and merely empirical. (Billig 1999a:549)

Schegloff, on the other hand, accuses CDA of being partial or biased, as though there were a possibility of entirely neutral and ideologically unfettered research:

I suspect that Billig and many who share his position believe that students of the social world know basically how things work, whereas I and many colleagues who work along conversation-analytic lines believe that basically we do not, and that we need to win that knowledge bit by bit from the social world we try to understand by examining it bit by bit. (Schegloff 1999a: 567)

Whilst Billig may be closer to my position in this section, there is nevertheless a force in what Schegloff says here. No conscientious researcher wants to be accused of bias, even those who accept that all research is grounded in ideology. There has to be some common position that can be taken up by which the basic premises of a model can be acknowledged. Whilst we may agree that a fault of early work in CDA was to insufficiently acknowledge the principles it is based on, this is very far from saying that the whole enterprise is flawed. What Billig and Schegloff seem to be doing is missing each other's point. Billig does not acknowledge the superior efforts at objectivity made by CA and Schegloff fails to accept that CDA, like all scientific endeavour, can operate (relatively) objectively if its premises are made plain.

What CDA does is take some of its premises about the nature of unequal power relations, in Western society in particular, from social and political theory. By contrast, Schegloff's claims to objectivity do not need to (indeed they cannot) hinge on a complete lack of foundational assumptions. Billig attempts to map out the assumptions of CA – including the claim that it relies on notions of the equality of the speakers – but Schegloff denies any assumptions of this kind. This is a pity because a more fruitful discussion between the two positions would have been possible if they were able to argue for the credibility of their respective - and potentially conflicting basic assumptions.

3. The issues that divide us

Methodology is one of the sources of the divisions between researchers into language in use and discourse. In general terms we can choose among things like introspection and example-building, corpus-based studies, experimentation, informant-testing, intervention and ‘simple’ analysis. I cannot see a good reason for attacking another linguist’s work simply on the basis that s/he has chosen one of these methods over another, although this seems to be the basis of Green’s (1997) attack on the relevance theorists. I would argue for methodological eclecticism on the same grounds that I argue for theoretical eclecticism. As with models themselves (and methods are often, but not necessarily tied to models), there are different insights to be gained by using different methods. One interesting path that research could take, it seems to me, is to take a method originating from one model and apply it to the problems of another model. For example, I can see aspects of CA that would benefit from the insights of the social dimension of CDA, and probably there would also be advantages of cross-fertilisation in the opposite direction, though I am less aware of them. To some extent this is precisely what happens in interdisciplinary work, though more locally a strong sibling rivalry means that sharing is often harder to achieve. Such methodological diversity, it seems to me, may lead to more evidence, more insight, though it is important, of course, to justify the use of any method in terms which explain why that method gets closer to the (relative) truth you are looking for. Beyond this we will never achieve the perfect method any more than we will build the ultimate theory or model.

Another issue that divides us is the approach taken to the centrality of the notion of ‘code’ in our models. There have been recent moves to ditch the inconveniences of code-based models, such as the inflexibility, the apparent context-free meanings, the privileging of author intention. Unfortunately, it seems that in ditching the bathwater, we may also be in danger of losing the baby. For me, the baby is that huge body of descriptive and theoretical material that has amassed since the rise of modern linguistics in the early 20th century. That is too great a loss, I would argue, with too little gain.

I will argue shortly that there is a defensible position that can and must bring code and inferencing models of language together¹. First, we should consider what has brought about this apparently radical change in linguistic modelling. Toolan (1996) explains the integrationalist viewpoint, and the reasons behind their rejection of code and thereby of linguistic description:

a central linguistic tenet, which is the invariable uniqueness of every speech event and the impossibility of repetition as a first-order *realia* of experience with language. (Toolan 1996:42)

This may be a statement with which it is easy to concur, though it seems to me that it precludes linguistic description of any kind, since the model of this linguistic ‘reality’ can only ever be as complex as the data, and for the reasons explained earlier, this would have little or no explanatory value. As it turns out, of course, Toolan does also acknowledge that speakers must operate with some notion of a code-like language:

We have every reason to believe that these relatively “uninterpreted” interactional memories are subsequently sorted, matched, backgrounded and virtually forgotten or foregrounded into mental models, schema, scripts, notions of genre and grammaticality and contextual appropriateness etc., etc., with the passing of time and the development of interactional (including linguistic) proficiency. (Toolan 1996:46)

This sounds less like a wholesale rejection of earlier linguistic achievements than the previous quotation, and indicates the necessary breakdown of what is experienced as a unified event into some kinds of component parts if we are to understand them any better. As MacMahon (1999) says:

Toolan's assertion that frequency of use influences word meaning does not contradict the code model of language at all; it simply suggests a non-controversial way in which the code might be established and undergo change. (MacMahon 1999:51)

O'Halloran (1997) points out that the strong version of the Whorfian hypothesis was never intended by Whorf himself, but that the original theory indicated a *tendency* toward reality-construction, rather than complete hegemony. Nevertheless, the move away from textual and/or authored meaning has resulted in a general suspicion of the idea of encoded meanings which provide an Orwellian kind of mind-control. There has been a very large backlash against the more extreme ideas of socialism and some of this sense of anti-conspiracy seems to emerge through Widdowson's and Toolan's critiques of CDAⁱⁱ. And yet, just as it is difficult to give up the basic tenet of feminism that all other things being equal men tend to have more power than women, it is hard to give up the insight that language (and other cultural systems of communication) at least partly determine the reality we live in. Thus we can hold political views at odds with society and find it hard to live by them. Similarly we can provide evidence of people actively reading against the covert hegemony of texts whilst still accepting that the hegemonic pressure exists.

It is difficult to avoid using political parallels when discussing such themes, not least because political debates figure in the arguments surrounding CDA and what Toolan calls 'traditional bi-planar linguistics'. One of the more useful analogies we could use here is that of the floating voter. In Britain the tag 'floating voter' as applied to people who do not consistently vote for a particular party is derogatory and has connotations of indecisiveness and lack of political knowledge or commitment. People interviewed on the street who are changing their voting habits often preface their comments by saying 'I've always voted X' as a way of demonstrating that this behaviour has been forced upon them and they are not the inevitable 'floating voter'. I would like to claim to be a floating voter as regards linguistic theory. But I would like to reclaim the term as a positive rather than a negative one. Often I do not want to 'buy' the whole package offered by a theory, but will search it for useful insights and interesting leads. There will be times, because of my fundamental beliefs (which will influence the premises I adopt) and my local motivation (for doing that particular piece of research) when I choose to vote one way and use more of one theory than another. This does not close off the other options, or lead me to spend energy discrediting them.

The other political analogy I will use here is that of the popular opposition between negative stereotyped views of socialism totalitarian, denying of individual rights etc.) on the one hand and of right-wing libertarianism (selfish bordering on anarchy, right to bear arms, low taxation leading to choice etc.) on the other. It is easy to read some early CDA and be reminded of Orwellian arguments about the state controlling language and thought. The evidence that it doesn't – and can't – at least in any extreme way, is well-rehearsed. However, I find it hard to accept the other extreme – that a truly anarchic model of language can work – when I find Toolan arguing for infinitely variable meaning and Widdowson attacking Fairclough for admitting 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):

This admission would seem to invalidate the whole critical operation.
(Widdowson 1998:143)

That it is difficult to include the contextual features in analysis is not in dispute. What is at issue is whether there is any mileage at all in textual analysis per se, or whether Widdowson is right to claim that the whole project is thereby invalidated.

Some may deplore what they see as the unreconstructed and irredeemable conservatism of the integrational position, notwithstanding the hostility to rules, innatism, and homogeneity in the foregoing arguments. In particular, a cult of individualism may be inferred, and a reluctance against assuming collective control or social determination of language.

Similarly, one could stereotype Billig as an unreconstructed socialist and Schegloff as a dyed-in-the-wool individualist. The trouble with such stereotyping is that it gets us no further with the models we are trying to build. I'm personally not very keen on the kind of so-called 'consensual' politics that Tony Blair's government claims to be trying to create in Britain, with his notion of the 'Third Way', but in a way that has analogies with that project, I do see that we need to model communication as a cross between a collective and an individualised enterprise. The fact that this is difficult should not deter us.

4 Why we need flexible systems – and why that is not a contradiction in terms

Let us return to the question of whether we should model language as based on some kind of a code. This will be the main test case for the idea that theoretical eclecticism will allow for more progress to be made in our joint project than theoretical entrenchment.

It is easy to demonstrate that a fixed does not work in all cases. Transformational Generative Grammar went through years of agonising about the exact mechanics needed in the grammar to produce 'all and only'ⁱⁱⁱ the sentences of English. The exceptions always did – and always will – spoil the rule. But what of the opposite extreme? That all meaning is simply inferential and that no code exists at all? Surely this is also unacceptable? First, I have already touched upon the point that the model would be too cumbersome; that it would be as complex as the data and therefore of no explanatory value. But in addition, even the fiercest critic of the code acknowledges that it is likely that speakers operate with some kind of code-like organisation of prior knowledge, as we have seen in the earlier quotation from Toolan (1996). Whilst Toolan is right to argue against literal meaning as foundational, I'm not certain that he needs to go as far as rejecting code altogether. If we develop a more flexible notion of code, both 'literal' and non-literal meanings are likely to be learnt in the same way, by an accumulation of experience.

It seems likely, then, that an amalgam of some sort is needed. And the reason this is not a contradiction is that with the exception of maths and logic (and, I have been told, even them) no system in the world is watertight in the sense that there can be no change, no overlapping categories etc. It is rather like the argument for objectivity; you must aim for it, knowing it cannot be achieved. And hard science is in the same boat as the human sciences when it comes to both objectivity and fixed systems.

Of course, one of the reasons for going down the route of holding onto code, but adapting it to account for context is that it produces the best explanations of language that we have. So far, the non-code version of a linguistic model has not explained anything much extra, and appears to want to abandon what we already 'knew'. As I claimed earlier in relation to Newtonian physics, it is not always necessary to use the latest model, particularly if it is not as well worked out as an older one, however flawed. For example, there may be advantages to using a phonemic model to describe the sound-patterns of poetry which may not need the extra apparatus of generative phonology as explanation. That does not mean that generative

phonology is not valuable, of course, but no phonologist, I think, would argue that early structuralist phonemics was anything other than foundational for their own discipline. And whilst there might be finer distinctions developed between the different places and manners of articulation, and a more dynamic model of their production too, it would be hard to imagine the insights of those descriptive dimensions disappearing altogether.

There are other things that benefit from code-like explanations – and ironically, these are very often the precise places where I would want to argue that the reader's knowledge of the code was likely to be played around with by the text/author, to produce new, creative, aesthetic meanings. I have argued, not very originally, that much of the creative practice in poetic language was derived analogically from existing aspects of language practice:

As well as being more colloquial, the language of twentieth-century poetry has also been more daring in its stretching of the rules of English, whether rules of grammar, semantics or text structure. This experimentation, however, has only in a few cases been different in kind to the type of experimentation which goes on in ordinary conversational contexts. In other words, poets have usually chosen to create meanings in ways that seem natural to speakers of the language. (Jeffries 1993:5)

What is meant by this is, for example, that words which are 'normally' unaffected by a particular rule or process (such as passivisation or addition of a negative affix) might be creatively drawn into such a group of words on a temporary basis. On a practical basis, why would we try to describe what is going on in a text without using category names like 'verb' and 'noun', even though there are examples that challenge the boundaries between them? It makes sense to me to use categories as idealised reference points, much like Daniel Jones used the cardinal vowels, in order to provide some basis of comparison between different usages of words and structures. Toolan seems to challenge this point – and provide a further potential subtitle for this article – when he says:

In both cases, it looks as if an attempt is being made to have one's cake (category membership is not a matter of category-wide sharing of specific properties) and eat it too (categories are well defined and structured relative to a core or central exemplar that bears exemplary attributes). (Toolan 1996:48)

This is an expression (having your cake and eating it) that I confess to spending many hours puzzling over as a small child learning English. What else would you do with cake? I would argue that the model that draws partly on a code-like theory is intuitively satisfying, not because it gives linguists, educators and right-wing politicians the right to pedal notions of correctness, but because it explains a vital part of the success of language as a communicative process. If there were a strict one-to-one form-function relationship, all the richness of human language would be lost. Creativity, playfulness, political propaganda, lying, slippery meaning are all, it seems to me, apparently paradoxically dependent on the existence of code-like behaviour, albeit with the flexibility of shifting and overlapping form-function relationships.

If I have argued that analogy is an important part of the creativity of poetic language, I may have been unconsciously drawing on another early influence; an article by Quirk (1965) explaining off grammatical structures in terms of 'serial relationships'. Quirk was trying to deal with a problem for Transformational Grammar, which had decided amongst other things that premodified noun phrases such as 'the tall tree' were derived from underlying sentences such as 'the tree is tall'. It seems that some of these expressions, like 'a major factor', do not work in the same way, as Matthews (1967:142) points out, since they do not obviously derive from an underlying sentence 'the factor was major'. Quirk suggests that a solution which would retain the insights of TG and reflect the data more closely would be to see some words as entering the 'transformed' structure directly, on analogy with other examples. More

generally he argues that the system is really a much looser confederation of structures with a range of different networks of connections between them, some of which will look more like the transformations of Chomsky's model than others. It seems that Quirk's contribution is an early sign – missed by many, and until recently forgotten by me – that there is life in the old code yet.

5. Relative objectivity is desirable in stylistic and related endeavours

Whilst the struggle for objectivity is one that causes us a lot of trouble, I would argue that we should resist the temptation to dispense with it just yet. Whilst I have suggested that no science is completely without premises (fundamental assumptions) and no scientific data is amenable to absolute proof, it remains true that our discipline struggle more than physics, for example, to adhere to principles like 'transparency', 'rigour' and most of all 'replicability'. One of the reasons for this is the lack of an agreed methodology (or methodologies) which can be replicated by different researchers. This lack of an explicit methodology has been a particular weakness of CDA work up to now, which is not a necessary feature of the approach, but an outcome, perhaps of its genesis and its strong association with the media studies strand of discourse analysis.

As someone who came to language study as a refuge from the vagaries and unsubstantial claims of literary criticism, I am not keen to be driven back to that starting point. As long as researchers make explicit the means by which they have arrived at their conclusions, I see no reason why we should not work towards both methodological refinements and improved objectivity, whatever the approach, model or method that we adopt. As Short and van Peer (1999) say:

Stylisticians can try to be precise about what they are claiming, to make their analyses as detailed and precise as they can, to be systematic in what they do, not allowing themselves to sweep counter-evidence under the carpet, to avoid making un-understandable and unfalsifiable statements, to be as inclusive as they can when undertaking the stylistic analysis of a text by making sure that they look at all the aspects of language and pragmatic processing that are available to them, confront their hypotheses with the real responses of real readers etc. This is what we mean by trying to be objective. (Short and van Peer 1999:273)

6. Some examples of eclecticism in practice

As a final effort to convince the reader that what I have been arguing has practical benefits in textual analysis, I would like to present three rather different 'texts' or 'communicative events'. These demonstrate the danger of scientific 'rigidity', the virtue of the linguistic category in interpreting deviations from that category, and the resilience of textual meaning alongside deviant, reader-determined meaning respectively.

I have been in email contact with someone called Margaret Manus recently. She is starting up an e-journal on iconicity and is looking for contributors, referees etc. Since I am interested in poetic iconicity (amongst other things), I contacted her to express my interest. She circulated the discussion list with her own definition of phonological iconicity, which surprised me, and reminded me how I had been away from 'hard' linguistics!

It must be shown that all words which contain that phoneme have some element of meaning which words not containing that phoneme do not have.

What strikes me as unsatisfactory in this definition^{iv} is that she does not allow for the kind of stylistic iconicity that seems to me most common, and also most likely to be interesting. She is trying to demonstrate the non-arbitrary nature of language (a rather different way of having a go at de Saussure), by showing that some sounds are *always* connected with certain meaning. I have no objection to her attempting this unlikely feat, but should we not begin by searching for local iconicity that appears to occur in the texts we are researching, and only then build up to the more general - or even universal - claims?

In fact, I would go further and suggest that the larger claims are not only less likely to hold true, but will lose some of the interest that contextualised examples can provide. Take, for example, the article on 'The Right Bra' that I have been analysing as part of my research for a book on the female body (Jeffries, forthcoming). The use of the word 'girlies' to refer to those readers who have small breasts and 'gals' to those who have large breasts would be interesting as an iconic text even if it were the only example I ever found. It would be fascinating, though surprising too, if I discovered an absolute - or even statistical - correlation between these words in such magazines and the size of breasts. It would be difficult to establish a very regular correlation between the vowel sounds and size more generally through the English language, despite the existence of a diminutive morpheme. And though we may be able to provide physiological and acoustic reasons for such a correlation^v, it will be a long time before a universal can be safely established. In the meantime it seems a pity to lose the insights into the text, which anyway seems to me the more interesting end of the spectrum.

So far, then, this is an argument against having inappropriate expectations of our models or our methods. But the corollary is that I would not want to make the case for my assessment of the iconicity of these words if there were no other evidence at all for this analysis. Thus, one might bring examples from poetry (Dylan Thomas has some) where the closed vowels also appear to represent small dimensions and the open vowels larger ones. This kind of comparative evidence is at least a step away from the 'I saw this - don't you agree' approach.

A different example, which provides a justification for the use and importance of categories (and thus the 'code'), comes from CDA. The argument that grammatical nominalization is often a way of avoiding the naming of participants in an event or action, as well as a way of avoiding the transitivity choices involved in selecting a verb, is well known. Personally, I would want to expand the arguments associated with nominalization to include noun phrases in general, especially where there would have been a choice of a clause instead. This is not the place for that discussion, though the examples below include both derived nominals and noun phrases. Here, I wish to argue that using noun phrases allows the producer of the text to 'package' information or opinions in such a way that its existence is likely to be unquestioned - and unquestionable - by the receiver. This effectively means that the existential presuppositions that are brought into play by a nominalization would not necessarily be present if a clause structure had been chosen instead.

The CDA analyst would then argue that in certain cases these presuppositions were significant to the meaning or ideology of the text, rather than trivial presuppositions about the existence of things that almost anyone would agree on. It seems to me that this argument, if you accept it at any level, is based on a fundamental assumption - a premise if you like - that the category 'noun' is prototypically a category of 'thingness'; that it confers the status of existence, even where the nominal or noun phrase actually refers to an action, event or property.

Some examples of this phenomenon can be cited from one of the US military websites, produced by USSPACECOM (United States Space Command), which combines the command structures of the army, navy and airforce in planning the US use of space for

defence purposes. The project, known as National Missile Defence (NMD or ‘Son of Star Wars’) involves among other things a so-called ‘umbrella’ provided by space-based nuclear weapons. The details of this plan are set out in a number of documents, including one called *Vision 2020*, where we find the following kinds of statement:

- Rapid integration of information enabled by space capabilities will be the key to successful operations.
- The precision and lethality of future weapons will lead to increased massing of effects rather than massing of forces.

Here we have two relational sentences, with identity (A is the same as B) and circumstance (A will lead us to B) as their basis. The main body of the sentences is contained in the grammatical Subject noun phrases and the other noun phrases that follow the verb. In each case, the existence of the referent is presupposed:

- Rapid integration of information enabled by space capabilities
- the key to successful operations

- The precision and lethality of future weapons
- increased massing of effects rather than massing of forces

In the first pair of noun phrases, the reader may feel led to accept that *the rapid integration of space capabilities* is a fact and that *successful operations* will happen. That neither of these things is inevitable is not within the scope of the sentence to question. The only proposition – and thus the only easy question that can be asked – is whether the first is actually the key to the second. I would argue that this text asks readers to accept that the US is intent on using space for military communication on a larger scale, and that there will be future wars (i.e. successful operations). Only the relationship between these is up for discussion.

The second pair of noun phrases takes for granted two characteristics of *future weapons* (in themselves presupposed to exist) – their *precision* and their *lethality*. The evidence from the Gulf War that statistical precision does not always provide accuracy in the field is one of the issues that a critical reader might want to raise. This sentence does not invite such questions. The circumstance noun phrase *increased massing of effects rather than massing of forces* links two different military issues, fire power and ‘manpower’^{vi} in a relationship of opposition as though they were mutually exclusive. The image of a very few people in control of enormous and lethal weapons, without the unpredictability of chains of command and human error or failing may be supposed to have the intention of being comforting. At this point the cynical reader may become aware of the ‘ghost’ of some of the intended readers of this text – politicians and military commanders. How attractive it might seem to them to have wars with few casualties on their own side, the population of the US huddled safely under the umbrella of NMD whilst their protectors wreak havoc outside. The fact that these presupposed ‘things’ are no more than decisions made on our behalf by political leaders is underplayed by the structures chosen in this text.

It seems to me an intrinsic aspect of English that the discussion above depends equally on the existence of some kind of code, and on its flexibility. Examples can be found in stylistics more generally, as well as CDA. The examples we see time and again provide evidence not of infinite variability but of patterned flexibility. The noun ‘flexibility’ itself is a case in point; an adjective-like quality (the way English treats it) which takes on the ‘thingness’ of a noun in its derived form. Thus I would argue that the code (nominal categories reify their referents) and inferencing views of language (when events are treated as nominals, we draw inferences about their existence/reification) are not mutually exclusive, but are two sides of a rather complex coin.

More evidence of this multi-layering of code-like and inferential meaning comes from the simultaneous reading process that occurs in many communicative situations. I have already referred to the cynical reader of *Vision 2020* who can see exactly what the text is getting at (as privileged by the structures of the text) and at the same time rejects the ideology and presuppositions of the text. To generalise, readers often (perhaps always) read for the intended, i.e. authorial meaning. At the same time they may be irritated by, alienated from or surprised by the text. They may disagree, be confused, find it does not correspond to their experience or see things differently because of background. All of this does not seem to alter the fact that readers habitually identify (albeit sometimes 'wrongly') what they see as intended meaning, can often identify unintended, hidden and/or ideologically naturalised meaning and yet still retain their own unique reading as well.

Although much recent work in stylistics pays lip-service (or more) to the post-modern, multiple-meaning view of texts, there is little discussion of simultaneous, but identifiably different readings that I would argue are the norm for many reading experiences. Thus I read women's magazines for my current research on the female body from at least three different angles at the same time. I am the critical reader, irritated by the cultural pressure on women to look thin. I am the linguist, looking for examples of stylistic practice. At the same time I cannot avoid being the middle-aged, overweight woman taking mental notes of how to lose weight, dress to look thinner, keep healthy etc. The latter would be my 'ordinary' assessment of the intended meaning, and as such is clearly identifiable amongst all the other 'me's' that are coming up with other readings. An impressionistic example of how such a group of meanings might be integrated as I read is as follows (with the thought processes in brackets):

She knew she needed help if she was going to continue losing weight. Otherwise it would be too easy to slip back into her bad habits (*critical reader: annoying construction of eating as 'bad'*) again. So, in January 1995 she joined a Rosemary Conley class at the local school (*Overweight 'me': I wonder if it really works – is there one locally?*). At first she was nervous and stood at the back of the class during the exercise session, but as the weeks went on she gained confidence and moved towards the front (*Linguist: notice the opposite construction going on here – back/front = nervous/confident*)

Whilst I may use at least two of these selves in writing about this text, it is clear to me that the linguist and the critical reader represent the two aspects of CDA that we need to take note of; the analytical and the interpretative respectively. Widdowson (1995:158) has attacked Fairclough for arguing that since objective analysis is impossible, all analysis *is* interpretation. This is a theoretical standpoint which is inviolable, but not very useful, it seems to me, and is similar to the argument used by Mackay (1996) against the relative objectivity of stylistics in that it undermines any analysis of anything. I hope to have already shown in this article that although all investigations are already based on choice of premises, data, approach etc., this is no more subjective than any science or social science. Those who study psychological differences between men and women are beginning from a gendered point of view, the field trials of Genetically Modified Organisms cannot be neutral tests, as claimed by the British Government, since their clear intention is to demonstrate the safety of their introduction to the countryside. These are not isolated examples, and though we may wish that politics hid behind scientific 'truths' less often, the fact remains that it is the distinction between analysis (the science is right within its own terms) and interpretation (why do we do it in the first place? what shall we do with the results?) that is being fudged by those who have an interest in their outcomes.

We can, and should, distinguish between aspects of our analysis which are more open to scrutiny and those which are less so. CDA, it seems to me, has shown us many interesting

things about the way that texts work, including the way that they apparently help to naturalise certain points of view and feed the hegemonic process. One of the arguments against the view that CDA has an in-built inevitable bias arises when you find yourself analysing a text from a very critical point of view and yet you are aware that the producer(s) would probably agree with your assessment. The writers of *Vision 2020*, for example, would probably not balk at my analysis of the noun phrases above. They may indeed want to take for granted the existence of rapid integration of information, successful operations, precision and lethality of future weapons and the increased massing of effects rather than massing of forces. It is our different responses to these naturalisations that is the 'critical' part of my analysis. The assessment of the way that nominalisation contributes to that naturalisation seems to me independent of its interpretation. Another direction that we may wish to take is to use the techniques of CDA for literary stylistics, where no critical (i.e. hostile) motive exists, but where the same kinds of analysis will form the basis of literary, rather than political, interpretation and in some cases may lead to a literary or aesthetic judgement of value. These issues are summed up by Simpson (1993) when he neatly dispenses with the accusation of Fish (and others) that 'interpretative positivism' is a fatal problem for stylistic analysis:

Where the problem of interpretative positivism arises is where a *direct* connection is made between the world-view expounded by a text and its linguistic structure. Amongst other things, this step will commit an analyst to the untenable hypothesis that a particular linguistic feature, irrespective of its context of use, will always generate a particular meaning. (...) The question really is how far one goes in the interpretation which accompanies linguistic analysis. (Simpson 1993:113)

7. Conclusions

Widdowson (1998) accuses CDA of a lack of rigour, and on the basis of Fowler's statement that 'critical linguists get a very high mileage out of a small selection of linguistic concepts such as transitivity and nominalisation' (Fowler 1996:8) he suggests that:

Analysis is not the systematic application of a theoretical model, but a rather less rigorous operation, in effect, a kind of *ad hoc* bricolage which takes from theory whatever concept comes usefully to hand. (Widdowson 1998: 137)

I would agree with Widdowson that some of the practice of both stylistics and CDA has reflected his view, and I am not arguing for any kind of return to what he calls 'a collection of expedient practices' (Widdowson 1998: 138) as opposed to proper theory – and model building. The intention here is rather to defend the principles of rigour and clarity that linguistics brought to textual analysis and that have been somewhat undermined first by assertions of the impossibility that language data can be subject to (relatively) objective analysis and secondly by the refusal to acknowledge the strengths of theories and models other than those of the researcher, for fear of appearing to lack commitment or seem weak.

Rather than write at yet more length on the morass of inter-related topics of this article, let me summarise my conclusions as a list:

- That objectivity is an aim we should have, and can honestly aim for.
- That code is a necessary part of linguistic practice and analysis.
- That theoretical eclecticism is theoretically unavoidable and theoretical absolutism is a theoretical nonsense as well as being unproductive. The merits and demerits of theories can, of course, be debated, but ultimately it

may be more honest not to encourage people to nail their colours to any particular mast but to sail flags of convenience for their purpose^{vii}.

- That the premises of any piece of research should be made plain, as should the distinction between analysis and interpretation, where the interpretation may legitimately be based on the foundational assumptions of the work.
- That methodological eclecticism is advantageous across the board, though not always within a single piece of research. What is needed in general terms is clarity of method.
- That the charge of interpretative positivism is one that we need no longer fear, given that we follow the advice of the above conclusions.

NOTES

Toolan (1996) accuses relevance theory of using a two-stage process to join code and inferencing. I will not argue for two stages.

ⁱⁱ It is worth noting, however, Toolan's rather surprising conspiracy theory about linguists' abuse of power in claiming the existence of literal meaning:

I argue on the contrary, that literal meaning is itself a highly contextualised notion, that it is a cultural and ideological construct very much designed to characterize some language practices as orderly, authorized, and authoritative (and others not so); it is therefore well suited to and reflective of societal interests in literacy, order and authority. (Toolan 1996:25)

ⁱⁱⁱ Although I remember Chomsky's basic requirement of a grammar as 'all and only', the only quotation I can find is: The grammar of L will thus be a device that generates all of the grammatical sequences of L and none of the ungrammatical ones. (Chomsky 1975:13)

^{iv} To be fair to her, after a flurry of responses, she was quick to suggest that a jointly developed definition might arise from the discussion list.

^v If the lower pitch and the relatively open mouth shape of vowels apparently symbolising large sizes can be established, there might be some rationale for proposing a universal here.

^{vi} Although the US is amongst the first in the world to allow women to fight as equals with men, I could find no politically correct alternative to manpower here, since the term 'people power' has altogether more positive and more peaceful connotations.

^{vii} I seem to be making a habit of reclaiming negatives as positives in this article. Here, the usually derogatory expression 'flag of convenience' is intended as an 'internationalist' view of the disadvantage of being too parochial – either in your politics, or in linguistic theory.

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