

Stylistics, fluency, and language teaching

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Asserting the relevance of *anything* to language teaching is invariably a temerarious task, especially if one is a linguist, and not (except, perhaps, occasionally and mercenarily) a language teacher. But to make such a claim for stylistics, in the general sense in which I shall be using the term, seems to provide an absolutely safe and unimpeachable line for a temerarious linguist to take. The basic arguments are simple (indeed, to the unsympathetic, so obvious that it is surprising to see authors dwelling on them at such length), and have been frequently made over the past few years (as in Halliday, McIntosh & Strevens, 1964; Davies, 1968; or, more recently, Halls, 1970), so much so that it is nowadays almost a truism — at least, in the context of conferences of the present kind — to say that language teaching, whether mother-tongue or foreign, needs to recognise the fact of language variation within language, and to cope with it in some way. I shall therefore take it as axiomatic in this paper that language teachers, for various reasons (which I shall refer to below), wish to be aware of the range of systematic, situationally-distinctive variation in language, wish to make their students aware of it (at least as a theoretical terminus ad quem), and thus require techniques which will identify, classify, and, ultimately, explain the linguistic basis of this variation. Linguistics has undoubtedly been the main factor which has influenced the development of this situation; hence it is only natural that teachers, or teacher-trainers, who feel in need of information on these topics, will turn to linguistics for further assistance. And the point of the present paper is to ask frankly whether they will get it — or rather (to make my point of view clear from the very outset) to examine some of the theoretical reasons underlying why, in the present state of the art, they will be unlikely to get it.

Before developing this point, a terminological note is perhaps necessary. Stylistics for me is the linguistic study of systematic, situationally-distinctive, intra-language variation. By 'situation' I am referring to that sub-set of non-linguistic variables which a (linguistically untrained) native speaker can intuitively identify as accounting for a particular selection of linguistic features

in a given (spoken or written) utterance.¹ 'Feature', in this definition, refers to *any* bit of speech or writing which may be singled out from language and discussed — a particular word, morpheme, sentence, structural relationship, etc. Now the above definition of stylistics means that my view of the subject is an extremely broad one — it subsumes both literary and dialectal use, for instance — and a word of explanation for this breadth of definition may be useful at this point. It seems to me that stylistics cannot be meaningfully restricted to the study of literary texts only, as the linguistic explication of such texts is theoretically dependent on the prior explication of non-literary variation (I am not of course suggesting this as a necessary *pedagogical* procedure). To recapitulate the argument I have used elsewhere (see Crystal & Davy, 1969 : 79,ff) : literature is in principle mimetic of the totality of human experience — by which I mean that there is no subject-matter or mode of linguistic expression which is a priori incapable of being introduced into a work which, by critical consensus, will be considered literary. But the phrase 'the totality of human experience' comprehends linguistic experience, as well as all else; and consequently we have to argue that the identity of literary expression is, in large part, definable only by relating it to the range of linguistic forms available in the community as a whole, which the writer has, consciously or otherwise, drawn upon. In *The Waste Land*, for instance, we find lines reflecting conversational, legal, religious, scientific, archaic, and other kinds of English, as well as bits of other languages. Clearly, in order to appreciate anything of the purpose of this combination of effects, we have first of all to recognise their presence in the text, and this in turn reduces to a question of the extent of our previous linguistic experience, and our conscious awareness of it. Another example would be the way in which stylisticians relate their observations about linguistic originality (or deviance) in literature to 'ordinary' language, in some sense (cf. Leech, 1969, and references there). As a result, I think it is essential to argue for a definition of stylistics which subsumes *all* systematic variation within a language accountable for by postulating that its occurrence is restricted (in some probabilistic sense) to norms of behaviour characterising social groups or (secondarily) individuals.

The question of what 'upper bound' to give the domain of a stylistic theory — in other words, how widely does the notion of 'social group' extend? — is not in my view answerable at the present time, and I do not propose to take up a position on this issue here. One might, for instance, decide on a fairly restricted definition, seeing stylistics as the study of the range of situationally conditioned *choices* available to native speakers, and of the varieties which sets of these choices constitute, thus excluding such variation as is studied under

¹ I thus wish to distinguish this from the more general sense of 'situation' used in some approaches to language teaching, where it is argued that new linguistic forms should always be presented with a distinct 'situational context', which makes the form meaningful to the learner (cf. Wilkins, 1971). Only a sub-set of situational constraints are referred to in my use of the term — namely, those which account for the selection of one rather than another of a set of well-formed linguistic alternatives. For further discussion, see Richterich & Marchl (1970). A detailed analysis of the variables referred to in my definition is to be found in Crystal & Davy, 1969.

the heading of dialectology (either regional, social, or historical) on the grounds that choice, in any meaningful sense, is rarely a relevant factor in the linguistic analysis of these situations. Dialect features are background features, in this view, unaltering and unalterable features of a person's sociolinguistic identity, *against* which stylistic features can be seen to operate. On the other hand, one might decide to play down the criterial status of choice as being too unverifiable and too speaker-orientated, and concentrate instead on a holistic approach to the analysis of an utterance's extra-linguistic information, seeing dialect features alongside other features of social status, occupation, and the like, in an integrated model, all contributing to a speaker's sociolinguistic 'profile'. Which of these approaches (or any other) is likely to produce good results is not demonstrable until such time as a vast amount more data has been accumulated illustrating the nature of the supposed situationally-conditioned linguistic distinctiveness. On this topic, we are, very much, thinking in the dark: we are trying to solve a theoretical issue without having any clear idea as to the nature or extent of the problem in the primary data which the theory is supposed to be accounting for. For historical and methodological reasons, three 'branches' of study have developed — stylistics, sociolinguistics, and dialectology (this list could of course be extended). But the existence of these branches does not mean to say that the data, when we have analysed it, will best be accounted for in terms of a model which recognises these distinctions. We shall have to see. And meanwhile, it seems useless to go into questions of boundary-definition: it is certainly a red herring to raise this issue as a problem when one is trying to relate stylistics to a field such as language teaching. Any stylistic model is inevitably going to be to some extent arbitrary at present; and practical considerations are going to be primary in any questions of evaluation.

What the previous paragraph amounts to is the assertion that in this field, as in so many others in contemporary linguistics, theorizing has gone far ahead of experimental evidence, and as a result, pseudo-procedures and pseudo-problems have multiplied. The problem is not simply that few experiments have been carried out; rather, there have been few hypotheses formulated in ways which are testable — and indeed, a concern to think in terms of rigorous hypothesis-testing at all is sadly lacking in the published literature. But sophisticated speculation, no matter how stimulating, is not science. And surely this is the point. If the linguist is supposed to be claiming that his approach to stylistic variation is valuable, *because of its scientific basis*, then he must live according to his beliefs, and work in a scientific way. I know lip-service is paid to many a scientific notion in this connection: we frequently hear talk of stylistics being, or needing to be, 'objective', 'systematic', and 'explicit' — I have said this myself on many occasions. Moreover, I believe that such scientificness is indeed possible, and will be beneficial in the way in which present-day linguists claim. But I have come to believe that we are fooling ourselves if we think that what passes for stylistics at the moment is scientific in any genuine sense. Because we have had a few successes in analysis, and a generally favourable reaction from the language-teaching world, this does not constitute a validation of any theory or method. There are many brilliant stylistic analysts who are not linguists, many teachers who did successful

coursework on registers before the word was ever invented — as well as many perceptive linguists who transcend the limitations of their own methodologies. We have to be *sure* that it is linguistic stylistics which is improving things; and at the moment I don't see how we can be, as precious little self-criticism and real experimentation have taken place.

Of the three criteria of scientific thinking mentioned above, I think that current stylistic practice would get good marks for systematicness, but would probably fail in objectivity and explicitness (a full discussion of these terms is to be found in Crystal, 1971a). Let me try to substantiate this point with reference to objectivity. Emphasis on the need for objectivity in stylistics is so general as not to require quotation. It arose largely as a direct reaction against the impressionism and use of unverifiable value judgement which characterised so much of the talk about (especially literary) style. Stylistic statements were to be descriptive, not evaluative; they were to be substantiatable by reference to quantitative reasoning; they were to be phrased using a terminology which would be generally applicable; and so on. Largely as a result of this, the role of the subjective in stylistic research came to be minimised, and it has often been ignored. This was an unfortunate development, in my view, as it has fostered a conception of stylistics as being more objective, and hence more scientific, than it really is. The reason for this is that there are at least three places in any stylistic analysis where reliance on qualitative criteria of some kind is unavoidable: in the selection of data for analysis, in the analyst's identification of contrasts, and in the assessment of overall stylistic effects. Ignoring the problems posed by these areas can have serious consequences for the subject, as we shall see. I shall look at each of these topics in turn.

The standard research strategy in stylistics is to take some texts (I use this term to refer to either spoken or written discourse) and examine them to see if diagnostic features can be identified. But where does the researcher get his texts from? What criteria is he bearing in mind when he decides which texts to select? If he decides to investigate, say, the language of science, then this assumes he has some kind of intuition that there are features of language which correlate in some predictable way with certain events in non-linguistic behaviour ('situation'), which are generally and cumulatively labelled as 'scientific'. But who provides the initial assessment of the situation which allows him to select some linguistic material as being representative of scientific behaviour? How does he know, in advance, that his texts are valid samples of data, *relevant* to his hypothesis? His own intuition cannot tell him, as clarifying his intuitions about his data is the whole point of the exercise. And he cannot just assume that his sample is valid. For what does 'valid' mean here? At the very least, it seems to me, it presupposes the notions 'successful', 'satisfactory', or 'accepted'. It would be of little value a stylistician taking as a sample text a book which scientists generally recognise as being badly written, unscholarly, ambiguous, and the like. The possibility of obtaining an inadequate sample has got to be eliminated, and this inevitably involves obtaining some kind of qualitative reaction from a native speaker of the language being studied (in this case, a scientist). But I am not aware of this having been done systemati-

cally, or being considered as a routine check in research strategy.

It is perhaps not so obviously a problem in the language of science, where criteria are often quite explicit (as in the *Handbook for Chemical Society Authors*), but consider the difficulties we are faced with in evaluating the basis of a sample for such hypothetical varieties as advertising, journalese, political speaking, or sermons. How do you assess, or even obtain information about, the 'success' of an ad? One would not want a research student to use as his primary data a set of advertisements which an agency had criticised as poor, or which the public had failed to react to in the desired way. It would follow, then, that for any research in this field to be valid, one would at the very beginning have to do some market research into market research — to understand what the advertiser is trying to do, how *he* evaluates his material, and its effect, and so on. But if the researcher does so, he immediately finds himself faced with a highly subjective, intuitive area, which he will have to assess in its own terms, before he can introduce any kind of 'objective' reasoning into the exercise. Now as far as I know, this kind of 'contextualisation' is not a routine part of stylistic investigation; and to the extent that one thereby ignores causative factors affecting the nature of one's data, and fails to control them, one's hypothesis thus becomes non-rigorous, and one's results uninterpretable.

The difficulty, of course, increases along with the diminishing 'concreteness' of the variety being investigated. Advertising is a fairly well-defined field, with fairly explicit techniques and well-understood purposes; the important variables are relatively easy to isolate and define. But if we take a sermon as our object of study, the techniques, purposes, significant variables, and so on, are much more difficult to pin down. I do not think it would be too difficult a task to work out a questionnaire in order to establish the 'success rating' of advertisements, but my mind boggles at the way in which a sermon might be comparably evaluated. Can one stop the congregation as it leaves, and ask? Or should one work behaviourally, and quantify the intensity of the silence during it (a pin-dropping measure, for instance)? These problems are real, and they become dominating in cases of literary analysis. For example, if a student wishes to do some work on Dylan Thomas's poems (as seems usual), then he will generally make a selection to begin with — and initially, obviously, he will have to start with a given one. But which? May his choice be random? I do not think it should be; nor, indeed, do I think it can be (but this is a side-issue). Whichever text is analysed first is inevitably going to establish certain pre-conceptions about the subsequent analysis, some of which may be quite misleading, as far as ending up with statements of typicality are concerned. A great deal of harm has already been done to Thomas (and to poetic analysis in general) by students who have investigated his language in the firm belief that most of it was going to involve stylistic effects like 'a grief ago'! Not only has the collocational issue been rather overdone, as a result, but other, equally important features of Thomas's style in phonology and syntax have been ignored. To minimise the possibility of making his sample atypical, then, a researcher should try to make some criteria for selection explicit; and my point is that this rationalisation is always going to be evaluative. Either he will rely on his own personal feelings towards the poetry, or (as I recommend my students

to do) he will rely on the impressions of the next best thing to native speakers of Thomas's poetry that exist, namely those literary critics who have made specialist studies of Thomas. It would be a rash stylistician who chose to work on Thomas using a text which was generally agreed by Thomas critics to be sub-standard. (He may of course decide to research into precisely that issue. Why is it a bad poem?, but this is a different matter).

The scientific course in such questions, it seems to me, is not to work at our analyses as if the problem did not exist, or to think it trivial, or perhaps to assume that its solution is someone else's province, but rather to face up to the necessity of devising techniques for coping with evaluative criteria and relating these to our own, more familiar, linguistic ones. And such techniques do not exist. Which means that here is a point of weakness in stylistic research strategy, that anyone wishing to make use of the strategy should be fully aware of.

Moving on now to the second place at which evaluative criteria are inevitably introduced into our stylistic investigation, we can establish a similar weakness. When we have actually chosen a text, and got it in front of us, then how do we go about establishing 'objectively' the relevant stylistic effects? Once again, the procedure which seems generally in use is quite un-objective. To begin with, there seems to be some reliance on an assumption that is regularly false — that stylistic effects in a text stand out clearly. This is certainly a feeling that many students have. It is probably our fault, a product of the general and natural tendency in published discussions on stylistics to make use of the clearest possible examples as illustrations of general categories. I am not of course denying the existence of some clear, unambiguous cases of stylistic effect, e.g. the 'thou knowest' kind of feature, which is predictably religious; but I am beginning to suspect that such effects are not in the majority, in a language. Once we have worked through the obvious varieties, like science, religion, law and so on, then we come to a vast no-man's-land of usage, where there are clear lines of situational demarcation, but few readily demonstrable stylistic markers. After Davies's (1968) references to the register of 'policemen's English', I have heard people talking about 'traffic-wardens' English', and worrying because they could not find clear distinguishing features, apart from subject-matter. (I am always intrigued as to how these people get their data!) It does not seem to have occurred to them that perhaps there are no distinguishing features to be found. Many stylisticians seem to have assumed that because language displays situationally-distinctive variation sometimes, therefore it always does, on any occasion when it is used. Now, as a working hypothesis, to focus attention and get some research moving, there is some point in this; but now that some examination of data *has* taken place, we must surely begin to realise that it may not always be so — or, if this is too strong, that it is not always going to be useful to say so. An exhaustive classification of a language into discrete varieties may well be a chimera, and attempts to produce one may one day be viewed as little more than the manifestation of

To clarify the argument at this point, let us eliminate from the discussion those cases of stylistic uncertainty just referred to, and concentrate on the apparently very clear instances of situationally-distinctive features. The question which now has to be asked is, How do we verify our intuitions about the status of these features? Before we commence the quantitative part of the exercise, how do we know what to count? Do we simply 'notice' a feature, and assume that our allocation of it to a particular category is valid because we are stylisticians? This is scientific arrogance. It is true that previous linguistic training and experience of stylistic analysis may give us a sharpened intuition about what to look out for, but if this is all that is going on, then our position is really no different from that of the skilled literary critic. Intuition is no substitute for explicit criteria in this matter. Moreover, there is the point that the more stylistic analysis we do, the worse at stylistic analysis we may tend to get. It is a commonplace that people who have worked on surveys of English usage, and the like, are often very bad at giving off-the-cuff opinions about usage, as their intuitions are too flexible. Being at the opposite end of the pole from traditional prescriptivism, they will accept as permissible English far more than the 'average educated native speaker' will. And the same goes for stylistics. My own error is not to miss something out altogether in analysing a text, but to read far more in than the text might reasonably bear. A similar point is often made about editors of literary texts. So, how do we determine the validity of our intuitions? This is the really interesting question, but it has not, as far as I know, been faced. I am aware of no acceptability test (cf. Quirk & Svartvik, 1966) for stylistic data, using stylistically-naive native speakers as judges; nor do I know of any analysis of the variability in stylisticians' reactions to data. I shall discuss both these points in turn. In effect, what I am asking for is a stylistic analysis of stylistic metalanguage.

Perhaps one reason for the lack of development of any validation procedures here is that the real complexity of the problem has not been appreciated. One aspect of this complexity, which is relevant for the discussion of both intuitions about stylistic features and intuitions about the typicality of texts in a given variety (see above), is due to the existence of linguistic stereotypes. A stereotype is an individual or group's conventionally held, oversimplified mental picture of some aspect of reality: it corresponds in some respects to the reality of an event, but exaggerates, distorts, or ignores others (see Crystal, 1971b, for the application of this notion to concepts in phonetics). For instance, if I tried to speak in legal English (as in a joke), then I would introduce certain features that I felt were characteristic of lawyers speaking or writing (e.g. 'notwithstanding', 'hereinbefore', 'the aforesaid gentleman'), and this would probably be enough to get my reference recognised as such, though it would

¹ The claim for exhaustiveness, in relation to language teaching, is made for example by Gorosch (1970:4, ff), one of whose objectives is 'complete typological inventory of language situations'. For the opposite viewpoint, see Wilkins, 1971: § 3.2, 'Even where we know the general purpose for which a learner is acquiring language (the macro-situation), it does not follow that this overall aim can be segmented into smaller situational units, each of which is in turn behaviourally defined.'

certainly at times be little more than a poor parody. (Cf. Quirk 1961, where there is some discussion of conventional representation of dialect pronunciations in orthography.) Or, to take a different example, one does not have to be a believer to appreciate something of the force of a satire using religious language: educated atheists are just as able to identify and assess the overall effect of at least some features of religious discourse as anyone else, even though these may not be the central ones, from a stylistician's point of view. A good example would be the use of archaic language, which is probably the number one feature of a stereotyped view of religious discourse, though such structures are nowadays almost totally absent from liturgical, biblical, etc. language. And, as a third instance of a stereotype, there is the view of business English as containing many formulae (of the type 'Further to yours of the 11th ult'), a kind of language which these days most businessmen and business manuals try to avoid.

What theoretical status have these stereotypes? Should they be given any recognition in our stylistic models? It seems to me that explicit recognition of the concept of stereotype is an essential step for stylistics to take. It is important because it accounts for the existence of two stylistic intuitions, or 'modes of knowing', on the part of the native speaker, which should not be confused (I exclude for the moment the complications introduced by the possession of a third intuition, in the case of a linguist). Situationally-distinctive features constituting a hypothetical variety may be recognised in either of two ways, depending on whether one is involved in the variety 'professionally', so to speak, or not. As a lawyer, I will have a view of legal language, an awareness of the reasons for the form it takes (e.g. why much of its written medium is punctuationless, why lexical formulae such as 'without let or hindrance' are used), which a legally naive native speaker will not have. But, as a legally naive speaker, as I have suggested, I will have *some* ideas about what goes on, even if this is only from films, television, novels, and the like. Is my stereotyped view of any relevance to the stylistician? I argued above that a stylistic analysis had to be as compatible as possible with the 'professional' mode of knowing (in discussing the selection of television advertising); thus, when Davy and I were writing the chapter on legal English in *Investigating English Style*, we took pains to read up on manuals of legal expression, and to have our text and our analysis commented upon by legal colleagues. But it does not follow that, because we considered analysis of the professional mode a priority, we should not wish to pay attention to the 'lay' mode. On the contrary, I have some sympathy for those who might argue that *the* important phenomenon for stylistics to account for is the intuition of the lay language user on these matters, and I certainly think it should be studied.

This issue reminds me in some respects of the question posed by theory of literature as to whether the valid meaning of a text is that which corresponds to the author's intention, or whether a variety of individual readers' interpretations are equally valid. And the arguments which are familiar in that debate apply here too, in particular the point that as we shall never achieve a *full* understanding of legal language without becoming a lawyer, therefore the notion of a complete stylistic analysis of the professional mode becomes irrele-

vant for most practical purposes. The important question, for, say, the teacher, is How much of this complete analysis will the student *need* to know? It is this question which a field which might one day be called 'applied stylistics' might profitably begin to investigate. Meanwhile, what contemporary stylistic theory has to do is consider precisely what status the data it is supposed to be accounting for has. I am often confused in reading articles on stylistics as to whether a piece of illustration represents the intuitions of the professional native speaker, the lay native speaker, or perhaps someone else. It is conceivable that if the concept of stereotype is accepted, it will do much to clarify ambiguities in analysis of this kind. It provides an intermediate theoretical position which on the one hand avoids the totally introspective approach to analysis (which stylistics developed largely in reaction against), and on the other hand avoids the too powerful constraint that all shared reactions to stylistic features ought to be identical with those specified by a complete, 'professional' stylistic analysis. However, I do not know how to begin investigating stereotypes: it is a complex psycholinguistic concept which will doubtless require fresh techniques of analysis, including some new thinking on validation studies. And until I know, I do not feel I can safely and confidently make recommendations about usage to enquirers, such as in the field of foreign language teaching.

Some kind of test which would establish the generalisability of my stylistic intuitions is very much needed, then, as a *routine* research tool. I am not concerned only about the cases where two stylisticians are in open disagreement, where such a test would clearly be useful. Such cases are not common, in my experience. Far more frequent, and more worrying, are the cases where two stylisticians do not know they are in disagreement, because they are using the same category labels for a stylistic effect, but giving them different senses. What do labels like 'legal', 'formal', 'upper-class' and so on actually mean? I do not know, but one thing I do know is that they do not mean the same things to all men. A critical analysis of descriptive labels which displayed considerable disparity behind a commonly used terminology has already been carried out in the field of intonation studies (see Crystal, 1969: Ch. 7); and a similar kind of divergence is emerging when one analyses the way in which native speakers apply stylistic labels to pieces of text. In a project investigating the use of the labels 'formal' and 'informal' in English, for instance, Chan (1969) has shown that there is considerable disagreement between native speakers and inconsistency within individuals as to how these labels should be used. What is formal for one person may be informal for another; and the more intermediate grades of formality one recognises, the worse the confusion gets. Such terms as these are by no means self-evident, and should be carefully watched. There may be no common-core of usage which accounts for our ability to polarise texts in terms of a single formality scale. After all, to say that a sermon is 'formal' is by no means the same as saying that an election speech is formal, as the latter has a greater possibility of becoming informal than the former.

This problem is not solely a terminological one, however. If we allow the distinction between competence and performance to be introduced into the argument at this point, then it would surely be claimed — at least by those

who recognise a more flexible kind of competence than Chomsky apparently does (e.g. Lyons, 1971) — that at some stage we have to investigate stylistic competence, in some sense. That is, we are not interested in investigating solely a lawyer's (say) reaction to a feature we propose to describe, but also his view as to how typical this feature is, either in his idiolect, or in the variety as a whole which he professionally uses. If we find in a text four adjectives before a noun, for instance, then what should our stylistic statement be? Presumably none of us would want to say 'In this kind of English, a distinctive feature is that four adjectives may be used before the noun', and stop there. Stylisticians do not in fact say this kind of thing very much. What they tend to say is 'In this kind of English there is complex premodification using adjectives', or 'There is the possibility of long sequences of adjectives being used'. Notions of length or complexity are of course only as meaningful as the amount of data which has been analysed comparatively. In the present state of stylistics, such notions *can* be used, it seems to me, because very little data has been analysed. In our book, for instance, we frequently make use of such notions, but we always try to make their application clear by referring any descriptive statements about length or complexity to the sample of conversational English which we chose as a norm (see Crystal & Davy, 1969 : 95), and we try to keep the comparative part of our analysis within the scope of the samples in the book. As more and more data gets analysed, though, this situation cannot continue, and theoretically valid measures of complexity, and the like, must be found if stylistic analyses are to continue to be meaningful and consistent. Meanwhile, I think it is important for us to recognise that the intuitive leap which we make between the statements 'Four adjectives may be used . . .' and 'Long sequences of adjectives may be used . . .' is completely unscientific without the basis of our judgement being made quite explicit.

The third place at which evaluative notions seem to be unavoidable in stylistic analysis is at the very end, in what we might refer to as the 'renewal of connexion' between our stylistician's persona and our persona as ordinary language user. Once we have satisfactorily (sic) established a set of stylistic features, and counted them, and drawn up a comparative account of their occurrence and distribution among the texts of our sample, then what? Is there always a non-arbitrary, objective way of deciding whether two texts (or sets of texts) can be considered samples of the same variety? In most stylistic research, the assumption has been that statistical techniques will be adequate to this task, and the illustrations of varieties generally given are usually of such distinct kinds of English that one might be forgiven for thinking that demarcation lines are invariably clear. In fact, statistical analysis rarely gives a clear answer, in my experience, and requires reference to qualitative criteria at a number of points (cf. Reed, 1949 : 235, ff.). There is, for instance, the decision that has to be made as to which statistical measures are likely to be the most appropriate to handle a problem — let alone the question of whether *any* normal statistical techniques are really appropriate for the kind of problems presented by language samples of this kind. A typical stylistic analysis of two texts will display varying degrees of identity and divergence throughout all levels and ranks of linguistic structure (perhaps I should say, 'in principle', as few stylistic

analyses ever approach comprehensiveness in this respect — but cf. Moerk, 1970). Using Halliday's terminology, we can readily imagine a situation where two texts are almost identical at sentence rank, less so at clause rank, very different at group rank, identical graphologically and lexically, slightly different semantically, and so on. A single statistical assessment of structural identity is meaningless in such cases, for obviously from a given statistic one would be able to say little about the underlying configuration of structure which gave rise to it. And this situation is typical. Thus, at some point in our study, we have to decide on the degree of abstraction at which a quantitative analysis might be usefully made (at what level of delicacy, so to say), and make some kind of statement about relative importance of variation at the different structural levels. Immediately, the question becomes one of evaluation, and the usual, largely subjective criteria of elegance, simplicity, and so on, are raised.

But even assuming that arbitrary decisions have been made on these counts, there remains the general question of assessing the 'amount' of statistical difference and similarity between samples of an assumed population. If we have collected ten samples of journalism, let us say, and wish to establish that this label is stylistically meaningful, then we have to establish that the differences between the samples are insignificant. Unfortunately, language being the way it is, the application of most statistical criteria, such as the χ^2 test, shows that most differences are significant, though some differences are vastly more significant than others (χ^2 results up in the hundreds are by no means uncommon, even for such 'stable' varieties as scientific English — see Thakur, 1968). Of the ten samples, for instance, two might be so different that this might justify a decision to sub-classify the label journalism — say, into 'popular' v. 'educated' press report; but the others might be spread between these two, in such a way that there is no clear boundary-line as to where these two subclasses of journalism part company. Unless then, we wish to argue that each sample is its own variety, we are forced to make some kind of intuitive grouping, on situational grounds: there may be no greater statistical difference between samples 5 and 6 as between 6 and 7, for instance, but we will choose one and not the other on intuitive grounds (that it produces the 'best' analysis), e.g. by convincing ourselves that the *Guardian* is educated whereas the *Telegraph* is not. But such an analysis is circular, and makes any descriptive stylistic statements vacuous. Without a much more refined statistical and data analysis, and a more sophisticated linguistic theoretical notion of evaluation procedures, I do not see how this circularity can be avoided. Meanwhile, the difficulties should at least be recognised.

So far I have been arguing that many of the assumptions underlying stylistic theory and method need to be made explicit and tested in some way; otherwise our stylistic analyses will become naïve simplifications, capable of being shot down by the first sharp teacher who reads our findings. There have, in short, been too many attempts to produce taxonomies of stylistic effect, with too little attention being paid to the criteria which should form the basis of the taxonomy (or indeed, to the more fundamental question of whether varieties should be studied taxonomically at all). As a result, theoretical terms tend to multiply redundantly or be used inconsistently. On their own, terms like

'register', 'tenor', 'field' or 'situation' seem innocuous enough; but when one tries to piece them together to make a complete theoretical picture, then one recognises the inherent weaknesses in many of the definitions. A term like 'register', because of its breadth of definition, is almost bound to produce confusion. Any situationally-distinctive use of language may be called a register, it seems, regardless of what the most important criteria of distinctiveness are. Newspaper headlines, church services, sports commentaries, popular songs, advertising, and football, inter alia are all referred to as registers in Halliday, McIntosh & Strevens, 1964 (pp. 88-9). The danger, of course, is that people new to this field will think that they will be saying something new by referring to these uses of language using the term register, and that because these uses can all be labelled in the same way, that they are therefore the same. But they are not the same: different situational variables are involved in each case. For example, 'sports commentary' conflates two distinct notions, that of 'sports reporting' and that of 'commentary form'; 'football' is vague, but presumably this is an occupational notion only; and 'church services' could mean many things — would it include 'sermons', for instance, or is this a separate variety? This last point is a characteristic problem raised by the present approach. What level of abstraction produces the optimum characterisation of a variety? Is there *a* variety (or register, or whatever) of 'advertising', or are there *many* distinct varieties of advertising (e.g. newspaper, television, public announcements), or are these best regarded as 'sub-varieties'? Is there, in turn, a sense in which advertising may be viewed more abstractly as a 'sub-variety' of, say, propaganda? Without very explicit criteria, there is no way of avoiding inconsistent judgements on different occasions, e.g. viewing the different categories of advertising as different varieties, but ignoring the quite comparable differences which mark the various categories of scientific language (e.g. reports, laws, definitions, experimental instructions). This difficulty gets worse the more languages one studies. So far, stylistics has been very Indo-European in its orientation. It is difficult to see how it will cope with some of the situational categories developed by anthropologists, for instance, to talk about the variations they have noticed (Crystal, 1971c). The 'choice' factor already referred to is an example of a criterion which seems much less relevant when one discusses bargaining dialogue between tribes, and the notion of restricted language (which on the whole receives little mention in stylistics) seems much more relevant. Difficulties of this kind will disappear only if we develop a thorough understanding of the basis and limitations of our terminology, and perhaps a comprehensive survey will not be long in forthcoming. It is certainly much-needed, for while I have heard it said that the terminological disagreement is a healthy sign of a developing subject, myself I prefer to see it, less optimistically, as an inevitable outcome of confused thinking.

I have spoken so critically about a branch of linguistics which is generally uncriticised, because I feel that one of the jobs a conference of the present kind can usefully do is make the people who formulate policies and write textbooks aware of the difficulties as well as the facilities which come from a contact with our subject. In this way, I trust that attention will be paid where it is needed. I am not myself too pessimistic, however, regarding the future

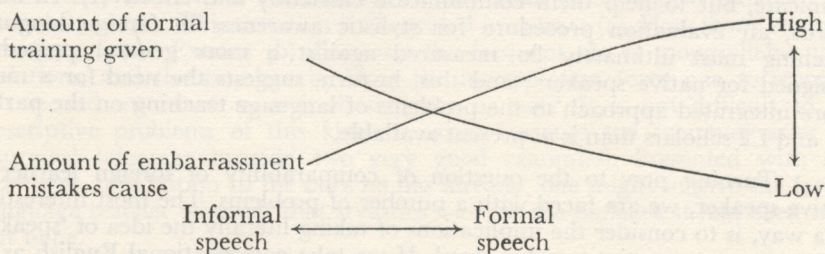
relationship of stylistics to foreign language teaching, if a careful analysis of the requirements of the language teacher and learner takes place, and over-ambitious projects (such as large-scale variety analysis of a foreign language, cf. Gorosch, 1970 : 6) are avoided. What we need, of course, is a job-analysis of our own job. What exactly are the foreign language teacher/learner's stylistic needs? Can they be categorised, and will these categories correspond to the theoretical constructs already recognised in the stylistics literature? I do not know. There seems to have been a fair amount of discussion about applying a given set of stylistic categories to foreign language teaching situations, on the assumption that they will be relevant, but little study of what happens when the reverse approach is made. Let us examine the implications of this a little.

There seem to be a number of reasons given arguing for the relevance of stylistics to foreign language teaching. Firstly, it is hoped that an awareness of stylistic variation will provide a rationale for selecting a particular variety to teach, and ensure that a single stylistic level is maintained consistently as the basis of a course. (In other words, stylistic awareness is *not* to be seen as solely the province of advanced language teachers, as has sometimes been suggested. Most stylistic effects, it is true, can be explained only by reference to the idea of choice between alternative constructions, which presupposes a certain minimum of structure to have been acquired by a student. But any selection of materials, even at introductory level, implies a stylistic selection, and this has to be made consistently, with the author as fully aware as possible of the consequences of his choices at all points.) Secondly, it is claimed that stylistic awareness will allow for a principled introduction and grading of categories of stylistic effect different from the variety which has been chosen as a norm, and thus promote a more systematic coverage of the 'resources' of the language than would otherwise be possible. Thirdly, stylistics brings with it methods for dealing with the analysis of any specific difficulties involving situational variability in speech or writing. In so doing, it will provide a terminology for describing stylistic effects, and a means of relating these to the 'common-core' features of the language. Fourthly, stylistics accumulates facts about usage not otherwise available; ideally, a comprehensive 'dictionary' of stylistic 'meanings'.

Now stylistic analysis, at least in principle, seems able to satisfy all these requirements: each requirement clearly relates to a task which theoretical stylistics has already recognised as important and meaningful, and research carried on within stylistics (if done properly) thus looks as if it will be relevant.

We can now ask the question: to what extent can stylistic notions be incorporated within foreign language teaching procedures, as these are generally viewed at the present time? This is a vast question, so I propose to restrict it by illustrating just two of the theoretical problems which arise when one tries to turn this relationship from theory into practice — one in connection with error analysis, and one for theory of testing. Taking the case of stylistic errors first, it is generally recognised, both in mother-tongue and foreign language teaching, that a stylistic error is in principle different from a linguistic error per se. A linguistic error refers to a usage which could not occur in *any* context of English use; a stylistic error refers to a usage which is inappropriate in the situation in which it occurred, but which could have occurred in some

other situation. But there is of course more to it than this. Depending on the degree of restrictedness of usage of the feature, so there will be a gradation in the likelihood and seriousness of stylistic errors. As I mentioned earlier when discussing formality, some errors are more serious than others, because some situations are less permissive than others. In conversational English, for example, if foreigners make mistakes, then from the stylistic point of view they are relatively unimportant, as conversation tolerates more 'noise' and is more flexible than other varieties of English. Mistakes are missed, ignored, or joked about. On formal occasions, however, where it is more important to 'make a good impression', stylistic mistakes are going to be more serious. Introducing a stylistic perspective into foreign language teaching thus brings with it a certain tension: on the one hand, under the influence of linguistics, language teaching has begun to recognise the centrality and distinctiveness of everyday conversational English, the more formal kind of English in the older textbooks being considered artificial; on the other hand, it is the more formal kinds of English which present the greatest problems as far as social acceptability is concerned. At the moment, the pendulum seems to be swinging well into the conversational end of any formality scale — which is alright, so long as the more formal varieties of spoken English do not thereby become ignored. If there was nothing better to do, one might spend some time developing a scale of linguistic embarrassment, which would reflect this state of affairs, e.g.



This may not in itself be a particularly serious pastime; but it does I think indicate the kind of issue raised when one tries to incorporate stylistic reasoning into one's general practice.

If we turn now to the relationship between stylistics and testing, we find a different kind of problem posed. It is, to begin with, difficult to see how stylistic awareness fits in with some of the evaluation procedures language teachers refer to. This point is clearly illustrated if I take the six criteria postulated for the evaluation of oral and written proficiency in modern languages at a Council of Europe conference on 'Continuous assessment in upper secondary education' (held at Sundsvall, Sweden, in July 1969). These criteria are: pronunciation and accent (sic), grammar and structure (sic), vocabulary and idiom, fluency, comprehension, and subject-matter. Stylistic awareness would seem to be a separate dimension, relevant to all these areas, but not easily subsumable under any one. Fluency would seem to be the nearest relevant category, but this is not particularly satisfactory. It is rated in the above procedure on a 7-point ascending scale, as follows: 'negligible', 'disjointed and hesitant', 'uneven',

variety, a more formal spoken variety, a fairly formal written style, and perhaps a professional style as well. Most foreign learners will never need to productively use legal, scientific, literary language, and so on. It will not, in a word, affect their fluency if they are never introduced to more than these three or four basic varieties. Now if this were so, a stylistician would indeed have difficulty arguing for the relevance of the whole of his subject to language teaching. But there is much more to it than this, as one can see if the notion of 'fluency', on which I am hanging this discussion, is broadened to take account of (for want of a better term) 'receptive' fluency. By this I mean native-speaker-like awareness of (or sensitivity to) the full range of vocal (or graphic) stylistic effects in the language of *others*. (There seems to be no single term which maintains a balance between productive and receptive fluency, though 'command' gets near to it. The traditional notion of 'comprehension' is too restricted for the purpose, usually referring solely to the awareness of cognitive content, as mediated by syntax and vocabulary.) In the field of receptive fluency, the foreigner is on very similar ground to the native speaker: in principle he might be exposed to precisely the same range of stylistic effects, and find himself faced with precisely the same problems of interpretation. And in this case, given a descriptive framework incorporating all the stylistic features of a language, it is not difficult to see ways of introducing these features to a foreign learner, and evaluating his progress. One might, for example, present utterances, systematically varying one situational component and displaying the corresponding variation in linguistic form — a procedure which is commonly used in mother-tongue teaching, and which is, in effect, a stylistic substitution drill. Its value and practicability, of course, depend on the adequacy of the descriptive framework used as a basis. It would have to involve at least the eleven variables outlined below, and there are probably others. Maximum receptive fluency would involve building up the foreigner's ability to understand the full range of meaning and nuance presented by each of the categories listed here (my inventory could of course be considerably extended). For a full discussion of each of the main variables, see Crystal & Davy, 1969: Ch.3.

1. *Individuality*, e.g. differences between male, female, child, homosexual speech or writing.
2. *Regional dialect*, e.g. American/British/Cockney English, foreigners' speech.
3. *Class dialect*, e.g. uneducated, upper class, public school English.
4. *Historical dialect*, e.g. archaic forms, old or young speech.
5. *Medium*, e.g. speech on the telephone, public address, handwriting, reading aloud, reading from notes.
6. *Participation*, e.g. monologue, dialogue, 'multilogue'.
7. *Province*, e.g. religious, legal, advertising.
8. *Status*, e.g. formal, informal, types of phatic communion.
9. *Modality*, e.g. commentary, telegrams, lecturing, letter-writing.
10. *Singularity*, e.g. literary identities, recognisable contemporaries (e.g. Queen, TV characters).
11. *Others*, e.g. baby-talk.

Each of these examples could appear in either of the two modes discussed above, 'real' or 'stereotyped', hence this would be an extra dimension to the classification. For example, apart from recognising and classifying cricket commentary, one has to note its stereotyped association with West country accents (a distinction that seems to be shared by agricultural discussants on the BBC and Long John Silver, amongst others). Or again, the framework has got to allow for the stereotyped fact that clergymen, lawyers and undertakers speak monotonously. A further point is that each of the above categories has to be seen in different contexts of 'noise', reflecting as far as possible the actual constraints on receptive fluency affecting native speakers, where hesitations, interruptions and background noise in general presents itself in varying proportions and intensities.

Finally, in view of the popular use of the term 'fluency' in language teaching contexts, it is surprising that very little attempt has been made to determine exactly what is involved. How does one account for a reaction of 'fluent' or 'non-fluent' in the first place? How might one validate experimentally the categories of fluency referred to above? Well, one way might be to present a piece of language to judges, systematically varying certain features of it, and noting variations in terms of fluency (or some synonym). I have tried this informally, and on the basis of this it seems to me that a great deal more is involved in the notion than is generally recognised. It is not by any means reducible to a question of hesitancy, or the like. A small set of syntactic features are involved — in particular, the inter-sentence connecting devices (such as introductory adverbials). Omitting these causes severe disruption of fluency. But more important than this are the prosodic features of connected speech. Apart from the uncontrolled use of hesitation and tempo contrasts ('uncontrolled' is an important qualification here — controlled hesitation is highly effective in some speaking styles), these perhaps being obvious factors influencing judgements of fluency, there is the avoidance of pitch-range, loudness and rhythmicity variation, and the over-use of single intonation contours. It is surprising just how much common linguistic variation gets allowed in as factors affecting fluency judgements. 'Fluency' thus seems to be another one of those labels in need of evaluation. Is 'monotony' a feature of fluency? For some judges, it is; for others, it is not. Intonation, it seems, is of primary importance here. It is interesting, in this connection, that if we speak English as the intonation handbooks would apparently have us do, by producing sequences of tone-units in an additive kind of way, the result is by no means fluent. There seems to be no attempt to read into non-segmental phonology one of the most elementary principles of segmental phonology, namely, that when phonological units are juxtaposed, they modify each other. Tone-units modify each other, too, and form clusters — 'major' and 'minor' tone-units, for instance, as Trim pointed out years ago (1959) — and it is *these* combinations, or rather, a knowledge of their combinatorial properties, which seem to be the important thing in the analysis of fluent connected speech.

Problems, then, assail the stylistician from all sides. There are the theoretical problems which he has to resolve to put his own house in scientific order; and there the problems arising from the existence of a wide conceptual

and terminological gap between his academic motives and techniques and those of language teaching. Throughout this paper, I have insisted on the importance of much more data analysis than has so far been done, and on the need for the development of validation techniques for central assumptions. Only in these, rather unfashionable ways, it seems to me, will stylistics become the valuable tool of the language teacher that it is already being claimed to be.

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Commentary

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The analysis of the deficiencies of stylistics as a branch of the study of language which Crystal presents as the first part of his paper seems to me a brilliant formulation of just what the critics could justifiably say against the discipline as we know it. We are often told that when you have formulated a question sufficiently clearly you have answered it. My feeling here is that although Crystal has presented us with problems, he has done it so lucidly that it becomes possible to suggest how solutions worked out in related branches of study might be re-deployed to deal with the most substantial issues he raises.

I have only two things to say about the middle term of the title — *fluency*. First that its placing in central position seems to me a subtle device for indicating just how the wide-ranging first and third terms are to be taken — a device whose constraints Crystal has not wholly accepted; and secondly that I agree with everything he says on the subject.

Let me then turn to his analysis of the problem and procedures, real and pseudo, of stylistics. I don't think terminological issues are usually fruitful, but his statements on p. 34ff. seem to me to constitute a pseudo-terminological note, in the sense that they state a position on points of substance, not merely of terminology. I do not find it useful to use *stylistics* in a sense so broad that it subsumes all systematic variation within a language due to social group norms, and including, for instance, dialectal variation. I accept of course, that it is concerned with more than literary material, and that the notion of choice is probably relevant, but probably not criterial. If there is a case for a distinct branch of study called *stylistics*, the case, to my mind, rests on the distinction between texts (whether written or spoken) and systems. Stylistics, to me, is necessarily the analysis of language in use, a branch of performance study; whereas dialectology and various types of variety-study can operate as either competence or performance studies. Crystal's examples seem to show that he really takes a similar view; though he does apply the term *style* to *variety*. I am unable to follow in what sense the term is applicable. Again, it is true that we may not in the end be well-advised to operate with such distinctions

as those implied in the terms *stylistics*, *sociolinguistics*, *dialectology* (p. 36), but Crystal does not really mean us to sink as many distinctions as he says in the stylistic deluge. It is still useful for him to be able to speak of *The Waste Land* as including various 'kinds of English, as well as bits of other languages' (p. 35). The distinction he makes here, between what characterises the text and what composes it, could not strictly be made if he applied his definition of *stylistics* rigorously. The bits of other languages would have to be treated as features of an English style and we would still have no means of distinguishing them as styles from the character of the whole. I would begin, then, with a hypothesis, that stylistics is the study of the use of language in texts; at the primary level in individual texts, though we would expect secondary levels to develop, leading us to speak of the style of say T. S. Eliot or journalistic reporting, etc. As an initial hypothesis I consider this to be justified by the widespread tendency of people to feel that there are distinctive uses which can be talked about. I would not feel this hypothesis was subject to direct testing, but that it could stand provisionally until we know enough about a) what any given language is in itself b) what other kinds of systematic variation operate in any given language to be able to formulate precise questions about what is distinctive in particular texts. I would therefore consider this, on a long-term basis, to be a hypothesis scientific by Popperian standards.

Because Crystal has placed stylistics firmly in the field of language variation I can, despite our differences, now turn with some confidence to the standards of scientific rigour which he proposes, and particularly to the criteria of objectivity and explicitness, by which he rightly claims that stylistics fails. He attacks, first, sampling procedures (p. 37ff.). His hypothetical solution is that the linguist should 'face up to the necessity of devising techniques for coping with evaluative criteria, and relating these to our own, more familiar, linguistic ones' (p. 39). But, as he continues, such techniques do not exist, and the alternative, of analysing all data within a field, is rarely practicable. Now, although this is a real problem of stylistics as practised, I think it is in theory and fact a pseudo-problem. For its existence depends on the demonstrably false notion that we have identified the 'fields'. The second evaluative problem, that of deciding which are the relevant stylistic effects (p. 39), will, I believe, evaporate in a similar way, for it depends on the also demonstrably false notion that we know what 'the varieties' are, and only need to characterise them (though some will stand out less boldly than others). The same attack will have *something* to say about those problems of evaluation relating to the stereotype (p. 40ff.), and what in my field of work are sometimes distinguished as in-group versus out-group diagnostics (p. 41ff.). The final evaluative problem, of deciding whether two texts are samples of the same variety (p. 43ff.) will, I think, already have been disposed of, and we may have the glimmerings of a way into the statistical problems (p. 43ff.).

Crystal makes much, in his Introduction, of his temerity. Clearly I am sticking my neck out a good deal further in making such claims as I have. What I think is at issue is that Crystal has stepped back from the practice of stylistics to take a good hard distanced look at it by what it claims as its standards, and when you stand beside him at that distance you realise for the

first time that the problems are just like those of a wholly different field. In fact, it is my belief that other work can throw fresh light, just as it has in turn borrowed light from yet other studies.

In the *Tyneside Linguistic Survey* we ask the question: 'How can we determine the ecology of varieties of spoken English in an urban area?' We want to know what varieties are used, by whom, quantitatively and qualitatively speaking, and with what change through time in varieties and distributions. So what is a *variety*? We have, like the stylistician, a provisional hypothesis that there *are* varieties, on the grounds, that native speakers speak as if they think there are ('He sounds like a Geordie, speaks like a book, etc.'). But we are acutely conscious that the varieties traditionally recognised (RP, Modified Regional, etc.) are crude, intuitive, overtly and covertly confusing. So we ask: in disciplines which have a longer and more rigorous tradition of distinguishing varieties, how is it done? We turn, of course, to the biological sciences, and we find (as other linguists have done before) that the best attack is provided by numerical taxonomy. The essentials of this method are that the number of criteria should be as high as possible, and that none of the criteria should be dependent on any other. How many is a lot is something we don't know exactly, but it seems obvious that the greater the number of variables scored the greater the independence from intuitive judgement will be. In the *TLS* we use some 300, many of which are ratios which can be broken down into many sub-variables. To apply this sort of analysis stylistically seems to me to call for a large concerted effort, after which the individual analyses so far attempted will look very different. But that one can, given the resources and the motivation, start on a sound footing by this means seems to be unquestionable. The outcome could be a series of profiles in multi-dimensional space, clustering more or less exhaustively into varieties; if they did not cluster, the hypothesis of varieties would have been disproved.

Having got out our objectively identified varieties, we want to do things with them — particularly to see how they are distributed in terms of frequency (how normal they are) and in terms of what kinds of people speak them. The relevance of the first doesn't call for comment. The question of social distribution does, for it has often been assumed, (e.g. by Labov) that the social frame is a simple one which can be intuited, namely the single scale of socioeconomic class. Our intuition, supported by the evidence of a pilot study, was that for the English situation this would not do. So we were, on the point of the social dispersion of speakers, at the same starting-point as with varieties: we did not know how or what to classify on. And we resorted to the same technique of numerical taxonomy: score for all the criteria you can think of and let the classification emerge. So we are looking at varieties existing in one multidimensional space, characterising a human population dispersed in another multidimensional social space. The mappings involved, by the way, are new to mathematics and computation; or so my colleagues tell me. This is a great advantage to scholars in the humanities wanting to enlist the aid of their computing colleagues. We are not just seeking their services, but pointing the way to original research in their field.

It seems to me that exactly the same relationship exists between the characteristics of individual texts, the varieties postulated in stylistics and the distribution of them across multidimensional 'fields' — having the function of a commentary, and the subject sport, for instance (p. 45) — and that the stylistician's problem of defining relevant 'fields' is analogous to our problem of defining relevant social groups.

This leaves the problem of the stereotype. I would like to draw your attention to a most thought-provoking discussion of linguistic stereotypes in the Inaugural Lecture of T. E. Hope, Professor of Romance Philology at Leeds¹ It sent me back to think afresh about the role of the stereotype in variety identification and modification. Parody and hyper-correction, for instance, are often explained in terms of the stereotype. Labov's investigation of social stratification in New York English is based linguistically on the stereotype (he investigates only five phonological variables), just as socially it is based on one variable. The investigation works in the sense that significant correlations are identified. It may be the case that the stereotype is sufficient to account for New York variation, but it is certainly the case that no investigation has been made of anything else. We know what Labov has caught in his net, but we do not know what has slipped through it. One's intuitions are that although stereotypes operate in English English, there is a great deal they do not account for, and much experiment in applying some of Labov's questions to English informants confirms this impression. However, a secondary stage in the application of the principles of numerical taxonomy is the identification of diagnostics, i.e. a relatively short list of criterial identifying characteristics by which a variety can in future be identified once its existence has been established by the original long list of criteria. I won't go into the mathematics of how this is done, but I would compare the difference between the original profile and the diagnostic identification with the difference between a spectrogram and the stylised input to a speech synthesiser. As for a current test for the role and identification of stereotypes, I shall soon be doing an experiment on this as it relates to varieties. Possibly stylisticians could find something in these ideas to apply in their own field. The essential point is that stereotypes can be made to emerge from a sufficiently exhaustive taxonomic approach.

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