

OBJECTIVE AND SUBJECTIVE IN STYLISTIC ANALYSIS¹

David Crystal

University of Reading

It would be unwise as well as arrogant, in a volume of this kind, to commence a theoretical discussion of any one aspect of stylistic analysis without providing some characterization of my approach to the subject as a whole. Some kind of terminological preamble is, regrettably, still a necessity for mutual intelligibility in this field. Stylistics, then, I would define as the linguistic study of systematic, situationally-distinctive, intra-language variation. By 'situation' I am referring to a sub-set of non-linguistic variables (such as occupation, status, purpose, regional or class background)² which a native speaker can intuitively identify as accounting for a particular selection of linguistic features in a given (spoken or written) text. 'Feature', in this definition refers to any bit of speech or writing that may be singled out from language and discussed--a particular word, morpheme, sentence, structural relationship, etc. Now this definition of stylistics is an extremely broad one--it subsumes both literary and dialectal use, for instance--and some explanation of this is perhaps necessary.

In my view, stylistics cannot be meaningfully restricted to the study of literary texts, as the linguistic explication of such texts is theoretically dependent on the prior explication of non-literary variation. 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 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. Any stylistic model is inevitably going to be to some extent arbitrary, at present; and practical considerations are going to be primary in any question 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. Stylisticians, if it is true, do frequently pay lip-service to various fundamental tenets of a scientific approach--saying, for example, that stylistics is, or should be 'objective', 'systematic' and 'explicit' (a full discussion of these terms is to be found in Crystal, 1971a); but their practice invariably falls far short of the rigorous standards implied by these terms. It is instructive to examine why, and I propose to do this with reference to the notion of 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: 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 implies the notions 'successful', 'satisfactory', or 'accepted'. It would be of little value for a stylistician to take 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 systematically, 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, hypotheses become non-rigorous, and 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 behaviorally, 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 these days), 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 preconceptions 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 minimize 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 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. (The only way of avoiding the problem altogether, of course, would be to ensure that an analysis was based on comprehensive collection of data within a field. This may be a theoretical possibility in the synchronic analysis of some of the more restricted of 'restricted languages' [in Firth's sense]--but to insist that selectional criteria be removed from data gathering is not feasible as a general procedure. It is reminiscent, in many ways, of Abercrombie's example of the pseudo-procedure of defining the phoneme on behavioural grounds [1963:115].)

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 the fault of professional stylisticians--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, for example, 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' (1968) references to the register of 'policemen's English', I have heard people talking about 'traffic-warden's 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 a stylistic psychosis. Similarly, we have to beware assuming that because some varieties of a language use distinctive linguistic features consistently throughout any given text (as in religious English), therefore this consistency is always present; for this is not so. In oral story-telling, for instance, it is uncommon, even with professionals, to find the storyteller who puts on the voices of his various characters being absolutely consistent in his imitations. On the contrary, what normally happens is that the first few words of utterance in the voice of a particular character are spoken distinctively, so that there is a clear contrast with the preceding narrative, but after this the voice takes on an increasing number of the speaker's normal characteristics. As long as the initial identification is made, there seems to be little need to preserve the person-identifying features consistently. And what happens here may be found in many other spoken varieties

too (consistency is more likely in written varieties, of course).

However, to clarify the argument at this point, I propose to eliminate from the discussion all cases of stylistic uncertainty and inconsistency referred to in the previous paragraph, 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-naïve 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. Physician, heal thyself!

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,c 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 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 consider 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 irrelevant for most practical purposes. The important question, for, say, the teacher, is how much of this complete analysis the student will 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.

Some kind of test which could establish the generalisability of our 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. 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. Here, as elsewhere in linguistics, we must not let ourselves be fooled into thinking that our metalanguage is more precise than it really is. To take slightly different examples, the stylistician who talks about a linguistic feature constituting a 'dominant' pattern in a text, 'highlighting' a meaning, or making a part of a sentence 'more emphatic' is basically not doing anything different from the critic who comments about the forcefulness or vividness of a text. Because we know an author to be an experienced linguist, we tend to give his classifications more credence than the evidence will sometimes bear--for example, there are many who interpret Joos's suggestions about five degrees of formality (1962) with a rigidity which I am sure was never the intention of the author. Semantic metalanguage needs validating, just like any other.

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 broader concept of competence than Chomsky apparently does (e.g. Lyons, 1972)--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 Crystal & Davy (1969), 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 p. 95, ff.), 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 [cf. Moerk, 1970]). Using Halliday's terminology (1961 and elsewhere), 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 a situational basis; 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), for example 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 naive simplifications, capable of being shot down by the first sharp critic or 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 taxonomies are necessarily the best ways of handling varieties 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', for instance, 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 and Strevens (1964:88-9). 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 labeled 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, for example, 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 coming. 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.

Finally, I think the point has to be made that sooner or later this whole discussion must be related to the question of aims. Without an explicit and unambiguous statement as to what the objectives, immediate and long-term, of stylistics actually are, theoretical discussion will make little progress. There is space for one example of the kind of misleading emphasis commonly found in stylistics, which, unless corrected, produces a mass of pseudo-problems. It is often assumed that the main aim of stylistic analysis is increased semantic understanding of a text--that is, the analysis in some sense increases one's awareness of the 'full' or 'underlying' or 'pattern of' (metaphors are endless) meaning of the text. Now this sometimes happens, but it would be wrong to assume that it always does, can and should. Fresh and illuminating semantic information about language use is not a sine qua non of stylistics. The point is frequently misunderstood. It was misunderstood recently, for instance, in an interchange in the *Times Literary Supplement* following an article by Leech (1970). One argument used by F. W. Bateson in an attack on stylistics was that it was not helpful in analysing the meaning of a text: 'Unless the professional linguists can give us concrete examples of the relevance of their mystery . . . we must advise the literary student to ignore them' (letter, 28 August 1970). But this argument is beside the point, and if stylisticians rely too much on an argument from semantics, then they will lose. It is highly unlikely that a stylistician's analysis of a poem could provide anything in the way of an illuminating interpretation to a critic as sensitive and experienced as Mr. Bateson. And if it could, it is arrogant to expect people to make the vast intellectual effort required to master a particular terminology and method, solely for the sake of occasional illumination. People want quick returns for their mental expenditure. Moreover, it is not to be expected that all texts are equally susceptible to stylistic analysis--there are many texts which it is simply not worth while analysing in a large-scale, stylistic way, as they are unlikely to yield any semantic secrets at the end of such an exercise. But one nonetheless regularly encounters students who are ravaging a text statistically (e.g. a Lyrical Ballad), on the assumption that there must be a problem here somewhere!

On the contrary, an awareness of a problem, a tentative formulation of a hypothesis, must be present before any kind of convincing stylistic analysis can begin. And further, the level of the problem must be fully appreciated--who is it a problem for? One reason why much linguistic analysis of literature has not been well received is that linguists take texts which seem interesting and problematic to them; they often forget that the text, or the problems, may not be of comparable interest to the critic. The stylistician must thoroughly appreciate the literary critics' problems and position, if he wants to sell his product; and most stylisticians (because of the nature of their training) do not. A clear example of the existence of a psychological gap here is in the way in which few stylistic analyses ever pay careful attention to the textual data which have often been amassed by literary editors: far from ignoring it, however, stylistic analyses should be at pains to incorporate it.

In short, it should not be the implicit claim of stylistics that, when its techniques have been mastered, one will always be able to find fresh and illuminating answers to traditional problems. The value of stylistics for providing regular semantic illumination is really restricted to the introductory years of mother-tongue or literary teaching, where one assumes that most students lack the sensitivity and discipline of a literary critic, and thus need a (one trusts, temporary) method of analysis which will enable them to get more out of their reading than they would otherwise be able to do. Apart from this, the main function of stylistics is to facilitate discussion of a text: for someone like Mr. Bateson, what stylistics can hope to do is provide a systematic account of a text, based on general principles which will facilitate comparison, and a precise terminology, so that his and others' observations can be discussed in an atmosphere of mutual intelligibility. This is task enough! New facts may come to light in the process, but these should be considered as bonuses.

There has been much discussion lately about stylistic models and theories; but if we propose to think seriously in these scientific terms, then we must be highly self-critical as to whether our current assumptions and techniques are compatible with generally recognised criteria of scientific thinking. The purpose of this paper has been to argue that many of our procedures are not, and that attention should be directed towards their theoretical and methodological weaknesses that have on the whole been ignored. I personally believe that stylistics can develop a genuine scientific basis, and will be beneficial in the way in which present-day linguists claim. But I 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 stylistic analyses, it does not follow that our claims are necessarily valid. There are many brilliant stylistic analysts who are not linguists, 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 the source of any success that the subject has had so far; and at the moment I do not see how we can be, as too little self-criticism and real experimentation has taken place.

NOTES

¹This paper is a revised and expanded version of the first part of a paper read to a conference on 'Interdisciplinary Approaches to the Study and Teaching of Modern Languages', held under the auspices of the Centre for Information on Language Teaching, London, in April 1971.

²For an analysis and discussion of all the variables involved, see Crystal & Davy, 1969.

REFERENCES

- Abercrombie, D. 1963. Pseudo-procedures in linguistics. ed. by D. Abercrombie. London: Oxford University Press, 1965, 114-9.
- Chan, R. K. 1970. Illustration of a methodology for stylistic investigation with special reference to formality in English. Reading, England: University of Reading dissertation.
- Crystal, D. 1971a. Linguistics. Harmondsworth: Penguin.
- 1971b. Absolute and relative in intonation analysis. JIPA 1. 17-28
- 1971c. Prosodic and paralinguistic correlates of social categories, in Social anthropology and language, ed. by E. Ardener. ASA Monograph 10 (London: Tavistock Press), 185-206.
- Crystal, D. and D. Davy 1969. Investigating English Style. London: Longmans.
- Davies, A. 1968. Some problems in the use of language varieties in teaching, Educ. Rev. 20, 107-22.
- Halliday, M.A.K. 1961. Categories of the theory of grammar. Word 17.241-92.
- Halliday, M.A.K., A. McIntosh and P. D. Strevens 1964. The linguistic sciences and language teaching. London: Longmans.
- Joos, M. 1962. The five clocks. IJAL publication 22. The Hague: Mouton.
- Leech, G. N. 1969. A linguistic guide to English poetry. London: Longmans.
- 1970. The linguistic and the literary. TLS (23-7-70), 805-6.
- Lyons, J. 1972. Human language. in Non-verbal communication, ed. by R.A. Hinde. London: Royal Society and CUP, 49-85.
- Moerk, E. L. 1970. Quantitative analysis of writing styles. JL 6.223-30.
- Quirk, R. 1961. Some Observations on the language of Dickens. REL 2.19-28.
- Quirk, R. and J Svartvik 1966. Investigating linguistic acceptability. The Hague: Mouton.
- Reed, D. W. 1949. A statistical approach to quantitative linguistic analysis. Word 5.235-47.
- Thakur, D. 1968. A stylistic description of four restricted uses of English in science. Reading, England: University of Reading dissertation.