

"COMPUTING ST. PAUL"

A cautionary essay in stylistics by David Crystal (1).

TRANSMISSION: Third Programme, March 16th, 1965.

I want to look at some recent and fairly heated controversy from what I believe is a fresh point of view. What is at issue is the research that has been carried out on certain Biblical texts, using a computer, and a combination of statistical and stylistic techniques. The controversy, in which the Rev. A. Q. Morton has had a prominent part, has centred on the question whether the Epistles traditionally attributed to St. Paul are genuine or not. I would like to examine and criticise this research from a standpoint that surprisingly has been ignored so far - that of Linguistics. Over the last year the dramatic and sweeping theological conclusions which the experimenters - including Mr. Morton - drew from their project and published in prominent newspapers have had more than their fair share of attention. I think it is unfortunate that the debate should have stayed wholly on this level - unfortunate because it has meant that certain fundamental flaws in the pre-theological stage of the argument have been passed over. Now I would maintain that the scientifically-orientated stylistic procedures and principles used in framing the experiments turn out to be invalid, when thoroughly examined in the light of the science concerned. If the stylistician had been brought in from the very beginning, he would have been able to demonstrate that in reaching their conclusions and in making such sweeping claims the experimenters had in fact overreached themselves. The theological debate might then have had more point to it, and not got totally out of perspective.

It is important to stress this viewpoint now, because otherwise the whole argument may come to a theological head all over again - and that, in my view, would be quite unnecessary. The controversy is being revived as the result of a new publication, a paperback by A. Q. Morton and J. McLeman called Christianity and the Computer, which repeats the theological views propounded in the earlier newspaper articles at slightly greater length. This work, unfortunately, is not the technical monograph which some of us had been expecting. It is, in fact, a rather premature popularisation. Most of the book is given over to discussing the wider theological and personal issues that arise, and little space is given to the research itself. Indeed, despite the title, the computer is hardly mentioned.

Are the linguistic principles on which Mr. Morton implicitly bases his stylistic analysis valid? This question is crucial, but so far no-one has made the point that the revelations and conclusions of stylometricians such as Mr. Morton stand or fall by the soundness of their linguistic criteria. The evidence he takes from other disciplines, such as statistics or logic, is ancillary to that deduced from linguistics, from the scientific study of language; and stylistics has its place as part of this study. Now the linguistic study of style must not be confused with some traditional notions that surround the word, notions which link it primarily with literary criticism. There is of course an overlap, but stylistics, in its linguistic sense, is more comprehensive, descriptive, systematic and objective than this. It may^{be} broadly defined as the study of patterned variation in the use of language, which can be related to definable situations. The language of literature, of poetry or creative writing generally, is just one such use, to be taken, in the first instance, as on a par with other uses such as the language of (say) law or science. The scope of stylistics is wide. One can study the language of a period, or a group of people, to find out the linguistic characteristics they share; or again, one can examine the language of an individual, in order to discover what variations in his use of language are properly his, and not features shared with other contemporary users. From here, there is an easy bridge to problems of determining authorship: by comparing two texts attributed to the one man from an identical point of view, one can highlight similarities in patterning which would suggest a common author. Of course this is so, only when rigorous controls are set up for the research. If you pay no attention to these, then serious errors appear in the results; and this seems to be just what has happened in the present instance.

One of the reasons, of course, for the failure to argue the Pauline controversy on linguistic grounds is that the subject of Linguistics is relatively new. The information about language which has already been got together is still unfortunately largely the property of a specialised academic minority. Even such a closely-related subject as the teaching of modern languages has only recently begun to wake up to the developments in linguistic science. It's not surprising, then, that other fields in which language plays an important part, such as philosophy, or religion, have not so far received much stimulus either from linguistic research. This has meant that some fundamental principles of language, and well-tested descriptive procedures, have as often

as not been ignored or abused. You have only to think of the logical positivists - of the narrow basis on which they selected the linguistic evidence used in support of their theory, for example; or, more recently, of the Bishop of Woolwich's failure, in Honest to God, to appreciate the essential role of metaphor, and analogical language in general. It is no coincidence that such abuses have frequently occurred in recent attempts at revising Christianity because the central role language has to play in the expression of a religion provides a natural way in for more serious theological criticism. It has happened again here.

I take Mr. Morton's work as my chief example mainly because his research is so well-known, but also because he has chosen to launch such a frontal attack on certain fundamental beliefs. Even so, my argument would apply to any case where somebody tries his hand at settling problems of authorship in this way. I shall make use of two assumptions: first, that any scientific experiment must incorporate realistic principles consistent with the evidence that the science has already uncovered; and secondly, that in interpreting his results, a scientist must ask realistic and pertinent questions to get correspondingly useful answers. I do not think the Pauline experimenters do either of these things.

You may be familiar with the general lines of Mr. Morton's research; but I would still like to briefly summarise his experimental procedure, because certain points need to be emphasised. I shall paraphrase the earlier newspaper article in the Observer, therefore, as the recent book adds little to this. In this article, he told how, by using modern scientific techniques, including the operation of a computer, he had proved beyond reasonable doubt that Paul himself wrote only five Epistles. This meant, among other things, that theologians had to jettison doctrines which had thereby been shown to be groundless, that the authority of the Church as the interpreter of the Bible had to go, and that authority was called upon to yield to the advance of knowledge. Mr. Morton's attempt to determine the authorship of the Epistles was based on the assumption that one could discover "fingerprints" in the brain in the form of word patterns; or in other words that there were some unconscious literary habits that could be traced in everything an author wrote. These were to be found by first analysing the number of words in sentences - on the assumption that the different sentence-lengths were characteristic of an author - then by taking six common words of Greek prose, to see if there was any

consistent pattern in the frequency of occurrence. The words were, in their English equivalents, "and", "he/she/it", "but", "in", "the", and the aggregate use of all parts of the verb "to be". These seven criteria were also applied to about 600,000 words drawn from more than a dozen authors whose works spanned several centuries; and Mr. Morton found that his tests applied to all those writers of Greek prose, regardless of the length of time over which they wrote or the variety of their subject-matter. Applied to Paul, the tests showed that five of the fourteen Epistles were stylistically indistinguishable - Romans, first and second Corinthians, Galatians and Philemon. The remaining nine came from at least five other hands. As no scholar has ever challenged the view that Paul wrote Galatians, Mr. Morton concluded that these five must be the genuine Pauline Epistles.

Now on the evidence produced, the stylistician is not likely to accept Mr. Morton's method or his conclusions. There are important procedural problems which anybody setting out to analyse style in this way has to bear in mind. The study of style, in fact, has been judged by many scholars to be the most elusive and complex part of the whole discipline of linguistics. It lends itself very easily to distortion and oversimplification. There are a number of good reasons for this. The most important is that stylistic analysis only becomes possible after - not before - one has successfully completed some other nonetheless complex linguistic tasks. You have to be able to describe the grammar of a language, for example, before you can begin to talk about the "stylistics" of grammar, that is, about the way individuals or groups of individuals habitually use certain grammatical patterns. And the same applies at other levels of language organisation, which deal with patterns in sound, vocabulary and conceptual meaning. Another way of putting it would be to say that you have to know the norms of language first, before you can assess at all adequately the departures from the norm which are the basis of stylistic effect.

This concept of the norm is of central importance. You may be studying the style of an individual, or the style which characterises a particular group of people, or you may study that style which characterises a particular situation, regardless of the individuals who compose it (such as the language of science); but in each case you need first to set up a norm or yardstick to provide a stable framework for description, and to indicate which

information is relevant and which is not. For example, you cannot decide which part of (say) T. S. Eliot's work is characteristically his and which is not unless you first know what the normal, contemporary language is from which he departed. Just as it's impossible to estimate a pathological state without first understanding the normal physiological condition, so with stylistic effect and analysis. All stylistic judgements imply a previously worked out analysis of normal language, so that the author's own patterns can be assessed in a realistic perspective. This is the only way to discover significant characterising patterns in a text. Without a norm to use as a yardstick, which is derived from a description of the contemporary language of an author, there is going to be a distorted sense of stylistic values. "Without a norm", says Professor Randolph Quirk in The Use of English, "it is difficult to recognise or practice originality"; and a recent monograph on the nature of style, by Professor Nils Erik Enkvist agrees. He says at one point that "All stylistic analysis is ultimately based on the matching of a text against a contextually related norm". Robert Graves has also made the same point in a letter to The Times, but from the author's point of view: "every English poet", he says, "should ... master the rules of grammar before he attempts to bend or break them".

The procedure involves then setting up a norm, and afterwards comparing an author's personal linguistic quirks to it. This is especially necessary when the language under discussion is no longer living, as there is then no possibility of having recourse to direct intuitive judgements about the general usage of the time; and it is crucial when authorship is uncertain in the first place. Characteristic patterns must first be determined statistically from the language as a whole. Then one can compare them with the data in whose authorship one is interested. Without such a comparison, it is very easy to assume that one has pinned down an idiosyncratic feature - something that will help to identify a particular writer - when actually the feature is a 'normal' one, common to every user of the language. The fact that St. Paul uses a particular language form so many times per thousand words is only significant if you can show that other writers of his place and time do not.

My first major criticism of Mr. Morton's method, then, is that he gives no evidence of sufficient empirical descriptive work to set up the relevant norms. He has consulted "more than a dozen authors whose works span several centuries"; and he cites Isocrates and Aristotle as examples. But they lived three hundred years earlier than Paul; they worked in very different social situations, and their language was in a very different state. More important than this, though, is that they treated of very different topics and themes, and this would produce substantial linguistic divergence. It is like comparing the language of a 16th century philosopher with the Screwtape letters. The diversity of texts and times cannot be taken as any kind of norm, even if the size of the sample, some 600,000 words, were adequate. They are too far removed from the language and themes of St. Paul to be comparable.

A much more convincing computer experiment has been carried out in Sweden which puts this inadequacy more in perspective. Here the question was to determine the authorship of a set of eighteenth century diatribes known as the Junius letters. The Swedish investigators got better results because the manuscripts were set against a norm of comparable work within the same time and theme. Also, the frequency of a large number of formal features was analysed against this norm. Sixty contemporary authors were analysed, all writing on the same political themes; 450 tests were used, and one-and-a-half-million words were processed; and even then, the investigators did not pronounce themselves completely satisfied! Alongside such figures, I wonder whether the Pauline Epistles provide sufficient data for statistical analysis under any circumstances.

The fact that norms are so important is the main reason why carrying out a stylistic analysis in a satisfactory way is so difficult.

The second cause of difficulty is that you have to cater for stylistic development. The style of an individual changes frequently over the course of a life-time; and one must ask: what variables are the potential causes of stylistic variation? In my view, these variables fall naturally into two contextual groups: internal, psychological pressures, and external, environmental pressures. In the psychological category fall such matters as the author's maturity; his past experience of writing, of life, of his theme; his knowledge of the language in which he is writing; his emotional state of mind; the aim of his writing; his knowledge of the subject-matter; the scope of his theme; his views on future

developments; his difficulties of transmission, and so on. In the external category would come the pressures of his times; the character of his immediate and distant audiences; the restrictions of the language in which he is writing; the status of his subject-matter; his reputation, and so on, any of which could influence the probability that an author would choose one form rather than another. In view of these variables, it is unlikely that an author would write naturally in the same style over a long period; to give the impression of uniformity demands careful polishing and revision. It is not even a question of years, but of hours: style can vary inexplicably within a few pages, within a talk like this, with no obvious functional purpose behind the change. How much more variation, then, is there liable to be in a person's style over his writing lifetime? Stylistic analysis is therefore further complicated, as it cannot afford to ignore variation due to inevitable contextual and temporal change. In questions of authorship this is a very important complicating factor.

In the light of all this, Mr. Morton's claim that his tests have produced uniform results is highly uncalled for and unrealistic, particularly in the case of St. Paul, where the list of contextual variables was particularly extensive. For instance, he was bilingual, and had that to cope with. He wrote his letters in what was probably for him a second language, Greek; and if his native language was Aramaic, this must have had some effect on his early writing - an influence which probably decreased as he improved in fluency. Again, there is the means of transmission he used; you cannot just dismiss the amanuensis theory as Mr. Morton does, without giving good reasons; and if there was a secretary of some kind, then here is another source for stylistic variation.

But there was a third - and indeed central - influence on Paul's style. The differences in situation in which he wrote over the span of twenty years were extreme; and the changes in personal mood and outlook must have been very marked, as he increased in maturity and developed his belief. The Junius experiment catered for such factors: there the investigators said: "Linguistic preferences are not permanent. Our language, our habits, our tastes, change over the years". On the face of it, this would seem to be obvious; but Mr. Morton nonetheless assumes a state of stylistic uniformity in the Epistles. He even claims his proof of this as a point in his favour. It is in fact the heaviest point against him. If a style is "proved" not to change -- and he says "five of the fourteen Epistles are indistinguishable" -- then this strongly suggests a linguistic unreality. I would even call it, stylistically speaking, a solecism, a contradiction in terms.

A cautionary essay in stylistics by David Crystal (2).

TRANSMISSION: Third Programme, March 23rd, 1965.

I suggested last time that there is a need for a much closer look at the research methods used by the Rev. A. Q. Morton and others in their analysis of the Pauline Epistles. Mr. Morton's experiments need to be subjected to rigorous linguistic analysis, based on scientifically valid procedures. He claimed to have proved that only five Epistles were genuinely Pauline. I suggested that these results could not be fully trusted because they ignore the complexity of valid stylistic analysis. Two kinds of information are required as a preliminary to such analysis: first, adequate norms on which to base comparisons have to be established; and then, one has to take account of the many variables which affect stylistic development over an author's lifetime. In my view, Mr. Morton has not adequately met either of these preliminary requirements; and I now want to look at the effects of this in more detail, by examining the implications of his specific stylistic criteria. He used two such criteria: first, he assembled information about sentence-length; and then, taking six common words of Greek prose, he tried to determine whether there was some consistent pattern in the frequency with which they occurred.

But there is a third major stylistic principle involved here - or rather, it should be. If we are to discover the stylistic norm of any writer then we must take into consideration all the work attributed to him for extra-linguistic reasons. Style may be the man; but if so, this is without qualification: it includes the whole self or output, not just a few memorable sentences, or a few selected patterns from it. In other words, an author's personal linguistic habits are not to be found by examining only part of his work and generalising from that. A stylistic description must begin with no preconception. The analyst cannot pick or choose certain patterns or texts which he assumes are likely to be characteristic. That would be, not science, but guess-work. One needs a methodical, comprehensive description of the whole corpus, using a tested linguistic framework to cover every formal feature it presents. Only this will yield information that may validly be compared with the contemporary language norms - I am assuming, of course, that those norms have previously been established. Statistics of (say) frequency of occurrence can indeed help form an opinion as to authorship, but only if you take note of all possible patterns, and do not select a predetermined few which happen to suit your case. The relevance of this point will be plain if we look at the kind of formal feature that would be involved in an adequate descriptive stylistic study of this kind.

A descriptive study involves analysing and classifying the linguistic features of a text. This is where the statistician and the computer come in, of course, as elementary sorters and classifiers. They determine totals of forms and proportions of usage, and help to form opinions about the balance of stylistic patterns in the data. These patterns are either physical or non-physical. The physical patterns, which are more readily quantifiable, are the actual spoken or written forms used - we discuss the patterns of phrase, clause, sentence and paragraph structure, the morphological composition of words, the occurrence of "favourite" words, rhymes, rhythms, and so on - in other words, the phonological, lexical and grammatical aspects of language. The non-physical patterns that need to be described are those connected with the content of a writer's work - his themes, typical attitudes, recurrent concepts, methods of pointing relationships, and the like. Both kinds of pattern are necessary, because either may provide the key to a person's textual individuality; although usually, originality in style and originality in content do tend to occur together. Merely to describe formal patterns and then to stop there gives us only half the author - and indeed the less important half; (for ultimately what you want to say is more important than how you want to say it). To fail to cover a large number of formal patterns is even more hopelessly unrealistic.

Now on this count alone, Mr. Morton's criteria are grievously incomplete. He leaves so much out. The semantic patterns in the Epistles are, after all, part of the raison d'être of the texts in the first place; yet he gives them no place at all in his research. On the other hand, when we examine his attempt at formal description, remembering what I said about the need for a total survey covering all formal features with a stylistic potential, we are in for an even greater surprise. Having, as he says, "thought the minimum number of tests of authorship would be three, and that five would be enough for all normal purposes", he goes on to choose seven patterns only, and assumes that these satisfactorily represent St. Paul's style. This is in itself a gross inadequacy; but it looks even worse when one considers just what seven formal features have been chosen - six features from grammatical systems of words, that is, and finally sentence-length.

Grammatical words, or "form" words as they are often called, are words like the definitive articles of English or Greek, the personal pronouns, or the conjunctions. They are finite in number, and function together in closely-knit groups, or systems, at the grammatical level of language organisation. Such words, like "the", "in", or "but" have little referential meaning when taken in isolation. They have more of a grammatical meaning with a function as relationship-indicators.

They point the relationships that exist between the main, open-class, lexical words of language; so that in such a sentence as "The big man was walking down a road at teatime", the lexical words are "big, man, walking, road, teatime", and these are related to each other (apart from their word-order) by the grammatical words "the, was, a, down, at".

Now from the point of view of stylistic analysis, it is important to note that it is the second group of words - the grammatical words - that language cannot do without in normal circumstances - I mean, for example, in letter-writing. They occur with little regard for which lexical words are being used. Their high frequency, then, and their heavy functional load, would suggest that they are of very little use as stylistic criteria, because no author could exercise much choice in their use - and style, after all, is largely concerned with matters of conscious choosing between linguistic alternatives. For any kind of complex self-expression an author needs to make use of important content-manipulators like "but", "and", the definite article, and the parts of the verb "to be". It is usually meaning which governs their use, not style. The grammar of a language exists above and beyond the desires of individuals; and while you can manipulate the patterns of word-order to a certain extent, such flexibility is not present within the more finite sub-systems that indicate specific relationships. It is highly unlikely therefore that they would represent unconscious literary habits, of the kind Mr. Morton suggests.

His mention of "unconscious" language habits in any case begs a question. It is, of course possible for writers to display habits of an unconscious nature; but how do you distinguish conscious from unconscious literary habits on the page? On what grounds do you tell when an author is using a pattern unconsciously, and when not? It is extremely difficult to decide this, even when your subject is alive and so able to confirm your judgements. It is probably impossible with somebody who has been dead for nearly two thousand years. And so, to assume that certain linguistic features are unconscious habits, without being very carefully explicit about your criteria, is highly unsatisfactory.

There is a simple, practical way of showing that the grammatical words an author uses - whether his use of them is conscious or not - are inapplicable as a criterion, and that is by comparing their frequency in his work and that of other writers. The fact that one gets the same results would indicate that they are part of a situational norm, features of the language as a whole, rather than personal features from an individual norm. Taking the word "and" as an example, Mr. Morton argues that if it occurs regularly more than a certain number of times every thousand words, let us say, then this is statistically significant, and is evidence for common authorship. On that I would say first

that it could only be statistically significant if the statistical norm for the contemporary language had previously been determined, so that the frequency of deviations over a wide range of texts and authors could be found. This would, of course yield an objective norm of occurrence. Secondly, with such a high-frequency form word as "and", the results would in any case be unlikely to show significant evidence for a common authorship. Mr. Morton concludes that five of the Pauline Epistles are indistinguishable in this respect. He gives few figures, so it is difficult to argue; but I wonder how much of a swing he is allowing in order to class work under the same author. When does a "slight anomaly" (as he puts it in his recent book) become something which is "mathematically significant"? What are the "accepted limits" (which he never defines) that distinguish the idiosyncratic from the general use of language? If text X has "and" once every 100 words, for example, and text Y has "and" once every 103, would these be allowed together? If so, would 104, 105 and so on? Where do you draw the line? - for there has to be one - and what are the criteria for your choice of line-drawing?

But even if you allow a generous measure on either side of an arbitrarily determined percentage, this does not produce usable results; for a similar percentage occurs time and again in other authors. Positive results based on frequency of occurrence alone prove nothing. Take, by way of illustration, Mr. Morton's own newspaper article. It used "and" once every 61 words on average; and some advertising copy on the same page once every 60 - and these were two short articles with two distinct kinds of subject-matter. On the other hand, Mr. Morton's answer to objections in the newspaper the following week displayed "and" once every 104 words - a substantial difference for the same author. Of course, if positional criteria are invoked, then the evidence is potentially more significant; to know exactly where in a structure the word "and" tends to occur could be useful information. But even in this case, it would still be necessary to set up norms for the language as a whole; and in the research I am discussing now positional criteria are not given at all.

So the choice of form-words is not a useful criterion in stylistic analysis; nor is the choice of sentence-length, unless great care is taken with it. C. C. Fries gives a whole chapter in his book, The Structure of English, to past definitions of the sentence: there are over 200 of them, all different. The sentence

is one of the trickiest linguistic concepts to define. In stylistic analysis, however, it is essential to define the term, if only to provide a frame of reference for comparison - especially with the Epistles, where a number of sentences are left unfinished. But no definition of sentence is given by Mr. Morton. Of course, even if you did establish satisfactory correlations in some texts by using these criteria, it is important to remember that the absence of such correlations in other texts proves very little. This is a very relevant point of principle. Mr. Morton's criteria show statistical similarities for five Epistles; but it does not therefore follow that the other nine are not by the same man. Reliable judgements of style must rely on positive data, not on negative inference. You can only really prove authorship from features which do appear; you cannot positively disprove it from features which do not.

If such criteria as form words and sentence-length are so uncertain, what would be reliable style characterisers? The answer lies in considering what you could call an author's "unfamiliarity potential", the source of his stylistic originality.

There are two kinds of language variation which would make good criteria, because they allow a wide range of choice on the author's part; but both of these Mr. Morton omits. First, from the semantic point of view, there are the lexically-full, open-class words of language which carry most of an author's meaning; these are good indicators of style because more thought has to go into the choosing of the "right" word in a given context, especially when abstract topics are under discussion. Every author has his "favourite" words in this sense. Again, from the formal standpoint there are the stretches of language longer than the single word, units of structure. Single words as a test of authorship are not really relevant, because people do not write in single words. As Professor Nils Enkvist has recently said, "A given word in a text only acquires stylistic significance by juxtaposition with other words. Therefore uncontextualised statistics on single items are of no stylistic significance". People write rather in groups of words - phrases, clauses, and above all, sentences. These can of course be of any length depending as much on what the author wants to say as on how he wants to say it; but they do have a definable internal structure which can be to some extent characteristic. Thought is carried by stretches of language occurring successively; it is not characterised by single words occurring at intervals. Since it is the author's

thoughts - and the way he orders his thoughts - that are the most unique part of him, these are what exercise most influence on his choices in language; and they cannot be ignored by the stylistician.

Perhaps the most fundamental criticism of Mr. Morton's scientific method is that it is not scientific throughout. In the Junius experiment I mentioned last time, there were writings available known to be the work of the supposed author of the letters, so the task of comparison was eased by a valid extra-linguistic premiss. In the case of Paul, there is no autographic certainty about any of the Epistles. But Mr. Morton uses a fundamental, extra-linguistic proposition and bases his conclusions on it. He assumes that one of the Epistles is unquestionably genuine: "No scholar has ever challenged the view that Paul wrote Galatians", he says, and he concludes that those Epistles which conform to the proportions of the seven criteria in Galatians are by the same person. The argument, then, is resolved by a pre-scientific pre-conception. The fact that five Epistles show comparable patterns is not significant; indeed, it is to be expected. Patterns would emerge from any five texts you happened to select, depending on which criteria you used; and I wonder how many other formal criteria exist in the five texts which would serve to differentiate them. There is just no way of resolving this uncertainty without a complete description of the Epistles' language. Only this could provide really worthwhile statistical correlations.

Finally, I think it is worth emphasising the humble place of statistics and the computer in this research. The computer, as has often been pointed out, is only as clever as the information with which it is fed at the beginning and the questions it is asked. It is as fallible as the linguist or other scientist who puts the questions and programmes the machine for operation. There is always a subjective element involved here. Any result a computer arrives at must be read by human beings. Before a conclusion can be reached, they will have to assess which points in the result are significant and which are not. Statistics by themselves prove nothing. As Mr. Morton himself says elsewhere, they are no panacea. No amount of statistical data can ever positively prove a point like authorship. Proof is a matter of intellectual satisfaction regarding the truth; and all statistics can do is help form an opinion. And of course, if you ask the wrong questions, then the answers will be irrelevant. Mr. Morton, then, has erred on two counts: he has not put in sufficient information for the computer to assess the problem satisfactorily; and he has asked the wrong questions about the problem. The newspaper headline at the time read: "A Computer Challenges the Church". It gave the wrong impression; because the computer can do no such thing. The machine can only do what its temporary master tells it, and it

There is nothing binding therefore about the results. The computer and statistics cannot speak ex cathedra. They certainly cannot challenge the Church; only people can do that.

In all that I have been saying, I have taken the work of one investigator as my chief example. But the stylistic principles which I have outlined would apply to any work of a similar kind; and I am more concerned that future researchers on these problems should not make similar mistakes. In the present instance, I have tried to show that those concerned with theological issues - the status of doctrine and so forth - need not worry too much about the attack. Not only are the theological implications that arise from the question of authorship marginal - as others have pointed out before me - but the procedures used in the experiment are suspect on too many grounds to be finally convincing. And while many interesting religious arguments were brought to bear on Mr. Morton's work after it first appeared, these need never have been raised if the stylisticians had been the first to answer back.