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Colección dirigida por Ramón López Ortega,
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2nd International R.C.E.I. Prize

The winning paper in the 1984 competition was Thomas Frank's "The Rise of Do-support in Modern English. A Reappraisal", published in this issue of *R.C.E.I.* Also appearing in this number are three others which the jury accepted for publication. These are "Recursive Premodification as a Literary Device in Iris Murdoch's 'The Sea, the Sea'", by Angela Downing; "How to Talk about Prose Style: An Example from Golding's *Lord of the Flies*", by Norman Macleod, and " 'Spot this Mumbo Jumbo': Thomas Pynchon's Emblems for American Culture in 'Mortality and Mercy in Vienna' ", by Claire M. Tylee.

R.C.E.I. wishes to extend special thanks to those who participated in the competition, and to congratulate the winner for his success and those whose work deserved publication.

The editor and staff of *R.C.E.I.* would also like to express their gratitude to the Council of Education of the Government of the Canary Islands for generously financing this prize.

THE RISE OF DO-SUPPORT IN EARLY MODERN ENGLISH: A REAPPRAISAL

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The origin and the development of the syntactic feature known as do-support, a feature peculiar to modern English, has been amply investigated, both by older, traditionalist historians of the language, and more recently by linguists working within a transformationalist framework of reference. It might therefore at first sight appear that little else remains to be said on the subject. The present article seeks to show that, without in the least pretending that what follows constitutes the last word on the nature and history of do-support, the whole question is still open to new contributions and interpretations.

1. In what follows I shall limit myself to the investigation of the incidence of do-support during the 16th and 17th centuries, the period during which the situation as we know it today establishes itself, but I shall, for the sake of comparison, also take into consideration certain evidence from late Middle English. Previous scholars, and particularly Ellegård 1953 in his meticulous and detailed study of the history of do-support, have concentrated either on a largely statistical, distributional approach (but in spite of the large number of texts investigated by Ellegård, I shall have some reservations to make about the way he uses his figures), or have treated do-support within the general framework of a transformational theory of grammar (Traugott 1965 and 1972, Hausmann 1974). This latter approach has the advantage of considering the use of 'do' as part of the wider question of the structure of the verb phrase (VP), or rather the feature generally labelled "aux" in the phrase structure. I shall attempt to show that the rise and use of do-support can only be understood when considered in conjunction with the whole structure of the feature aux, and my contention will be based not only on theoretical considerations, but also on sample statistics of the distribution of the relevant features. In other words, what I shall attempt to do is to combine a theoretical and a statistical approach to the subject. It is obviously impossible at this stage to go into the many theoretical problems this raises, but it seems fairly clear that in any diachronic study the almost a-priori rejection of corpus-based investigations by earlier generations of

transformational grammarians is misplaced, since we simply have no other evidence to go on. It is certainly useful to speak of different synchronic states, or grammars, of given features, but these grammars surely have to be somehow connected, and corpus-based, statistically tabulated evidence would seem to be a more useful procedure than random examples from different stages of the language. In this connection it is perhaps not superfluous to insist that any reference to "speakers" in a historical investigation of this kind is entirely improper, since we are dealing exclusively with "texts" in the most traditional sense of the term, namely written documents that have come down to us, and however close we may feel some of them to be to the spoken language (and some of this sense of "closeness" may well be illusory), they remain part of the written language.

It is true that the scholars just mentioned, like previous historians of the language, and others like Lightfoot 1974 in his interesting work on the history of modals, use illustrative material from the texts studied, as is of course inevitable when one sets out to treat the history of a form. But these materials generally constitute merely random examples. Just as in lexical work the first occurrence of a word may be profoundly interesting, but does not tell us anything about its frequency or general acceptance —indeed, it may well be a freak occurrence—, so also in dealing with syntactic structures illustrative examples are valid and interesting, but do not tell us enough: they do not tell us whether a new rule has become established and fully accepted, and to what extent it has replaced an older rule. In order to verify that, we must have some recourse to statistical evidence and possibly also to the theoretical formulations of contemporary grammarians, especially pedagogic grammarians, even though during our period not very much can be gleaned from these sources.

2. We have referred above to the concept of "new rule". Chomsky and Halle 1968: 249 formulate the classical transformational view of the nature of linguistic change in these words:

"an observed linguistic change can have only one source — a change in the grammar that underlies the observed utterance.

A straightforward way of effecting changes in a grammar is to add new rules."

These rules may, in the words of Traugott 1972: 14, be of two types:

"When we look at the kind of differences that have occurred between grammar A at time X and grammar B at time Y, we usually find that these changes involve either simplification, or elaboration, very rarely just the rearranging of materials already available."

Chomsky and Halle in the passage cited are mainly concerned with phonology,

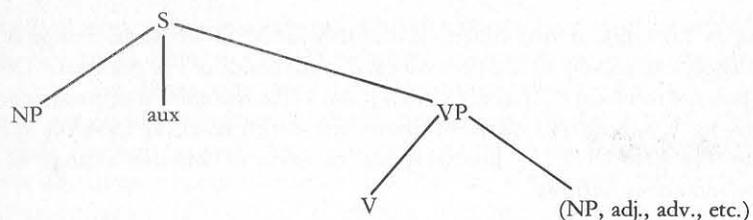
but as Traugott, among others, shows this model of language change is not meant to apply exclusively to the phonological component of the grammar. On p. 251, in a passage too long to quote, Chomsky and Halle maintain that parents and children may have internalized different grammars, which however have the same output¹, and Traugott 1972:13, discussing the question of whether change is gradual or instantaneous, affirms:

"Certainly, in the empirically observable performance of any one individual or group of individuals who come to favor one pattern already available to them over another also already available to them... we can observe over time quantitative change that is correlateable with such extra-linguistic factors as sex, age, class and so on. In another way, however, change is instantaneous. There is nothing gradual about acquiring a pattern; the moment it becomes part of one's competence, even in the most limited way, one's competence is instantaneously changed, at least to the extent that the pattern is new. Changes at the level of the speaker's competence are therefore instantaneous: changes at the level of the speaker's performance — that is, at the level of the quantitative and situational use to which he puts the changes that have occurred — are gradual."

Similar formulations abound in the literature. The reason I have devoted some space to the explanations provided by transformational grammarians for the mechanisms of linguistic change will become clear when we return to this particular problem towards the end of this paper in order to explain the co-existence for a considerable period of time of two sets of mutually exclusive rules, certainly beyond the sort of "generation gap" envisaged by Chomsky and Halle's G_1 and G_2 . But for the moment I would like to set out a description of how the structures that interest us are generated and compare this grammar with a previous grammar that generated structures historically attested, but noticeably different from those in current use today.

3. Clearly, as has already been pointed out, the whole question of the function of do-support in modern English must be seen in the wider context of the syntactic feature known as auxiliary (aux) in TG grammar. At this stage it is perhaps not superfluous to give an account of the grammar in question and of its historical evolution, based on some of the standard authorities on the subject, for example Traugott 1965 and 1972:137-42, Samuels 1972:173-76, Hausmann 1974, Lightfoot 1974, Bynon 1977:159-63, although it will be found that my notation differs somewhat from the one used by these authors, and above all that the conclusions I draw are my own.

In present-day English the following phrase structure will generate the required declarative active sentences:



The feature aux is an obligatory component which *must* always comprise the feature tense (tn) and *may* comprise a modal (M) and one or more Aspectuals, i.e. the auxiliaries 'have' and 'be', thus:

Rule 1: $aux \rightarrow tn + (M) + (have) + (be)$

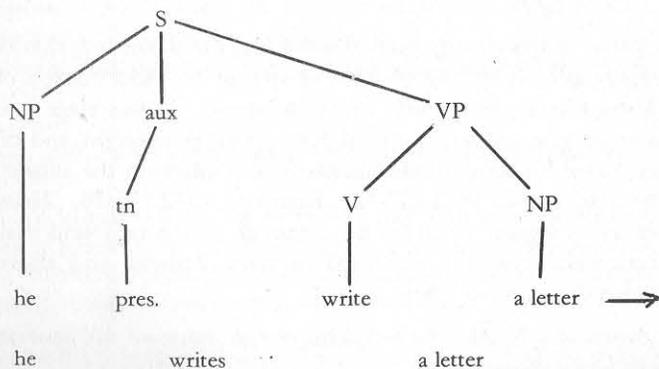
in this order. tn is a feature that is attached to the verbal element immediately to the right of it; it may be optionally followed by not more than one modal, 'have' or 'be', either on their own or in any combination desired, provided that the order given above is observed.

If we take as an example the verb 'write', we can call the forms

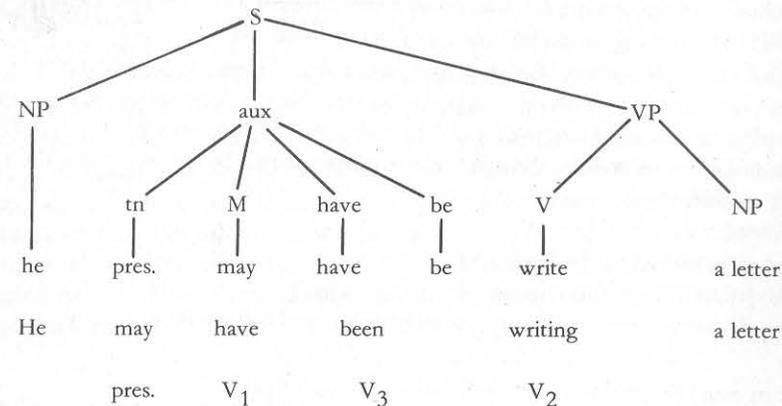
write = V_1
 writing = V_2
 written = V_3 and set up

Rule 2: $M \rightarrow V_1$
 $have \rightarrow V_3$
 $be \rightarrow V_2$

that is, the verbal form immediately to the right of M is 'write', the form to the right of 'have' is 'written' and that to the right of 'be' is 'writing'. This description will account not only for a sentence like



but also for more complex verbal structures like



where tn is attached to 'may', which is immediately to the right of it; 'may' in its turn generates a form V_1 'have', which in its turn generates a form V_3 'been', and 'be' a form V_2 'writing'.

We may for the sake of convenience say that tn is attached to a verbal feature X immediately to the right of tn. The eight possible forms generated by this rule are (V in this context means "main" or "lexical" verb):

- a) $tn + V \rightarrow$ he writes a letter
- b) $tn + M + V \rightarrow$ he may write...
- c) $tn + M + have + V \rightarrow$ he may have written...
- d) $tn + M + be + V \rightarrow$ he may be writing...
- e) $tn + M + have + be + V \rightarrow$ he may have been writing...
- f) $tn + have + V \rightarrow$ he has written...
- g) $tn + have + be + V \rightarrow$ he has been writing...
- h) $tn + be + V \rightarrow$ he is writing...

Rules for passive transformations are applied to the phrase structures thus generated. These provide, among other things, for $be \rightarrow V_3$, giving us for example:

the letter has been written
 $tn \quad V_3 \quad V_3$

A rule to the effect that $be \rightarrow V_2$ and $be \rightarrow V_3$ cannot co-occur in the same S may explain why a sentence like

* The letter has been being written
 tn V₃ V₂ V₃

is blocked as being unacceptable in present-day English, even though there are signs that this blocking rule is in the process of being modified.

But this is by the way. Let us return to our main concern, namely the evolution of the construction with do-support. In forming interrogative and negative sentences, we can lay down the following rules, noting that there are two basic types of interrogative sentences, though for our purposes the choice of one or the other makes no difference:

- a) tn is shifted to the left of NP₁
- b) (i) NP₂ is replaced by int. pron. (e.g. 'what')
- (ii) int. pron. is shifted to initial position and followed by tn

Since tn must be attached to a verbal element X, we get

- Rule 3: a) tn + X + NP₁ + aux + V + (NP₂, etc.)
 b) int. pron. + tn + X + NP₁ + aux + V + (adv., etc.)

The rule for the negative transformation in 'insert not after tn', giving us

- Rule 4: NP₁ + tn + X + not + aux + V + (NP₂, etc.)

Since M, 'have' and 'be' are optional features of aux, in both cases aux may be void. We note that both Rule 3 and Rule 4 permit a form of "splitting", i.e. separating X from the rest of aux. These rules account for the structures we all know in present-day English, which generate forms like

Can he write?
 Is he writing?
 Has he been writing?
 He cannot write, etc.

Why then 'does he write?' and 'he does not (doesn't) write'? There clearly exists a mechanism which blocks

tn + X + NP₁ + aux + V + NP₂ or
 NP₁ + tn + X + not + aux + V + NP₂

where X = V and aux is void, so that the grammar rejects sentences like

* writes he? or * he writes not

although we know of course that this blocking rule did not operate at an earlier stage of the language.

The reason I venture to advance for the development of the blocking rule is twofold: in the first place the presence of a considerable number of structures which contain one of the optional elements of aux exerted a pressure on what I call "simple VPs", that is VPs in which X = V, that is to say where tn is attached directly to the main verb; this is an aspect of the question we shall shortly return to. In the second place, if we assume that there is a long-term tendency, or "drift" in Sapir's terminology, for English to conform to a basic SVO order, the development of do-support, which in effect means that X = V is blocked and a dummy X ('do') inserted, can be seen as the result of a pressure to preserve basic word order. We can now establish

Rule 5: aux → tn + $\left\{ \begin{array}{l} \text{(do)} \\ \text{(M) + (have) + (be)} \end{array} \right.$

where 'do' is generally cancelled in declarative sentences in which X = V permits the order SVO to be retained, an order which is violated by the older rules which allowed a left dislocation of V in the interrogative and the insertion of 'not' to the right of V in negative sentences. Rule 2 will now have to be modified by the addition of

do → V_i.

It would clearly not be appropriate in the present context even to attempt to explore the exceedingly complex question of word order, its historical development and its implications in a transformational grammar. The whole problem has been amply treated in the literature, both from an empirical and from a theoretical point of view. Fries 1940 gives exhaustive statistical evidence showing how in the course of early Middle English SOV order gave way to SVO; see also Ramat 1980: 238-39 and the detailed treatment in Old and Middle English in Messina 1983. More general discussions of word order can be found from a typological point of view in Greenberg 1966 and from a "psychological" standpoint (that is, relating word order to prominence and focusing) in Antinucci 1977: 57-83. What the development of English since approximately 1700 seems to show is that the preferred word order is preserved even in interrogative and negative propositions, which previously permitted different solutions. The rules for present-day usage as formulated above guarantee in all (or almost all: we shall deal with the exceptions shortly) cases a word order in which V (meaning "main verb") always occurs in the right-most position of the verbal component of VP, and is normally attiguous to O or to Adjunct, thus:

declarative:	NP ₁ + tn + X + (optional feature) + V + NP ₂ /adj.
interrogative:	a) tn + X + NP ₁ + (optional feature) + V + ... b) int. pron. + tn + X + NP + (optional feature) + V + ...
negative:	NP ₁ + tn + X +not + (optional feature) + V + ...

Let us now briefly return to Rule 5 as formulated above. In principle we might have two solutions:

- a) where X = V, and V is not the right-most element, substitute 'do' for X.

or

- b) a rule which presumes the presence in the deep structure of a feature 'do' which cannot co-occur with M, 'have' or 'be', and which is cancelled in most cases where X = V does not violate the preferred word order. This is schematically expressed by the bracketed notation used in Rule 5.

Whether we accept solution a) or b) might seem a question of preference, but I venture to suggest that, in spite of the more abstract nature of rule b) as compared with a)², the former has considerably more explanatory power. A rule like "replace X by do in context Y" is of course perfectly feasible; but then we find the same 'do' at times also appearing in other contexts, e.g. in sentences like "I do hope you'll be able to come" vs. a do-less form "I hope you'll be able to come", so that one rule to account for the presence of 'do' in *all* types of sentences would seem much more economical. All we need in that case is the specification, or cancellation rule, which can be expressed in the form "cancel do except in contexts Y or Z", where Y specifies the conditions that trigger off obligatory do-support and Z specifies the constraints on the use of affirmative 'do', what the textbooks generally call "emphatic do". To what extent the term "emphatic" gives an adequate account of this use of 'do' is a moot point: it might be more accurate to distinguish between an emphatic/intensifying use, as in the sentence quoted above, and what I should prefer to call "negation of negation", in sentences like "I do like coffee", which probably presupposes a conversational exchange something like this:

A: I know you don't like coffee.

B: But I do like coffee.

where B negates A's negative affirmation (or presupposition) about B's preferences. As for the often remarked upon extension of affirmative 'do' during the 16th century, I should be inclined to classify this as a "marked" form of the present, or less commonly of the past, as opposed to an unmarked form without 'do'. But this is an aspect of the question which we will not pursue further for the present.

There are however two verbs that seem to contradict the neat scheme outlined above, and it is surely significant that they are two verbs that have dual status. The verbs in question are 'be' and 'have', which may function as components of aux, as we have seen, but may also acquire the status of main verbs, e.g. in sentences like "he has a new girl friend" or "she is my new girl friend". In these cases the ordering rule seems to be violated. How are we to account for this?

Let us look at the status of 'have' first. It is surely significant that 'have' in its simple form is becoming increasingly less common, at least in British English, and, at any rate in colloquial speech, tends to be replaced by 'have got', in which, syntactically speaking, 'have' is part of aux³, and thus a sentence like "have you got a cigarette?" is perfectly in line with the predominant SVO order (i.e. tn + X + NP₁ + V + NP₂) postulated above. American usage, which is rapidly gaining ground also in Britain, (in this case it might well be correct to say that the younger generation has a different competence from that of the older generation of speakers), treats 'have' as syntactically assimilated to the major category of verbs and subject to the same restraints, which accounts for sentences like "do you have a cigarette?". It would be fair to say that 'have' is rapidly losing the special status it previously enjoyed, and that its preservation of this status can be attributed to certain sociolinguistic restraints.

The same cannot be said of 'be'. A sentence like "is she your new girl friend?" clearly violates the SV order which contributes, according to my hypothesis, to the development of do-support. That the verb 'be', in English like in a great many other languages, enjoys a special syntactic status, a fact which has frequently been recognized in the past, surely does not need emphasizing. One aspect of this peculiar syntactic status of 'be' evidently concerns word order, which permits left dislocation and 'not' insertion to the right of it⁴, but I should be reluctant at this stage to offer an explanation for this, and I would therefore like to leave open the question of the peculiarities (semantic and syntactic) of what earlier grammarians were accustomed to calling the 'substantive verb'⁵.

4. Let us now turn to the second part of my investigation, namely the empirical, historical evidence for the development of do-support in contemporary English. Recent studies, like those of Hausmann 1974 and Traugott 1972, both of which treat the subject in a broadly transformational theoretical framework, give a series of examples, but make no attempt to quantify them in relation to a certain number of sample texts. Moreover Traugott's chronology, followed by Hausmann, for the substitution of an obligatory do rule for do-less rule seems highly questionable. In particular, whereas 1700 as the date when 'do' as an "obligatory dummy auxiliary in questions" (Traugott 1972: 199) is probably acceptable (but we shall have to return to the concept of an "obligatory rule" in these cases in the last section of this paper), about 1900 for negative sentences seems decidedly late, and the same might be said of 'do' as an "optional tense carrier" until as late as 1800. Even more puzzling is

Traugott's contention that 'do' meaning "truly, indeed" (the textbook "emphatic do" I have referred to above) was in use from approximately 1300 until 1600.

Undoubtedly the most complete study of the subject is Ellegård 1953, which relies heavily on statistical evidence and on an examination of a large number of texts. It does, however, in my view have two drawbacks. In the first place, the author is exclusively concerned with the use of do VPs vs. the use of non-do VPs, without taking into consideration the total number of VPs of all kinds, that is to say also those VPs in which one of the optional elements of aux is present. In the second place Ellegård instead of giving the data for individual texts conflates them, giving average values over a certain period. This may be useful to determine general trends, but on the other hand it fails to capture certain finer points and stylistic variations, which are not devoid of interest. Nevertheless, Ellegård's monograph (strangely neglected in Hausmann's otherwise interesting and informative paper) gives us a very valuable insight into the spread of do-support, especially during the 16th and 17th century, and I shall have frequent occasion to refer to his findings in what follows.

My own statistical investigations, carried out on a much more limited scale than Ellegård's, are based on two different types of texts and distinguish individual authors. All occurrences of finite verb forms in the sample texts chosen⁶ are taken into consideration. It should be made clear at the outset that whereas it was felt that dramatic dialogue was closer to the spoken language than the sometimes highly formal prose texts, in *no* case are we dealing with "speech" in the strict sense of the term, which is not accessible to us until the fairly recent invention of magnetic tape. We are dealing with "texts" in the most traditional sense of the term, i.e. with written documents, some of which are more informal in style than others (e.g. Pepys is more colloquial than his contemporary Browne), and these stylistic differences may be presumed to be reflected in the incidence of different verbal structures, although it is remarkable how constant certain features tend to be.

In the first place a distinction was made between "simple" VPs, i.e. those verbal forms in which *tn* is attached directly to V, or to use the notation given above, where X = V, and "complex" VPs, where X = M, 'have', 'be' or 'do' (the different categories were distinguished in the count, but they need not concern us here). In the second place, VPs were divided according to whether they were affirmative, interrogative, negative or negative-interrogative. For the affirmative category a distinction was made between "simple" VPs, "complex" VPs and VPs containing 'do'. In this case "complex" VPs means VPs where X = M, 'have' or 'be', as well as a handful of minor forms, but not 'do'. Interrogative, negative and negative-interrogative VPs were divided according to whether X = V (e.g. "came he?"), with a subcategory V = 'have' or 'be' (e.g. "is he here"), X = 'do' (e.g. "did he come?") or X = M, 'have' or 'be' (e.g. "can he come?", "has he come?", etc.). It was felt that this framework would provide a more detailed and revealing insight into the factors that

favoured the development of do-support than a simple count of forms containing 'do' and those without 'do'.

In the previous section I have offered the hypothesis that present-day English do-support can be explained as a mechanism to preserve SVO order in all cases, so that deep and surface structure coincide practically throughout the grammar. In section 3 we have seen the maintenance of SVO order as the input which exerts pressure on the do-support rules to become obligatory, but from a purely diachronic point of view, we might just as well regard this as the output of other pressures favouring the increasing use and eventual obligatoriness of do-support. It is to this aspect of the problem that I should now like to turn my attention.

A full projection of the statistical data would be out of place in this paper, and in any case occupy far more space than would be appropriate in a general survey of this kind. I shall therefore limit myself to illustrating the most significant results.

a) Chronology seems to play no part in the relative frequency of simple VPs as opposed to complex VPs: for the prose texts the maximum incidence of simple VPs was 73'3% (Earle) and minimum incidence 49'4% (Halifax - an exceptionally low count), with an average value of 63'2%, which is very near the actual "score" obtained by Malory (64'3%) and Bunyan (64'6%), and not far from that of Defoe (65'2% (on the one hand and Paston (60'6%) and Osborne (60'4%) on the other. For the dramatic texts the values vary between a maximum of 71'2% (*Second Shepherd's Play*) and a minimum of 53% (Brome), with a mean average of 61'2%. There are therefore no grounds for supposing that texts closer to the spoken language (the language of drama) contain a significantly lower number of complex VPs, the 2% difference in the mean average values being statistically insignificant. But we can also consider these figures in a different light: nearly 40% of all finite verbal forms have a complex structure, a structure which by definition rules out recourse to do-support.

b) It is perhaps hardly surprising that the great majority of all propositions consisted of declarative (affirmative) sentences⁷. This prevalence of affirmative propositions over other types is slightly more marked in the prose texts than in the dramatic texts, with a maximum value of 95'9% in Swift and a minimum value of 86'3% in Defoe, whereas for drama the corresponding figures are 91% (*Second Shepherd's Play*) and 80'9% (Udall), but it is worth pointing out not only that the difference between the mean averages between the two groups of texts (91% and 85'3% respectively) is still less than 6%, but that in both cases the maximum and minimum values are found in texts which are chronologically very close, or even by actual contemporaries. Negative and interrogative propositions therefore (negative-interrogatives can safely be disregarded as being statistically insignificant) play a very minor role in the organization of written discourse, and I would venture to guess that these figures are probably pretty representative. It would certainly be interesting to know what the figures are for the spoken language in contemporary English, but to the best of my knowledge the question has not been investigated⁸. What this would suggest is that there may well be a strong pressure on the minority

categories to conform to the structural characteristics of the category that comprises the great majority of propositions, provided of course that the necessary semantic distinctions (declarative vs. interrogative vs. negative) can be preserved.

c) The incidence of 'do' forms with all kinds of propositions is never very high. Comparison between Ellegård's findings and my own very much more limited statistics are possible in this case. However, Ellegård not only disregards texts with no occurrence of do VPs, a decidedly arbitrary procedure, but what is more fails to take into account complex VPs, which as we have seen make up nearly 40% of all finite verbal forms: to take as a basis of comparison merely phrases in which X = V as opposed to phrases in which X = 'do', without considering the typology X = M/'have'/'be' seems to me entirely unjustified. "I did come" may well be, in a certain sense, synonymous with "I came" (we shall return to this shortly), but syntactically it is closer to "I have come" or "I could come", and the burden of my argument in this section is that it is this structural similarity that favours the adoption of 'do' periphrases.

According to Ellegård's statistics (p. 159) there is a maximum incidence of 10'8% of 'do' forms in all types of propositions during the period 1550-1575, whereas only fifty years previously (1500-1525) it was no more than 1'8%. From 1575 onwards there was a gradual decline to 6'2% during the period 1650-1700. My own count, which, as has been pointed out, does not conflate the figures of different texts reveals a very much more varied movement: for prose the maximum number of 'do' phrases is 14'4%, found in Pepys (1661), but this is almost certainly due to certain stylistic characteristics of this author, since Browne, some fifteen years before, has less than half this figure (6'6%), and Dorothy Osborne, writing in 1652-53, only 4'6%. The percentages are plotted in Figs. 1 and 2 for the prose texts and for plays respectively, but unfortunately the different criteria adopted by Ellegård and myself did not permit me to superimpose one graph on the other. The figures do however show the different percentages obtained if we take as a basis of comparison only simple VPs or all kinds of VPs.

What is significant is not so much a three or four point percentage increase or decrease, but the comparatively modest number of VPs in which 'do' is found throughout the period. The broken line in Figs. 1 and 2, which shows 'do' VPs as a percentage of *all* kinds of finite verb forms, reveals the even more marginal part played by 'do' VPs in the structure of the verb phrase at the time.

d) Let us now turn our attention to the oft repeated assertion that do-support in declarative sentences showed a steep rise during the 16th century and declined to figures pretty near zero by about 1700. Here too 'do' VPs seem to play a very modest part in the structure of the affirmative proposition: according to Ellegård's figures affirmative 'do' never exceeded 10% of all affirmative propositions, a peak reached about 1550, and since at that period do-support in interrogative and negative propositions was still very infrequent, this figure is pretty close to the total number of 'do' VPs counted by Ellegård. Here too my figures, not being mean

averages, show considerable variation, which will become clear from a glance at Figs. 3 and 4.

In a general survey like this it would be impossible to go into the vexed question of the exact semantic valency of these 'do' propositions, since only a very detailed study of all the possible contextual factors and of the verbs that seem particularly to attract do-support could provide us with an answer that goes beyond the little better than impressionistic – but I believe substantially correct – conclusion that, as has already been suggested, in many cases it would be preferable to talk of the 'do' forms in terms of markedness, as opposed to unmarked simple forms. But this is a subject that certainly requires further investigation.

Ellegård (pp. 166-69) discusses the question of whether the rise of do-support is to be associated with a literary or formal rather than with a colloquial style, and comes to the conclusion that at the outset it was "*chiefly* a feature of the literary language" (Ellegård 1953:169, his italics), and that in particular in declarative sentences do-support continues to characterize more formal styles throughout the 16th and 17th centuries, although his reference to "colloquial speakers" is somewhat infelicitous, for we can draw no conclusions about the speech habits of a particular group of *speakers*, but only about *texts*.

Rissanen 1983, working on mid and late 17th century American texts reporting the spoken language, which may therefore in some measure be presumed to represent a form of transcription of actual speech, comes to exactly the opposite conclusion, namely that while 'do' VPs were tending to decrease rapidly in written texts, they ones with reports of meetings conducted by the same minister, he finds that in the former, more literary texts only 7% of all affirmative propositions have do-support, whereas in the latter, presumably more colloquial group of texts, the figure is 18%.

From the empirical evidence outlined above it would certainly be absurd to maintain that affirmative do-support ever played more than a marginal part in the structure of declarative sentences in English, even though in a great many grammars of the 16th and 17th centuries forms like "I do come" are given as simple alternatives of "I come", e.g. Bullokar 1586, Greaves 1594, Butler 1633, Poole 1646; it was Wallis 1653 who first considered 'do' in declarative sentences as "emphatic" and in this he was followed by Miège 1688, and most early 18th century grammars, which directly or indirectly derive mainly from Wallis. The early grammarians were probably right, but in free variation clearly does not mean that they were used with the same frequency and above all that there were no contextual constraints on the choice of one form or the other¹⁰.

But unless we are prepared to reject out of hand the basic assumptions of a generative theory of grammar, the fact that in the surface structures we find only a very modest number of 'do' VPs in no way contradicts Rule 5 as postulated in the previous section: the presence of 'do' in an underlying structure is by no means

conditional upon the statistical corpus-based evidence we have been concerned with in the present section, since the two types of analysis operate on a completely different level, each of which, I would submit, is valid within its own terms. What is relevant in a transformational analysis is not so much the actual presence of a form as its *potential* presence and above all its *explanatory* power. An examination both of the structures of present-day English and of 16th and 17th century texts allows us to postulate an underlying structure

$$\text{NP} + \text{tn} + \left\{ \begin{array}{l} \text{do} \\ (\text{M}) + (\text{have}) + (\text{be}) \end{array} \right. + \text{V}$$

present since the 16th century, the linguistic innovation being the addition of the feature 'do' in the VP. If this hypothesis is accepted, that is, if it is considered inherently probable, all we have to do is to specify the constraints that result in the cancellation of 'do' in the surface structure, constraints which admittedly operated in the great majority of cases in the 16th and 17th centuries and in almost all cases in present-day English, but which nevertheless permit us to account not only for the presence of 'do' in interrogative and negative sentences, but also for its recovery in so-called "emphatic" declarative sentences.

e) We have seen in section 4b) that non-declarative sentences occupy at the most just over 19% of the corpus, with a mean average incidence of under 12% of the total. According to Ellegård's statistics, interrogative 'do' proposition already account for 50% of all cases around 1550, a date at which only 30% of all negative propositions use do-support. Negative propositions with do-support increase sharply after about 1620, but by 1700 have not caught up with the analogous structure for questions. But as we have seen, Ellegård does not take into account complex VPs, which, as I have maintained above, contribute to exerting pressure on these propositions to substitute rule $X = \text{do}$, i.e. to use do-support, in all cases where the optional element of aux (M, 'have', 'be') is void. Figs. 5-8 illustrate in the form of graphs the statistical evidence provided by my corpus. Unfortunately, as was of course only to be expected, interrogative propositions are rare in the prose texts and in some cases estirely lacking, but the graohs, in spite of a considerable up-and-down movement, due no boubt to stylistic factors, clearly illustrate two features: not only the gradual decline of do-less forms and the corresponding increase of forms containing do-support, a fact which hardly needed demonstrating, but also more interestingly, that the curve plotting complex VPs for these propositions, i.e. where at least one optional member of aux is present, runs in practically all cases above that, and in some cases well above that of the curves plotting the other types of interrogative and negative phrases structures. This fact is surely not without significance and supports the hypothesis of the "pressure" exercised by complex VPs which has been put forward above.

5. Why should these developments have taken place in English but not in other languages which have similar structures, and in particular in German?¹¹ Hausmann 1974 addresses himself to this problem, and in particular discusses certain uses of *tun* in German dialects, but rejects, convincingly, any hypothesis that a do-periphrasis was in any way a feature already present in proto-West Germanic. All the examples cited by Hausmann, as well as his sources, regard dialect uses, and in standard German the use of *tun*, such as it is, is certainly in no way comparable to the way do-support developed in English. To speak of a deep structure rule in which 'do' is present as an alternative to M, 'have' or 'be' in the aux component of S, as I have done above, may seem hazardous, or at any rate purely speculative, but to do the same for German *tun* would be frankly absurd. Whereas the structure of aux in English and German shows certain similarities (M and 'have/habe'), in other particulars the two languages diverge considerably (e.g. German has, notoriously, no progressive, i.e. *sein* — V_2 is a non-permissible sequence, and the passive transformation is obtained by *werden*, the cognate of OE *weorþan*, a verb that has now been replaced by "become"), so that it would not be right to speak of exactly parallel developments, and consequently expect an analogous output, where there is a decidedly different input.

A much more pertinent question does not seem to have attracted sufficient attention in the literature. Hausmann 1974:170 speaks (in relation to interrogative propositions) of a tense attachment rule, in my notation $\text{tn} + X$, which was at first optional and later became obligatory. This seems to me more than a mere verbal infelicity, since $\text{tn} + V$ vs. $\text{tn} + \text{do}$ was not an *optional*, but an *alternative* rule. An optional rule is one that can either be applied or not applied, like the insertion of M in aux, but in this case either one or the other rule *must* be applied. What is remarkable is that the two rules co-existed for such a long period of time, a fact amply demonstrated by the statistical evidence given in the previous section and by the illustrative graphs. One could of course maintain that merely statistical evidence gives an inadequate explanation of the different contexts in which the rules apply, so that in theory we might formulate the linguistic innovation (do-support in questions and negations) in the following way: $A \text{ — } B/X$, subsequently $A \text{ — } B/X, Y$, etc., where A represents a do-less rule, B a do-support rule and X, Y, etc. the contexts in which the rules are applied, in which case we would have a cumulative rule addition process. To some extent this formulation of the mechanism whereby the change in question came about is borne out by the empirical evidence, for example by the zig-zagging lines in the graphs representing do-less forms vs. forms containing do-support, an up-and-down movement which must in large part be attributed to stylistic variations in the different texts. But this does not explain why in a certain text we should have X% of do-less forms and Y% of do-support forms. In a great many cases the two forms, in similar contexts, simply exist side by side. Let us take a few examples from our corpus:

- a) Greene: whereas in I, i, 1. 103 "What say you to this Master Burden? Doth he not touch you?" we can attribute the application of different rules to the fact that the two questions belong to different categories, in the following example (III, ii, 11. 238-39)
- "What, scoff'st thou at a king?
What, doest thou taunt us with thy peasants' fare?"
- the contextual restraints in both questions are exactly the same.
- b) Middleton (III, iv, 11. 13-14).
- "Dampit: Do you use to go to bed so early, Audrey?
Audrey: Call you this early, Master Dampit?"
- a context which hardly seems to "justify" the application of two contrasting rules.
- c) Even towards the end of the 17th century in Etherege (I, 1.163 and 1.166) we read:
- "When did you see your *pis aller*, as you call her, Mrs. Loveit?:...
how stand affairs with you?"
- d) One last example from Defoe, i.e. as late as 1722, with the verb "know", with which do-support (especially in negative propositions) seems particularly slow in asserting itself; forms like "I know not" are common not only in Defoe but throughout the 18th century and they have a distinctly conventional ring about them. On the same page we read:
- "I knew not what to make of it... yet was frightened heartily before, and did not know what I might be charged with."

The very fact that alternative rules exist side by side for such a long period raises interesting theoretical problems, but these examples of the application of contrasting or alternative rules come from the same texts and may therefore be said, in a rather vague way, to represent the same idiolect at a given point of time: they are not merely contemporary uses of alternative rules, but, as it were, the same speaker (or more precisely writer) now applying one, now another, and therefore require us to find some kind of explanation for this type of "optional rule substitution". That an individual's speech is far from homogenous and varies not only in time, but according to the social context he finds himself in, the register he employs and the role he assumes, has become a commonplace in contemporary sociolinguistics, ever since Labov's seminal studies on the speech of certain groups of New Yorkers (Labov 1966, but see also Trudgill 1974 for British English and Sornicola 1977 for Sicilian-Italian - to cite just a few examples). Linguistic variety need hardly surprise us - indeed, absolute linguistic homogeneity would be highly suspect - and any form of code switching necessarily implies rule switching. But what our corpus reveals is apparently *unmotivated* rule switching.

Traditional diachronic linguistics has always assumed that all forms of linguistic change was gradual, though exactly what gradual change implied in a

microlinguistic context was not entirely clear: were speakers supposed to have used (or to be using) forms X and Y side by side, with the number of occurrences of Y gradually increasing as compared with the occurrences of X, so that in the end $X > Y$? Recent sociolinguistic studies have thrown a most interesting light on this process and have shown that if $X > Y$, it is because certain social constraints operate on the community of speakers favouring the choice of Y over X, so that in time a *linguistic change* (rule switch) takes place, and it has long been recognized that such changes may be due to upward or to downward movements along the social scale of certain linguistic varieties: a vulgar form may become standard or a high variety decline in prestige and come to be felt as vulgar or uneducated, and the same is of course true of local (dialect) forms.

Transformationalists, as we have seen at the beginning of this paper, look upon linguistic change in a rather different way: they see it as a change in a certain speaker's, or rather set of speakers' competence, a change conceived of as being instantaneous, but which is not necessarily reflected at once on the level of performance. It is significant that this theory draws most of its support from the field of phonology: it seems inherently improbable that over an extended period speakers should continue to use indifferently, and in a manner not conditioned by contextual or sociolinguistic constraints, to take a simple example, forms like [f] and [: f] for "off": in fact we can say that the latter form in Britain today is "old-fashioned" or "conservative", characteristic of a generation born shall we say before 1920 and therefore in the process of being replaced¹², but surely we are unlikely to get the same speaker using alternatively [f] and [: f] as the whim takes him.

Change in syntax may very well be much slower than in phonology, but the evidence at our disposal seems to show that the innovation that resulted in the replacement of $X = V$ by $X = 'do'$ in the structure $tn + X$ in interrogative and negative propositions took something like 150-200 years to establish itself, and perhaps it has not finally established itself yet, for surely many speakers of English would still regard forms like "what say you?" or "I know not" as historical survivals, but fully comprehensible and part of their passive competence of the language, unlike certain Middle English forms, rather than violations of the rules of English grammar, so that to some extent, perhaps to a considerable extent, the co-existence of two contrasting rules must be attributed to stylistic considerations. My deliberate choice of stylistically non-homogeneous texts would certainly account for many, but surely not all the variations shown up by the graphs.

Perhaps we simply do not know enough about all the constraints, both linguistic and extra-linguistic, that allow us to account for this very gradual replacement of one set of forms by another. In our present state of knowledge we must accept it as such and assume that generations of English speakers - or more precisely writers, for, as I have insisted above, we have no direct knowledge of the speech habits of our ancestors - had a dual competence that allowed them to

generate "what say'st thou?" and "what dost thou say?" (or "what do you say?") almost in the same breath.

Notes

1. Just how the historical linguist is to know whether output X is the result of G_1 or of G_2 is not quite clear. But whereas, in the theory, G_1 and G_2 can both have an output X, transformationalists, as well as logicians, would reject the converse, namely that G_1 can have an output X or Y, naturally given the same restraints. Another way of saying this is that a given fact can be explained by more than one theory, but that a given theory can only produce identical facts. Consequently, once X has replaced Y, this must be the output of G_2 (or some other G), but no longer of G_1 .
2. But no transformationalist is likely to be deterred by the argument that a given rule is too "abstract". Much of generative phonology is considerably more abstract than the formulation given for Rule 5.
3. What follows is true of 'have' in its "canonical" meaning of "possess". In its other uses (= "experience", "undertake", etc.: "to have a walk", "to have breakfast") 'have' follows the American usage discussed below.
4. Another example of the peculiar word order associated with 'be' is to be found in the collocation of adverbs like 'all' or 'both', e.g. "they both/all came" vs. "they are both/all here".
5. For a treatment of 'be' in 17th century grammars and linguistic writings and its antecedents in an earlier grammatical tradition, see Frank 1979: 183-90.
6. The samples are taken from 20 prose texts dating from the late 14th century until the early 18th century, and from 12 dramatic texts of the same period. The texts in question are: 1) Sir John Mandeville, *Travels* (1400-25); 2) Margaret Paston, 15 letters to her husband (1461-64); 3) Sir Thomas Malory, *The Death of King Arthur* (before 1470); 4) Roger Ascham, *Toxophilus* (1545); 5) Edward Hall, *The Union of the Two Noble... Families of Lancastre and Yorke* (1548); 6) Sir Philip Sidney, *Arcadia* (approx. 1581); 7) Thomas Deloney, *Jack of Newbury* (1597); 8) *The King James Bible (Authorised Version)* (1611); 9) Sir Walter Raleigh, *The History of the World* (1614); 10) John Earle, *Microcosmographie* (1628); 11) Izaak Walton, *Life of Dr. Donne* (1640); 12) Sir Thomas Browne, *Pseudodoxia Epidemica* (1646); 13) Dorothy Osborne, *Letters to Sir William Temple* (1652-53); 14) Samuel Pepys,

Diary (1661); 15) John Dryden, *Of Heroic Plays* (1670); 16) John Bunyan, *The Pilgrim's Progress* (1678); 17) Aphra Behn, *Oroonoko* (1688); 18) George Savile, Marquess of Halifax, *The Character of a Trimmer* (1688); 19) Jonathan Swift, *The Battle of the Books* (1697-98); 20) Daniel Defoe, *Colonel Jack* (1722). The dramatic works (mostly in prose) are: 1) *The Wakefield Second Shepherd's Play* (mid 15th century); 2) *Everyman* (late 15th century early 16th century); 3) Nicolas Udall, *Ralph Roister Doister* (before 1553); 4) Robert Greene, *Friar Bacon and Friar Bungay* (1589-90); 5) Thomas Middleton, *A Trick to Catch the Old One* (1605); 6) Ben Jonson, *Bartholomew Fair* (1614); 7) Philip Massinger, *A New Way to Pay Old Debts* (1621-25); 8) Richard Brome, *A Mad Couple Well Matched* (1639); 9) John Dryden, *The Wild Gallant* (1663); 10) Sir George Etherege, *The Man of Mode* (1676); 11) William Congreve, *The Way of the World* (1700); 12) George Farquar, *The Beaux' Stratagem* (1707).

The samples of prose comprise in each case between 15 and 25 pages approximately of text, giving counts of between 338 and 1160 finite verb forms. The samples from the dramatic texts were rather more extensive, and ranged from complete plays (*The Second Shepherd's Play* and *Everyman*) to two or three acts from a full-length comedy, producing a range of between 884 and 1740 finite verb forms. It was felt that these samples were sufficiently large to be indicative, but out of a potentially unlimited corpus, the examples chosen clearly represent only an infinitesimal part of the material that might be investigated.

There is some incongruity here: for structural reasons, propositions with negative adverbs other than 'not', e.g. 'never', were counted as affirmatives a category which also included non-negative imperatives.

Other aspects of the structure of the spoken language have been investigated; see for example the study of relative clauses in Quirk 1968.

I presume that like Ellegård he is counting only X = V vs. X = 'do' type structures. Unfortunately the fact that I have to rely partly on memory and partly on an abstract does not enable me to give full consideration to Rissanen's interesting investigations.

10. For the whole problem of how 16th and 17th century grammarians approached the facts of the English language, see Frank 1976.
11. The Romance languages develop rather different systems, a consideration of which would be out of place in this paper, but we might just mention the system of negative reinforcement in French (*ne...pas*) or Italian (*non...niente*), or how in these languages 'do' and 'make' correspond to a single lexical item *faire/fare*.
12. And many of those born before that date may well have adapted or changed their pronunciation to be in line with the fashion.

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(See. TABLES, pp. 22-29)

Fig. 1. Number of do VPs: prose texts

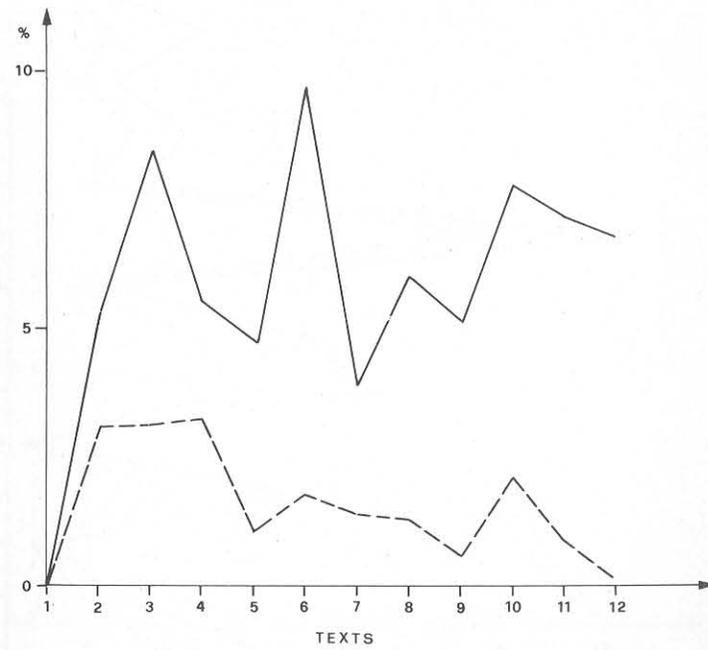
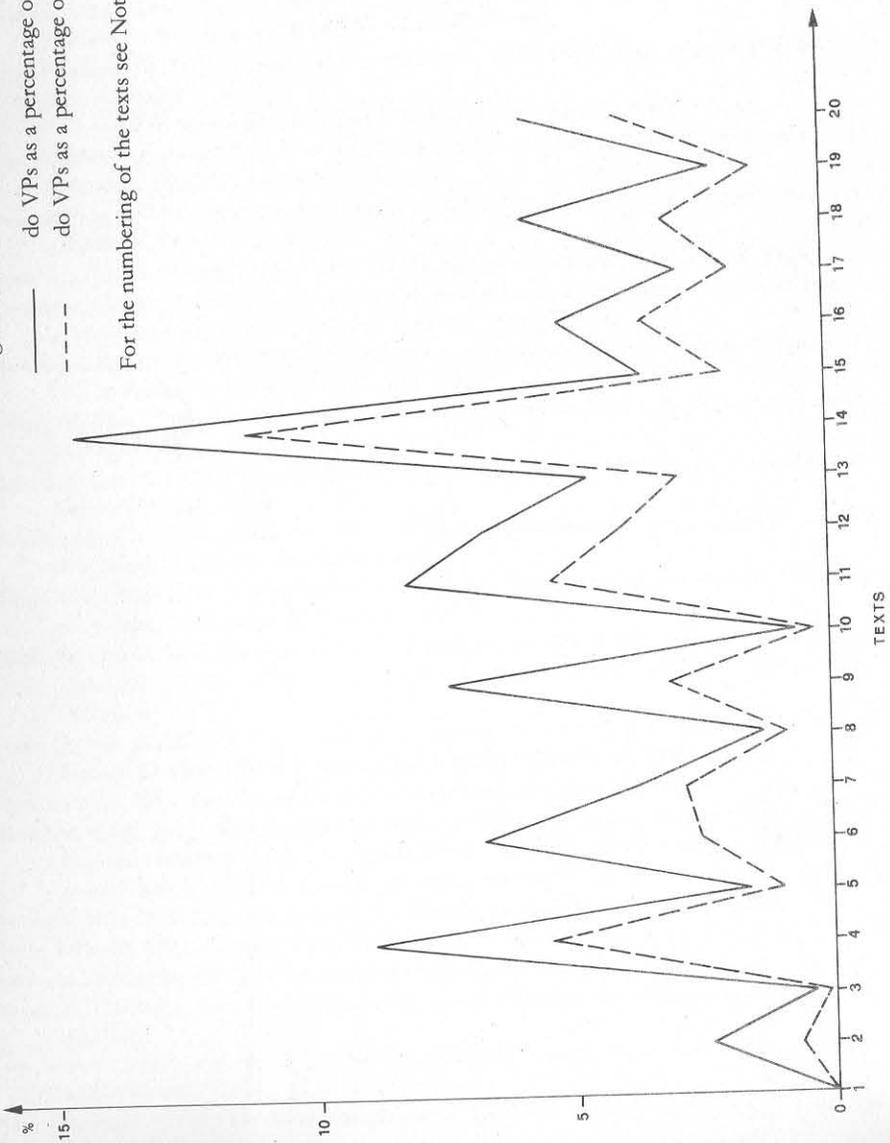


Fig. 2. Number of do VPs: dramatic texts.

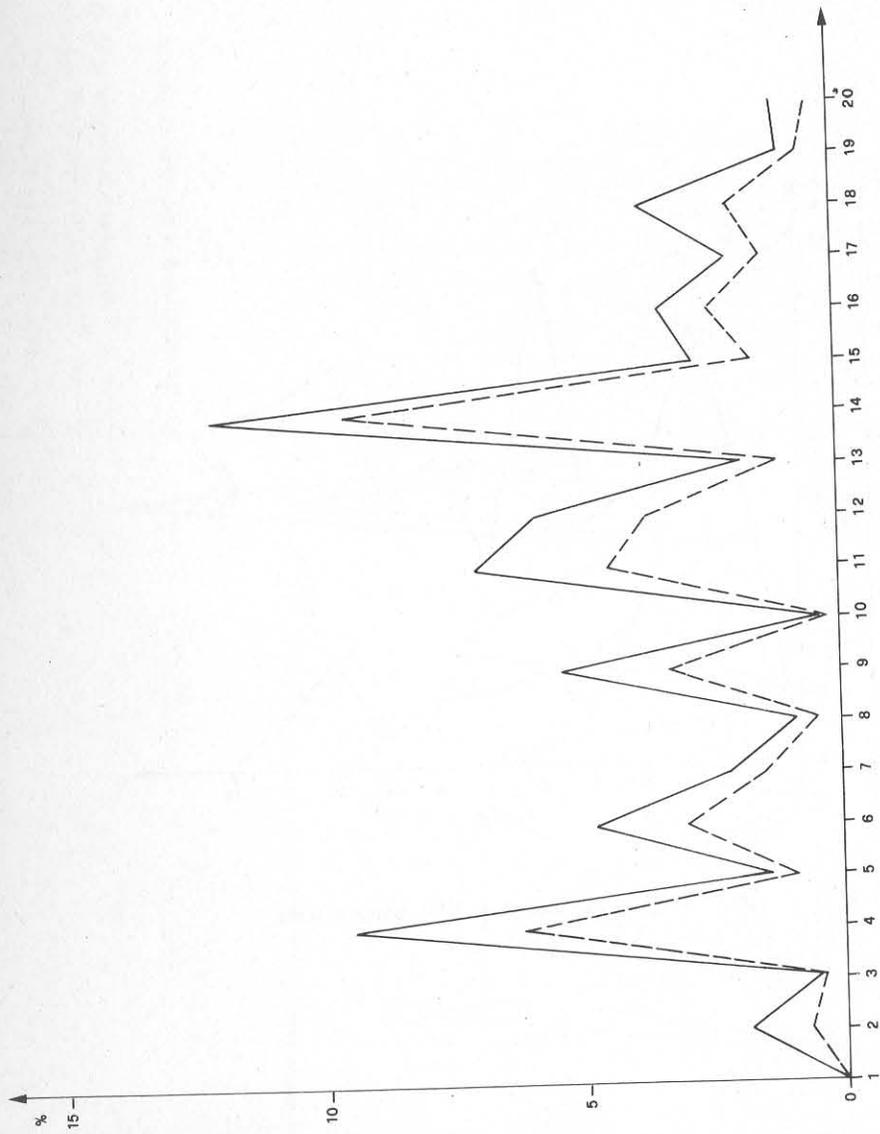


Fig. 3. Do VPs in declarative sentences: prose texts.

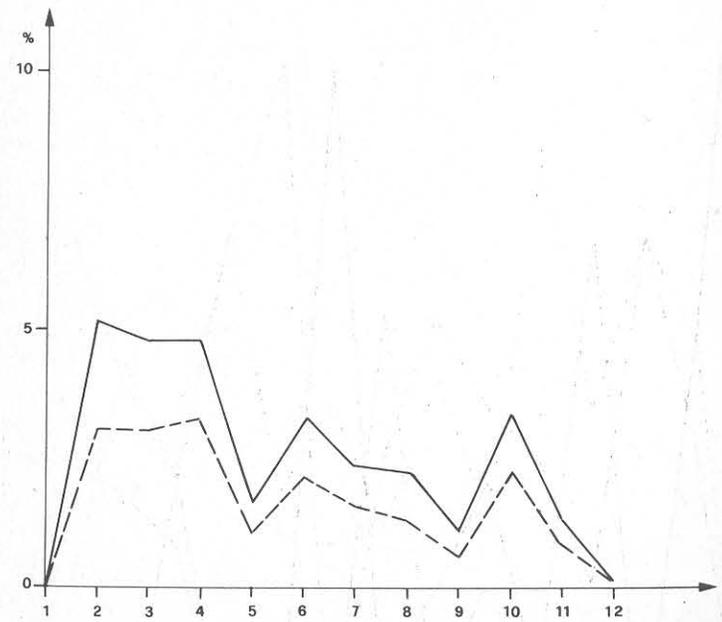


Fig. 4. Do VPs in declarative sentences: dramatic texts.

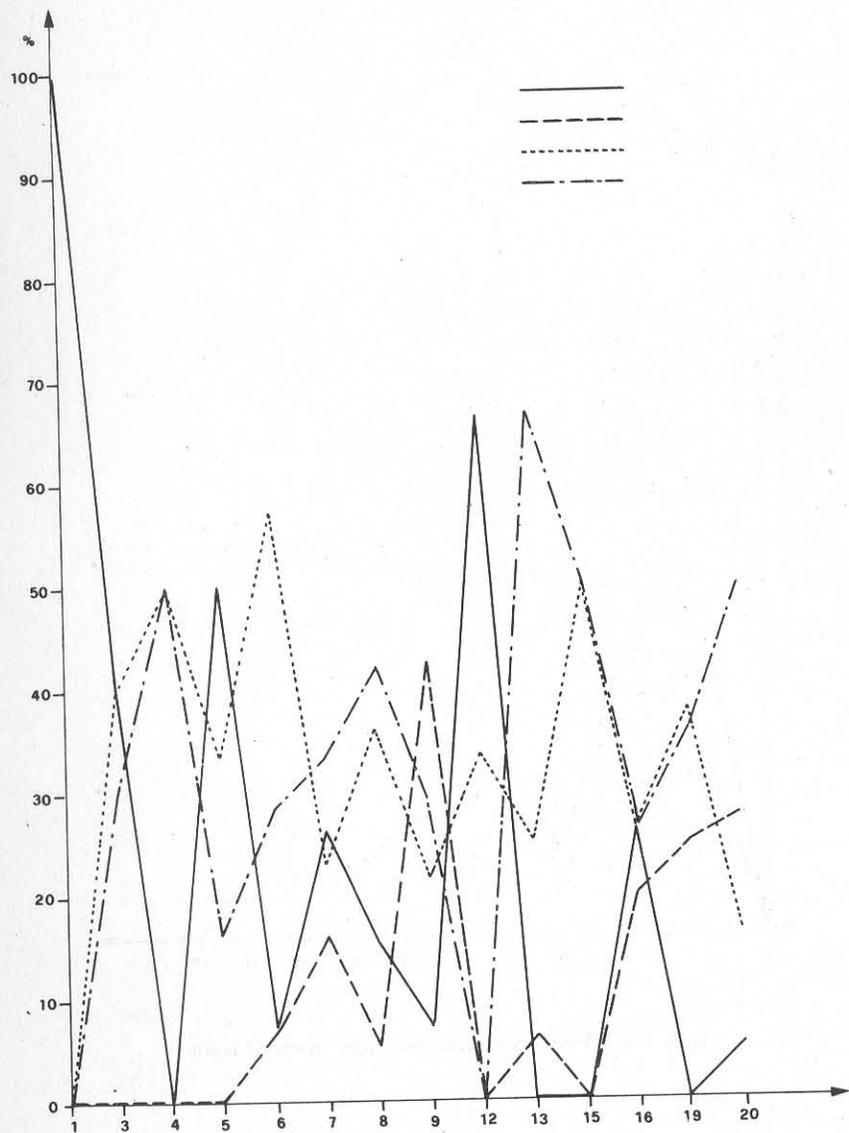


Fig. 5. Interrogative propositions: prose texts

- simple without do-support
- - - with do-support
- 'be' or 'have' as a main verb
- · - · complex VPs

Texts 2, 10, 11, 14, 17 and 18 contain no interrogative propositions.

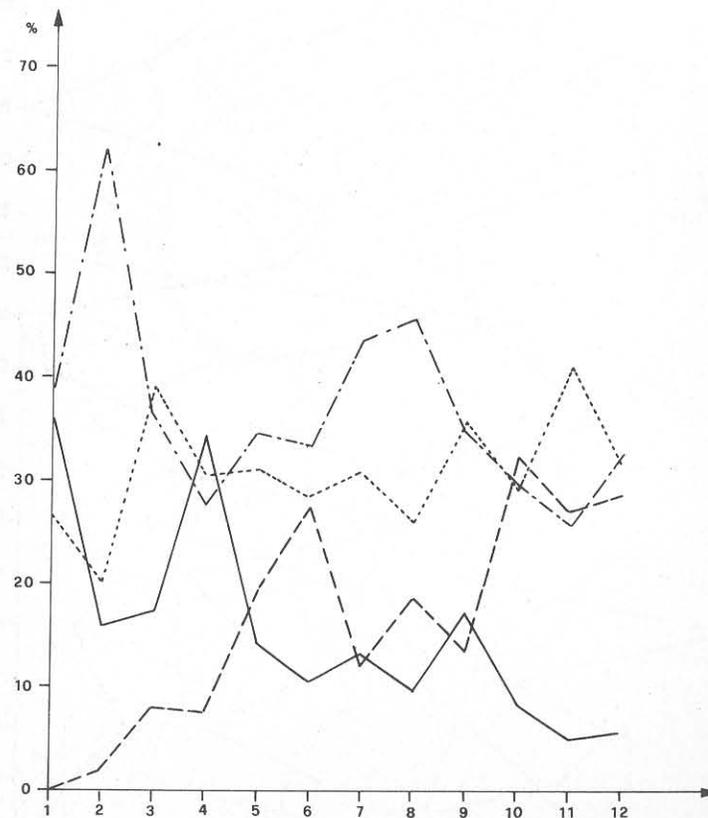


Fig. 6. Interrogative propositions: dramatic texts.

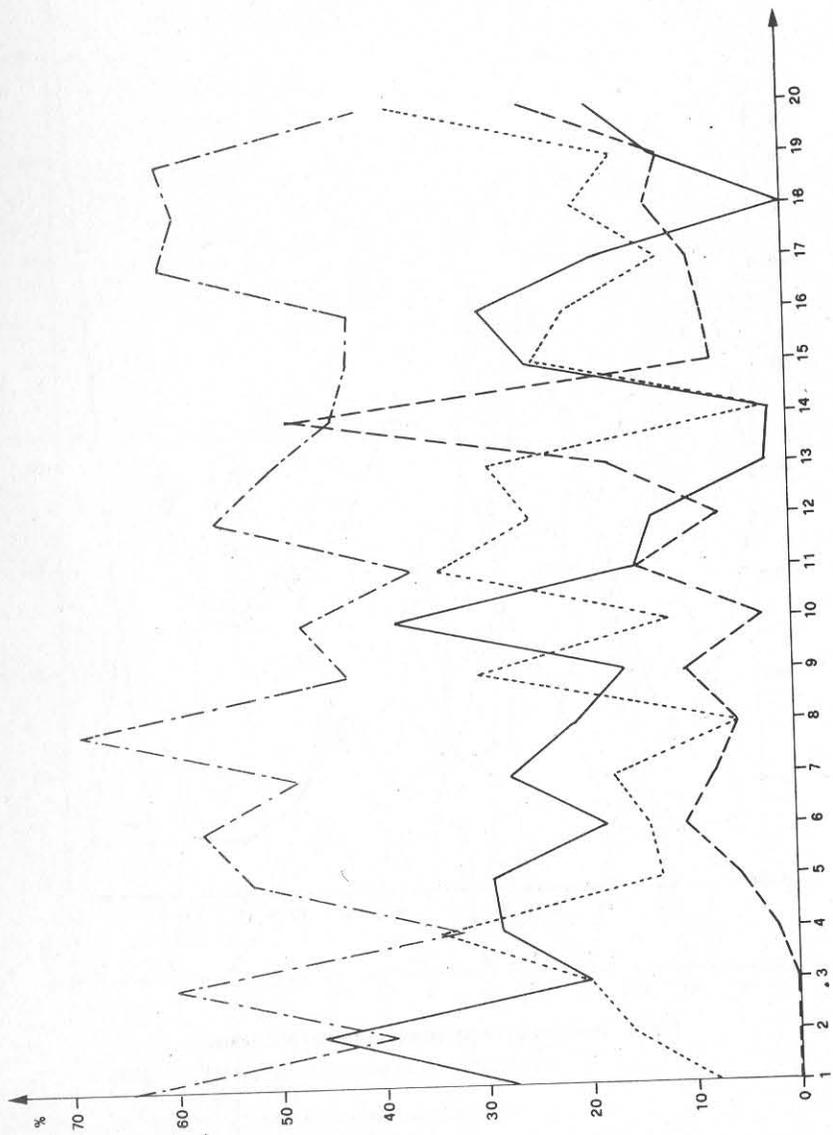


Fig. 7. Negative propositions: prose texts.

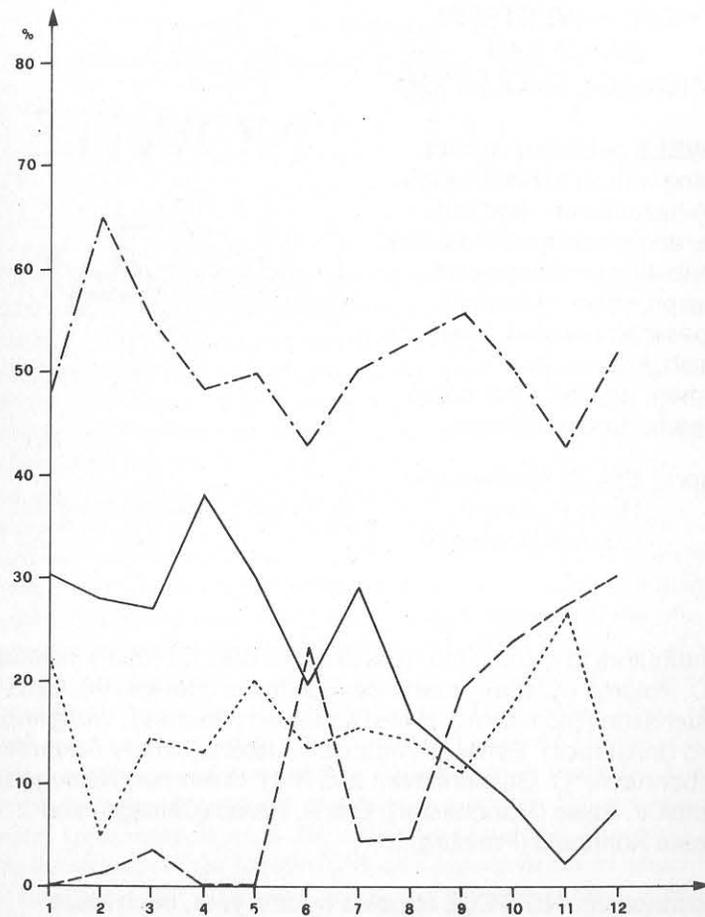


Fig. 8. Negative propositions: dramatic texts.