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Why DO dove: Evidence for register variation in Early Modern English negatives

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ABSTRACT

The development of "supportive" (or "periphrastic") DO in English suffered a curious and sharp reversal late in the 16th century in negative declaratives and questions according to Ellegård's (1953) database, with a recovery late in the following century. This article examines the variation between DO and the full verb in negative declaratives in this database, from 1500 to 1710. It is shown that both register variation and age-grading are relevant, and that the periods 1500–1575 and 1600–1710 have radically distinct properties. The second period shows substantial age-grading, and is interpreted as having introduced a fresh evaluative principle governing register variation. Negative questions supply data that suggest that the development of clitic negation may have been implicated in the development of the new evaluation. This change in evaluation accounts for the apparent reversal in the development of DO, and we can abandon the view that it was a consequence of grammatical restructuring.

In earlier English there was a long period of competition as "periphrastic" (or "supportive") DO replaced the finite verb in questions and in negative declaratives. Before the 14th century only the older form with a simple finite verb was available; this type is shown in (1). After the 19th century, these clause types have their modern form with finite DO, as in (2). In the intervening period, and particularly from 1400 to 1800, both are available, as they were in the 1670s to Thomas Otway, the source of my Early Modern English examples here and below. In negative declaratives like (1b) and (2b) this development shows an abrupt reversal in the late 16th century, when the incidence of DO dropped sharply. Hence the title of this article, which is focused on the interpretation of this sudden drop in incidence.

(1) a. Well, madam, how like you it, madam, ha? (301–13)
b. I question not your friendship in the matter, … (291–23)

I want very sincerely to thank Tony Kroch for giving me an electronic version of the database used by Ellegård (1953), in an act of straightforward generosity; also Ann Taylor for her work in compiling it, and Celeste Tereszczuk who completed this task. I am also grateful to the British Academy who funded a period of research leave which gave me time to investigate this, and to audiences at ICEHL 12 (held at the University of Santiago), at the Linguistics Research Seminar at the University of York, and at NWAVE 32 for their comments.

(2) a. why do I spend my time in tittle-tattle with this idle fellow? (215–8)
b. she does not deserve it, … (317–4)
(examples from Thomas Otway, *The Cheats of Scapin*, 1676/1677; *Friendship in Fashion*, 1678, cited from Otway (1812))

The origin and rise of DO has been much studied. See Rissanen (1999a) and Denison (1993) for recent general accounts of its history, and for bibliography. One of the most important studies is that of Ellegård (1953), both because of its care and sophistication, and because it is based on a large and systematic collection of data. This database is of great value, because it provides us with material for reaching a better understanding of the variation between DO and the finite verb across three centuries of change. In particular, it enabled Kroch (1989) to make his major claim about the Constant Rate Effect, when he demonstrated that the increase in DO up until 1575 showed an unexpected similarity between different surface syntactic contexts, in that the s-curves of change for each context were parallel. Subsequently, Ellegård's database shows different types of questions parting company from each other, and from "negative declaratives," that is, clauses in which sentential negation is expressed by NOT. It also shows the beginning of a decline in the use of unstressed affirmative DO, a type illustrated in (3), which had shown a steady increase up until the middle of the century. Thus in the second half of the 16th century there was a "point of inflection in the curves of change," and Kroch's reasonable hypothesis was "that this point of inflection … corresponds to a major reanalysis of the English auxiliary system" (1989:223–224).

(3) Why you must know, Frank, having a particular esteem for my family, (the nearest relation of which I would go fifty miles to see hanged) I do think her a very a – But no more, — mum, dear heart, mum, I say. (331–35) [The truncated word is 'adulteress' – AW]

The development of negative declaratives in Ellegård's database is rather striking. This clause type shows a dramatic collapse in the level of DO in the last quarter of the 16th century (the period of Kroch's "major reanalysis"), and an uneven recovery in the 17th century. So the relative levels of DO in negative declaratives and affirmative yes–no and adverbial questions differ sharply at different periods. Before 1575 negative declaratives were 25 years behind such affirmative questions in their adoption of DO. After 1600 they lag by over a century. The transition involves a very rapid decline in the proportion of DO in negative declaratives from 38% in 1550–1575 to 24% in 1575–1600 (Ellegård, 1953:161ff.). A similar decline in negative declaratives is attested in The Corpus of Early English Correspondence (Nurmi, 1999), and negative questions also show a decline in Ellegård's database, from 85% DO in 1550–1575 to 65% DO in 1575–1600.

A rather natural approach to this rapid decline of DO in negative declaratives and questions would be to seek to interpret it as a consequence of the more general change, which underlies Kroch's "point of inflection." If successful this would give a single account for multiple phenomena, in line with general scientific

practice and the methodology advocated in Lightfoot (1979). Indeed, in his 1989 article Kroch proposed an explanation of just this type. But there is also a further possibility that remains to be investigated: Could the decline be a consequence of sociolinguistic factors? In this article I will use Ellegård's database to investigate this possibility. The method will have to be somewhat indirect, because Ellegård did not pay attention to sociolinguistic categories when he collected his data, and it is therefore not possible to use such categories as gender or class. But the date of birth of many of Ellegård's authors is known, so that the age profiling of DO is open to investigation. The internal stylistic properties of texts can also be established, and the possibility of register variation examined. Between them, these properties of Ellegård's texts are sufficient to show that a dramatic sociolinguistic change took place between the mid-16th century and the 17th century. I will show that both the drop in DO in negative declaratives and its subsequent continuing low level can be explained in large part not as a grammatical phenomenon but as a sociolinguistic one. My data are a reconstitution of Ellegård's database of English plays and prose from 1500 to 1710, which I owe to Tony Kroch (see initial acknowledgments). My conclusions here leave intact the position that there was a grammatical change in the English auxiliary system in the latter half of the 16th century, though they remove one of the pieces of evidence from which one might have wanted to argue that position.

First, then, I will set up a stylistic ranking of Ellegård's texts and will examine the distribution of DO in negative declaratives from this perspective, showing that the data argue for the conclusion given in the preceding paragraph. Second, I will argue that the distribution of DO in relation to the age of each text's author points strongly in the same direction. Then I will lay out the data for negative questions, and show that these data suggest that the concurrent cliticization of NOT to auxiliaries may be implicated in this sociolinguistic change. Finally, I have some reflections on the consequences of my arguments for interpretations of the syntactic history of DO.

INTERNAL STYLISTIC DIFFERENCES BETWEEN TEXTS

A scale of lexical complexity

Ellegård's database was not selected with social structuring in mind. He did not pay attention to class or gender in compiling it. His informants are virtually all men who have sufficient education to be literate. It is not therefore possible to treat social variables directly, as has been done in work on the Corpus of Early English Correspondence. But it is possible to look at the internal properties of texts. There has been a range of approaches to the characterization of stylistic variation, and work on style has examined a variety of properties of texts (for some recent discussion see Eckert & Rickford, 2001). An important approach is found in the work of Biber and his associates, which depends on an assessment of cooccurring linguistic properties. Two factors that seem likely to be illuminating,

TABLE 1. *DO in negative declaratives 1600–1710*

Factor	Coefficient	Probability
Lexical complexity	−0.29	$p < .0001$

$N = 952$. This regression omitted Ellegård's "*know* group" of verbs. If the factor *lexical complexity* is omitted, the LogLikelihood difference yields $\chi^2 = 37.8$, $df = 1$. Transitivity and date (continuous) were also present in the regression and found to be significant.

given that Ellegård's texts include a range of types of published literature, are the type–token ratio and the average word length of each of his texts. Investigation shows that each of these properties separately correlates with the incidence of DO in negative declaratives, and that they correlate well with each other. They also form a component of Biber's most important dimension of textual variation, indicating the extent to which each text shows "high informational density and exact informational content" (1988:108), to borrow part of his characterization of this dimension. Together they should provide a robust measure of the lexical complexity of texts. So scales for each of these properties were established,[1] then normalized and summed, following Biber's procedure, to give a single scale of lexical complexity. The issue of constructing an appropriate measure of style will be returned to later after some results have been presented.

DO and lexical complexity in the period 1600–1710

When tokens are coded for two properties of texts, lexical complexity and date, both treated as continuous variables, then, in a logistic regression for the period 1600–1710, lexical complexity is very highly significant, see Table 1.[2] The dependent variable here is the choice between DO and a finite nonauxiliary verb, as in (1) and (2).[3] The coefficient is negative, which means that DO in negative declaratives is more frequent in lexically less complex texts, and less frequent in lexically more complex texts. Thus there is less DO in "higher" registers. For example, Congreve's play of 1693 *The Old Batchelor* has 89% DO in negative declaratives, but his novel *Incognita* (published the preceding year) has 77% DO. Some twenty years earlier, Otway's plays have 66% DO, Bunyan's novel, *Pilgrim's Progress* has 36% DO, and Dyden's late essays have 26% DO. These were all written within a few years of one another.

It is also important to look systematically at the individual texts. Here, it is helpful to set date aside as a variable impacting on the analysis. This can neatly be done by using the rate of change for the period 1600–1710 to estimate what the proportion of DO in negative declaratives for each text would have been in some base year.[4] This calculation has the effect of moving the text along the s-curve of change to the selected year. So, Beaumont and Fletcher's play

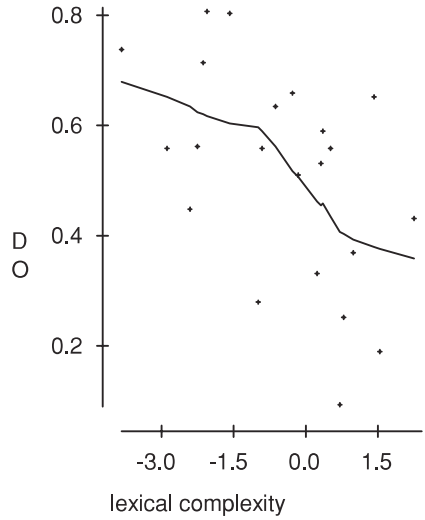

FIGURE 1. Proportion of DO for 1655 versus lexical complexity, 1600–1710.

The Knight of the Burning Pestle has 64.7% DO in negative declaratives. It was first performed in 1607, and it is possible to work out that the corresponding percentage for 1655 is 81.0%. Essentially this says that if the text had been written under the same conditions at the later date, it would have had 81.0% DO. Similarly, Congreve's play *The Old Bachelor* has 89% DO in 1693; it is on a distinct but very close s-curve, with a corresponding figure for 1655 of 80.4%. When corresponding figures are calculated for all the texts in the period, we can compare the results with respect to stylistic differentiation without interference from date.

Figure 1 is a scatterplot for the 22 texts occurring in the period 1600–1710, with proportions of DO estimated for the year 1655.[5] The line is a running average to help you see the trend.[6] You can see that the trend is sharply down from left to right. Three other points are relevant here. First, the linear regression of DO against the scale of lexical complexity is significant ($p < .01$). Second, if we present the figures underlying the scatterplot of Figure 1 in four quadrants, dividing at the mid point, the median, of DO% (56%) and of the scale of lexical complexity (-0.2), we find that most of the texts occur in two quadrants: low complexity and high DO; high complexity and low DO; see Table 2. Third, we can look at the averages for texts of low and high lexical complexity to get some real idea of the scale of this difference. The average proportion of DO in the 11 lexically less complex texts is 62%, whereas in the 11 lexically more complex texts it is 41%. The contrast is striking. Overall, the lexically less complex texts show 50% more DO than the lexically more complex texts. These three sets of figures for individual texts strikingly confirm the results of the overall logistic

TABLE 2. *Occurrence of DO in texts of low versus high lexical complexity 1600–1710*

	Texts of low lexical complexity	Texts of high lexical complexity	Total
High DO%	8	3	11
Low DO%	3	8	11
Total	11	11	22
DO% average	61.6%	41.1%	

Cells with a high incidence of texts are shaded.

TABLE 3. *DO in negative declaratives 1500–1575*

Factor	Coefficient	Probability
Lexical complexity	+0.18	$p < .0001$

$N = 2244$. This regression omitted Ellegård's "*know* group" of verbs. If the factor *lexical complexity* is omitted, the LogLikelihood difference yields $\chi^2 = 35.3$, $df = 1$. Transitivity and date (continuous) were also present in the regression and found to be significant.

regression given in Table 1, and show that the proportion of DO is higher in lexically less complex texts in Ellegård's database for the period 1600–1710.

DO and lexical complexity in the period 1500–1575

The situation is, however, interestingly different in the period 1500–1575. Here the results for lexical complexity in a logistic regression, including date and transitivity, show that this factor is very highly significant ($p < .0001$) (see Table 3). But now the coefficient is positive, that is, in negative declaratives, there is more DO in texts of higher lexical complexity, and less DO in texts of lower lexical complexity. So we have a striking difference between the periods 1500–1575 and 1600–1710. The later period apparently reverses the earlier situation, that is, complex lexis is associated with higher levels of DO in the earlier period and with lower levels of DO in the later period.

Figures for individuals also support this conclusion. The same method as outlined earlier was used to set aside the impact of date, but this time 1535 was the base year. Figure 2 is a scatterplot giving estimates of proportions of DO in negative declaratives for that year, plotted against lexical complexity. There is a clear upward trend from left to right, shown in the running average. Three points

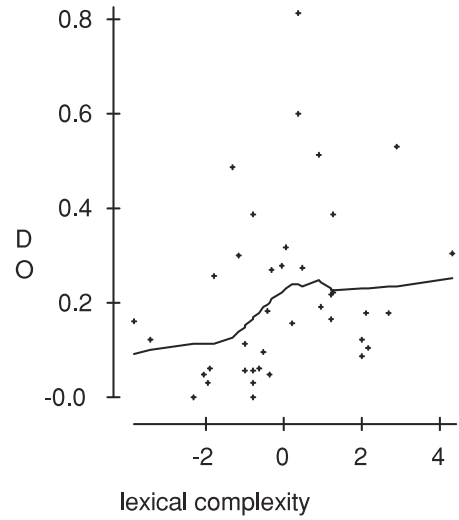

FIGURE 2. Proportion of DO for 1535 versus lexical complexity, 1500–1575.

TABLE 4. *Occurrence of DO in texts of low versus high lexical complexity 1500–1575*

	Texts of low lexical complexity	Texts of high lexical complexity	Total
High DO%	6	14	20
Low DO%	14	5	19
Total	20	19	39
DO% average	14.0%	29.9%	

Cells with a high incidence of texts are shaded.

to parallel those made previously can also be made here. First, the linear regression is significant.[7] Second, if we present the figures in four quadrants, dividing at the mid point, or median, of each scale, as before, we find that most of the data occur in two quadrants: low complexity and low DO%; high complexity and high DO%; see Table 4. Third, comparing the averages is again interesting. The average percentage of DO for the lexically less complex texts is 14%, whereas for the lexically more complex texts it is 30%. There is twice as much DO in lexically more complex texts. So there is also a considerable difference here. As before, we have mutually supportive results from both the logistic regression on individual tokens, and from the figures for individual texts, showing that the incidence of

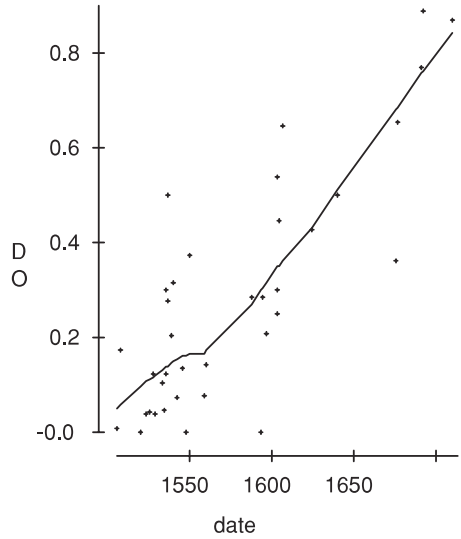

FIGURE 3. DO versus date in texts of lower lexical complexity.

DO in Ellegård's database is higher in texts of higher lexical complexity at this earlier period.

DO and lexical complexity in negative declaratives in the period 1500–1710

We can put this together by looking at graphs of change for the whole period 1500–1710, presented in Figures 3 and 4. Here texts have been divided into two groups of equal size by cutting the scale of lexical complexity at the median. In one group there are 39 texts of high lexical complexity; in the other, 38 texts of low lexical complexity. In each case, the value of DO for the text is the proportion of DO in negative declarative sentences, and (as before) a running average has been added to help you discriminate the major trends.

The difference between these graphs is striking. In texts of low lexical complexity there is no sign of the late 16th century collapse seen in Ellegård's overall figures, though there is a gap in the data from 1565–1585, and an earlier blip. Nor can you see the later irregular and rather flat development his figures reveal until the second half of the 17th century. But in texts of high lexical complexity the downward movement is dramatic, and a lower level of DO is maintained across the following century. The next graph, given in Figure 5, simply imposes the two running averages on each other. It shows that there is a difference in the 16th century, and that the situation alters radically as we approach the 17th century, and remains different.

This analysis is supported in general terms by what Nurmi found in the Helsinki Corpus, see Table 5. This shows a similar stylistic switch in the use of DO

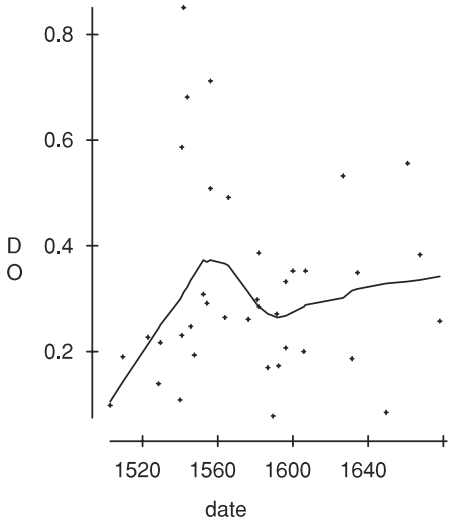

FIGURE 4. DO versus date in texts of higher lexical complexity.

NOT between the period 1500–1570 and the second half of the 17th century.[8] The categories used here (oral versus non-oral) differ, being based on a classification by types of genres. But the situation is clearly shown to be parallel, and it is not clear that there is any difference in timing. There is, however, a difficulty when we consider the evidence of The Corpus of Early English Correspondence. This parallels Ellegård's data in showing an overall decline in the use of DO NOT after a period of increase. But it differs in that the decline is shown in the early decades of the 17th century rather than towards the end of the 16th. I will return later to consideration of this difficulty.

It is clear, then, that in Ellegård's database:

1. A higher rate of DO with NOT is found in texts of high lexical complexity in the period 1500–1575.
2. There is a sharp reversal of this association in the 17th century. In this period a lower rate of DO with NOT is associated with texts of high lexical complexity.
3. The sharp drop in the late 16th century in Ellegård's overall figures may simply reflect this switchover, which essentially depends on the lexically more complex registers.

In texts of lower lexical complexity there seems to be a pretty steady increase across the whole period. This implies that there is likely to have been a relatively steady underlying development of DO NOT in the vernacular, given the reasonable assumption that we can extrapolate meaningfully from the lexically less complex texts in Ellegård's database. The further drop in 1625–1650, which can be seen in Ellegård's figures, does not appear in either graph: It is a result of sampling differences (as Ellegård himself suggested, 1953:163), because Ellegård's

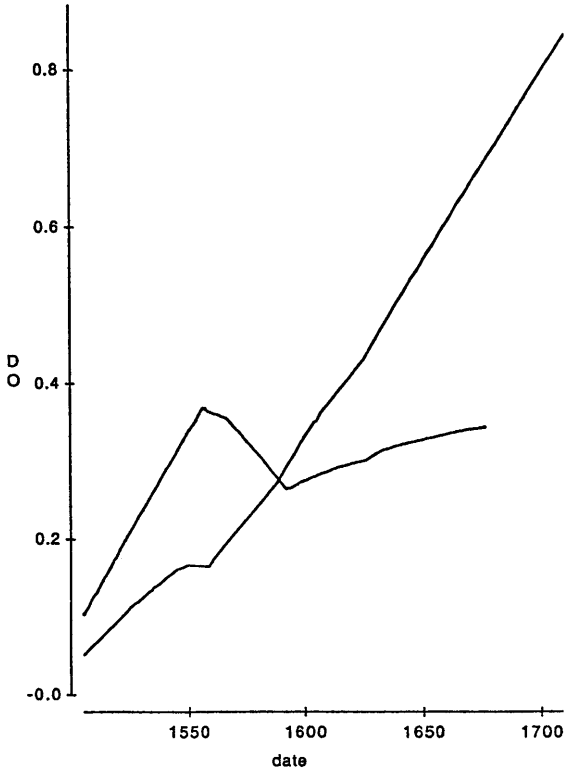

FIGURE 5. Superimposed running averages from Figures 3 and 4.

TABLE 5. *Incidence of DO in negative declaratives in the Helsinki Corpus*

Helsinki 1500–1570	Non-oral > Oral
Helsinki 1570–1640	Non-oral = Oral
Helsinki 1640–1710	Non-oral < Oral

Source: Nurmi, 1999:147.

period 1625–1650 has a much higher proportion of lexically more complex texts than the preceding and following periods.

The switchover shown in the graph points to a change in the basis for stylistic (or register) variation of DO with NOT compared to the simple finite with NOT at the end of the 16th century. Whatever were the reasons underpinning the greater proportion of DO NOT in texts of higher lexical complexity in 1500–1575, it is clear that there is a new situation in the 17th century.

The relevance of lexical complexity

Why choose lexical complexity as the measure of style here? I earlier referred to the work of Biber and his associates. Biber set up "dimensions" of variation where he found major groupings of cooccurring linguistic properties. The most important of his dimensions is what he calls Factor 1. He calls it "a very important, fundamental dimension of linguistic variation among texts" (1988:108), and says that it marks the contrast between "high informational density and exact informational content" on the one hand, and "affective, interactional and generalized content"on the other (1988:107). I have defended my measure of style as part of that dimension. Why not then include other properties which might relate directly to the degree of interaction represented in a text, or to the extent to which it reflects oral uses, by looking at the distribution of such characteristics as the use of first and second person pronouns and of questions? I started indeed by doing exactly this, establishing the incidence of such features in samples from Ellegård's texts, and setting up a scale for each feature. Now, since the dimensions established by Biber reflect the fact that a series of features essentially rank texts in the same way as each other, one should be able to see how well this dimension applies to data from earlier English by asking to what extent these features march in step with each other, and rank texts in broadly the same way. For the period 1600–1710, scales for the six features I isolated, which are listed in (4), correlated very well with one another: All 15 pairwise correlations were significant at the $p < .05$ level.

(4) Factor 1 'Informational versus Involved Production'
  More Oral, more Involved:
    Higher level of 1st person pronouns
    Higher level of 2nd person pronouns
    Higher level of yes–no questions
    Higher level of wh-questions
  Less Oral, more Informational:
    Higher type–token ratio
    Higher average word length

This strongly implies that an interpretation in terms of Biber's more general Factor 1 is appropriate, and that this gives us apparent evidence of an oral–literate scale. As before, scales for individual features were normalized and summed, to establish this more broadly based scale. It turned out to be directly relevant to the distribution of DO in the period 1600–1710, where results show a higher incidence of DO in more oral texts. The logistic regression across tokens is highly significant ($p < .0001$), and a series of results for individual texts parallels those given earlier. So there is a very clear correspondence between the frequency of DO in negative declaratives in a particular text, and that text's position in the oral–literate scale, as defined by the features in (4).

In the period 1500–1575, however, the situation was very different. The six features of (4) correlated very poorly with one another. Only two of the 15 pairs

TABLE 6. *DO in negative declaratives 1500–1575*

Factor	Coefficient	Probability
Date (of text)	+4.6	$p < .0001$
Age of author	+0.1	n/s $p > .8$

$N = 1805$. The regression omitted Ellegård's "*know* group" of verbs and authors of unknown date of birth. Transitivity was also present in the regression and found to be significant. Coefficients of date and age are expressed in logit units per century.

drawn from these six had a significant correlation (type–token ratio with average word length; and yes–no questions with wh-questions), and most had very low correlation coefficients. So the evidence that these features can be seen to define an oral–literate scale, or to correspond to Biber's Factor 1 in Ellegård's data for this period, is absent. It seems likely that the reason for this is the restricted nature of the 16th century texts; most crucially, plays are virtually absent before 1575, whereas they are predominant among the oral texts in the later period, and it seems likely that the more oral end of Biber's dimension is inadequately represented in the data. Plainly, then, I cannot set about using the full set of criteria to locate and compare the incidence of DO with any confidence, because the interpretation of the scale for the earlier period is unclear. But it is possible to back off and rely on the two of the features that did correlate well with each other, that yielded a series of differentiated values across the range of texts (unlike questions), and whose conjunction is readily interpretable. Hence, my use of these two characteristics to define a scale of lexical complexity, which has a clear relationship to a wider oral–literate dimension.

AGE-GRADING

Some further evidence that seems supportive of the suggestion that there is a change in the basis for register variation by the beginning of the seventeenth century can be found in age-grading. This also gives us a strong clue as to the nature of the change. Before 1575, the relationship between a writer's age and the incidence of DO in negative declaratives is straightforward. When tokens are coded for the date of texts and the age of the author, and these factors are included in a logistic regression, age is completely nonsignificant. See Table 6, where the coefficient of age is minimal. This looks just like a "communal" change in Labov's (1994) sense, whereby individuals change their usage as they grow older, without giving rise to a distinction of apparent time. The older you are, the more you use DO, in line with the changing community norm.

In the period 1600–1710, however, things are different. Age is a highly significant factor, and its coefficient is negative (see Table 7): The older you are, the

TABLE 7. *DO in negative declaratives 1600–1710*

Factor	Coefficient	Probability
Date (of text)	+1.70	$p < .0001$
Age of author	–5.13	$p < .0001$

$N = 952$. The regression omitted Ellegård's "*know* group" of verbs. The age of all authors is known or reasonably estimated. Transitivity was also present in the regression and found to be significant. Coefficients of date and age are expressed in logit units per century.

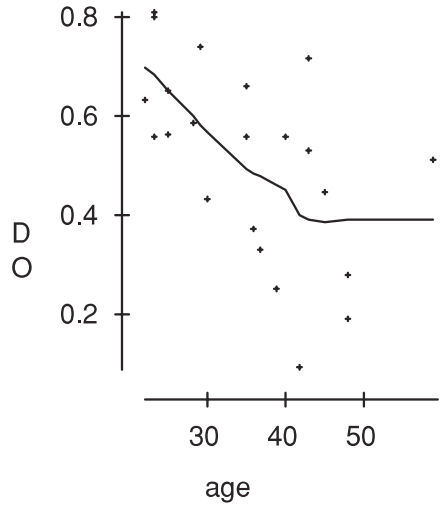

FIGURE 6. DO for 1655 versus age of author. 1600–1710.

less you use DO, although its overall incidence in the sample of users is continuing to increase. This too can usefully be examined by looking at the levels of DO for individuals estimated for 1655 in Figure 6. The scatterplot shows a declining trend, clear from the running average. The levelling out depends on one text with a substantially older author (Breton, publishing in 1604 at age 59, who perhaps has not adopted new norms,[9] or who perhaps shows a reversion to the less carefully monitored behavior patterns of his youth). The linear regression of age against date is significant ($p = .0076$). If the data is split at the medians for age (35.5), and for incidence of DO (56%), the quadrants are as given in Table 8, with a clear contrast, and the average incidence of DO also shows a clear distinction, as you can see in the table.

As Labov (1994:73, 83–84) points out, the distribution across age levels found in age-grading may resemble that resulting from "generational" change, in which

TABLE 8. *Occurrence of DO in texts by younger versus older authors 1600–1710*

	Younger (average age = 26)	Older (average age = 44)	Total
High DO%	10	1	11
Low DO%	1	10	11
Total	11	11	22
DO% average	63.8%	39.0%	

Cells with a high incidence of texts are shaded.

individuals do not alter their usage as they age, so that linguistic change results in a distribution in apparent time, showing different levels of the incoming phenomenon across ages at any chosen point in time. Is what is going on here reasonably interpreted as generational change in this sense? There are two reasons for rejecting such an interpretation. The first is that the estimated rate of decline within the individual for each year of his age is well in excess of the rate of increase within the community for each calendar year. Individuals are not stable, as in generational change, but are losing DO much faster than the general increase. You can see this from the fact that the running average of Figure 6 (where the overall increase has been eliminated from the figures) is not level, but shows a steep decline. You can also see it from the figures in Table 7. Pure generational change should give us coefficients for age and date which sum to zero, so that here the coefficient for age should be −1.7, whereas it is −5.13. So an account involving generational change by itself would be insufficient, and there is no need to suppose that there is a generational element in a more complex situation. Instead, we seem to have age-grading in the sense of a pattern that repeats stably across generations, combined with change, as, for example, in the case of lenition of (ch) in Panama City (Labov, 1994:94–97).

A second reason for rejecting an interpretation involving an apparent time distribution reflecting stability within the individual is that the development of DO in negative declaratives before 1575 seems to be an instance of communal change, since here age is not a significant factor. Moreover, the same is true of questions, both before 1575 and in the period 1575–1710 (Warner, 2004:238ff.). It would seem very strange if the development of DO was a communal change in these categories, but a generational change in 17th century negative declaratives. Such an analysis would demand an account of the nature of this shift in behavior, and its restriction to one subcategory of DO.

Given, then, that the decline with age found in the period 1600–1710 is indeed an age-grading pattern that repeats across generations, the situation is fully consistent with the development of an evaluative set up that differentiates DO NOT

from the use of the simple finite with NOT. In the 17th century we have a dramatic reversal of the scales for register variation, coupled, importantly, with the introduction of age-grading. As Labov noted, "distributions across age levels might not represent change in the community at all, but instead might represent a characteristic pattern of 'age-grading' that is repeated in every generation … Many well-established sociolinguistic variables exhibit such age-grading, where adolescents and young adults use stigmatized variants more freely than middle-aged speakers, especially when they are being observed." (1994:73). Given this, the facts about Ellegård's data (i.e., the 17th century situation itself and the contrast with the preceding period) are consistent with, and indeed strongly imply a differential evaluation which takes effect from the late 16th century, whether this is a stipulative, hostile evaluation of DO NOT or a positive evaluation of the simple finite with NOT. But either way, it is the onset of this that we see in the sharp drop of DO NOT in Ellegård's database.

The demonstration that the decline in the use of DO NOT shown in Ellegård's database is a consequence of the introduction of a new evaluation of DO versus lexical verb in combination with NOT can shed light on the difference found between the evidence of Ellegård's database and that of the Corpus of Early English Correspondence. This parallels Ellegård's data in showing a decline in the use of DO NOT, after a period of increase. But it differs in that the decline is shown later, in the first four decades of the 17th century (as reported by Nurmi, 1999:148–149, 165ff.). We might, however, reasonably suppose that a new evaluation, being a social psychological fact, will take time to diffuse across social groups and situation types (and may remain variable). The databases being compared here are differently composed. The letter corpus contains personal (including private) letters; as Nurmi noted, "In the Correspondence Corpus the letters are personal, but not necessarily private, business letters and other less private types being included" (1999:61). It also includes writing by women. Ellegård's data after 1600, by contrast, is the language of men, much of it written for publication. It must seem possible that a corpus of writing, like Ellegård's, which is more public, would be the first to show the impact of a new stylistic evaluation and would show it more sharply, and that a different mix of individuals and of ages in the databases might also underlie the timing difference. Further investigation will shed more light on this, and on possible relationships between stylistic evaluation and the age and gender differences noted by Nurmi (which also seem to undergo change in the early 17th century).

DO NOT IN NEGATIVE QUESTIONS AND IMPERATIVES

So far, this article has been concerned entirely with negative declaratives. But the development of periphrastic DO in negative contexts included questions and imperatives, where there was also variation between constructions with DO and a finite lexical verb, as illustrated next.

(5) negative direct questions (with NOT)
  a. did you not hear my lady call you? (327–10)
  b. why comes he not forth? (321–13)
(6) negative imperatives
  a. don't run yourself into danger thus rashly. (320–7)
  b. get into the sack, and stir not, whatever happens; (237–31)

In negative questions Ellegård's figures show a major collapse in the last quarter of the 16th century, which seems to parallel that in negative declaratives. But despite this collapse, compelling evidence for a contrast between texts of higher and lower lexical complexity in negative questions after 1600 is weak. The graphs for negative questions, which parallel Figures 3 and 4, do indeed show a similar configuration: There is a monotonic increase in the running average for texts of lower lexical complexity, whereas texts of higher lexical complexity show a sharp decline in the last quarter of the 16th century, to a lower (but increasing) level in the 17th century. The combined graphs are thus broadly similar to that of Figure 5, though the 17th century differences are less pronounced. But the differences between the incidence of DO in distinct text types are not significant for the period 1600–1710.

A possible reason for the lack of significant contrast is that negative questions occur most commonly in plays and other texts which are clearly "oral" in their general properties, and these are predominantly texts of lower lexical complexity. Texts of higher lexical complexity, which are typically literary prose, may contain few or no negative questions in Ellegård's sample. So, although token numbers in texts of higher lexical complexity are not especially low, they are drawn from a small number of texts. Thus, the contrast is at the mercy of the properties of a few individual texts representing one end of the continuum, which are arguably not typical of the wider distribution outside the sample.[10] At best then, it is possible to suggest that negative questions are consistent with the analysis of this article so far, and may well have partaken of the contrast that we can see in negative declaratives, although straightforward evidence for this is weak.

In negative imperatives there are very few instances of DO until the end of the 16th century, when the incidence of DO rises to meet that of negative declaratives. So there is no parallel to the late 16th century collapse in negative declaratives shown in Ellegård's graph. The possibility of a distinction between texts of lower and higher lexical complexity in the 17th century is hard to evaluate, because the distribution of tokens across texts of higher lexical complexity is even more restricted than with negative questions.

In the case of negative questions, however, there is a further striking distributional fact, which potentially gives us a major clue about the nature of the stylistic contrast or evaluation involved here. This depends on the fact that in questions with personal pronoun subjects there is variation in the position of NOT with DO. Taking examples, as before, from Otway, we find the contrast between (7a), with the order DO + pronoun subject + NOT, and (7b) or (7c), where (7b) has the order DO + NOT + pronoun subject, and (7c) shows the further development to *don't*, which appears as a written form in the 17th century.

TABLE 9. *Variation between Auxiliary + pronoun subject + NOT and Auxiliary + NOT + pronoun subject in negative questions in the Helsinki Early Modern English Corpus*

	1500–1570	1570–1640	1640–1710
AUX pronoun NOT	42	44	16
AUX NOT pronoun	18 (30%)	12 (21.4%)	34 (68%)
Total	60	56	50

Source: Figures are from Rissanen 1994:344.

(7) a. Did I not give it thee? (231–32)
b. do not I know the wine thou drink'st is as base as the company thou keep'st. (261–34)
c. Don't you admire it strangely? (301–15)

Historically, NOT in questions is increasingly found between the finite verbal form and its subject, instead of after the subject. This development takes place much earlier with full nominal subjects, and with them it occurs with both full verbs and auxiliaries. It seems to be effectively complete by the 16th century, though both variants remain grammatical today (Huddleston & Pullum, 2002:chap. 9, sect. 2.1). There are no instances in questions of the sequence Finite + NP subject + NOT in Ellegård's database after 1501 or in the first Early Modern English period of the Helsinki Corpus (1500–1570) (Rissanen, 1994:340). With personal pronoun subjects, the earliest examples of Finite + NOT + pronoun subject in the Helsinki Corpus are found in the Late Middle English period, the order develops strongly with auxiliaries in the 16th and 17th centuries, and, on the assumption that contracted forms such as *don't*, *mayn't*, *can't*, should be treated as a further development, today the change is close to completion with auxiliaries. In corpora drawn from the 1960s, the London–Lund Corpus of spoken English, and the Lancaster–Oslo/Bergen Corpus of written English, rates are 94% and 84%, respectively (figures from Rissanen, 1999b:198). So there was a rapid increase in incidence with considerable variation in the 16th and 17th centuries. The extent of increase and variation in Early Modern English in which the finite verb was an auxiliary can be seen from Table 9, which gives figures from Rissanen's (1994; 1999b) investigation of the Helsinki Corpus.

It is interesting to see that in Ellegård's database this variation patterned differently before and after 1600 in texts of higher and lower lexical complexity. Tables 10 and 11 give figures for negative questions formed with DO, omitting questions with inverted finite lexical verb. They make three things clear: Comparing the "total" column in each table shows that the overall proportion of DO + NOT + pronoun subject increases across time, from 17% in the 16th century to 47% in the period 1600–1710; Table 11 shows that after 1600 there is a clear stylistic element in the variation between DO + pronoun subject + NOT and

TABLE 10. *Variation between DO + pronoun subject + NOT and DO + NOT + pronoun subject in negative questions 1500–1599*

Lexical complexity	Higher	Lower	Total
DO pronoun NOT	74	77	151
DO NOT pronoun	18 (20%)	14 (15%)	32 (17%)
Total	92	91	183

$\chi^2 = 0.554$, $df = 1$, $p > .25$. The difference is not significant.

TABLE 11. *Variation between DO + pronoun subject + NOT and DO + NOT + pronoun subject in negative questions 1600–1710*

Lexical complexity	Higher	Lower	Total
DO pronoun NOT	35	45	80
DO NOT pronoun	13 (27%)	58 (56%)	71 (47%)
Total	48	103	151

Figures include instances of *don't* as the sequence DO NOT. $\chi^2 = 11.227$, $df = 1$, $p < .001$. The difference is significant. The difference remains significant if instances of *don't* are omitted ($\chi^2 = 5.107$, $p < .025$).

DO + NOT + pronoun subject; and Table 10 shows that there is no apparent effect of stylistic level on this variation before 1600. This holds also for subperiods before 1600, most importantly, the effect cannot be seen in 1575–1600 or 1585–1600. It seems reasonable to associate this development with the evaluation of negative forms proposed earlier for negative declaratives. Then, if we make the natural assumption that evaluation will have had a weaker effect in the texts of lower lexical complexity, and a stronger effect in the texts of higher lexical complexity, it is clear that when texts of higher lexical complexity use DO they are preferring the older variant DO + pronoun subject + NOT, and dispreferring the incoming variant DO + NOT + pronoun subject. So it seems likely that there is a more complex stylistic effect here. Instead of (or, in addition to) the opposition V versus DO found in negative declaratives, in questions with pronoun subjects, we have DO + pronoun subject + NOT versus DO + NOT + pronoun subject. The overall situation is consistent with an evaluation dependent on established literary models, in which the incoming variants are disfavored, and the older established variant is favored.

If this is the case, then arguably, we should expect the drop in Ellegård's data to impact more strongly on DO + NOT + pronoun subject than on DO + pronoun subject + NOT. And this is indeed the case, as can be seen from Table 12. Although the stylistic contrast is not clear until after 1600, Ellegård's drop in 1575–1599 clearly involves a dramatic reduction in DO + NOT + pronoun subject, alongside

TABLE 12. *Variation in negative questions with a personal pronoun subject between lexical verb, DO + pronoun subject + NOT, and DO + NOT + pronoun subject*

	1501–1529	1530–1539	1540–1574	1575–1599	1600–1624	1625–1710
Lexical verb	33 (52%)	21 (32%)	23 (28%)	41 (46%)	5 (7%)	8 (9%)
DO pronoun NOT	25 (39%)	39 (60%)	43 (52%)	44 (49%)	43 (57%)	37 (42%)
DO NOT pronoun	6 (9%)	5 (8%)	16 (20%)	5 (6%)	27 (36%)	44 (50%)
Total	64	65	82	90	75	89

The period 1575–1599 which shows distinctive developments is shaded.

an essentially stable situation in DO + pronoun subject + NOT. Overall, Table 12 shows a steady decrease across time in the incidence of inverted lexical verbs, and a steady increase in the incidence of DO + NOT + pronoun subject. But both developments are sharply interrupted at 1575–1599. In contrast, the incidence of DO + pronoun subject + NOT is much more stable, and does not show a distinctive shift at 1575–1599. Ellegård comments that the figures for negative questions are small and perhaps therefore somewhat erratic, so perhaps too much should not be made of this, but the difference here is certainly suggestive.[11] We might also expect that there would be a drop with nominal subjects. Here the situation is less clear, because with transitive verbs, DO has essentially gone to completion by 1575. But although the numbers are small, intransitives show the expected drop in the sequence DO NOT preceding a nominal subject.

A POSSIBLE RATIONALE FOR EVALUATION

Why should there be such an evaluation in negative questions and in negative declaratives? And why should it occur when it does? Maybe we should just accept as a fact that when DO NOT in declaratives reaches a certain level (here 38%) evaluation of the opposition between incoming and established forms becomes possible, and in lexically more complex registers it is reasonable to assume that the forms typical of established literary norms would be preferred. This would imply a preference for a finite lexical verb in negative declaratives, and for the finite lexical verb or for the sequence DO + pronoun subject + NOT in inverted questions. But the further development of contracted forms is evidenced in spelling not long after this period, and it is worth considering whether this change might be involved in some way.

Jespersen suggested a date of 1600 for the development of contracted forms of NOT (1909–1949:part V 23.15), although spellings that reflect such contractions directly do not appear until later in the 17th century. Subsequently, Brainerd (1989) has cited earlier forms than previously known, giving examples of *can't*, *don't*, *han't* (= *haven't*), *shan't*, *won't* from the 1630s along with a few instances from the 1620s. Brainerd also noted the earlier existence of spellings that show auxiliary and NOT as one word, including, for example, the combinations *didnot*,

TABLE 13. *Order of NOT and personal pronoun subjects in negative questions 1500–1710*

	1500–1539	1540–1575	1575–1710
DO pronoun NOT	64	43	124
DO NOT pronoun	11 (14.7%)	16 (27.1%)	76 (38.0%)
Total	75	59	200
V pronoun NOT	48	23	52
V NOT pronoun	6 (11.1%)	0 (0%)	2 (3.7%)
Total	54	23	54

shallnot, *wilnot* in the *Respublica* (1553), suggesting that these point to "portmanteau" disyllabic forms. Rissanen (1994; 1999b) has paid attention to the early stages of the development of contracted forms, and argued very convincingly that the reduction of NOT is comfortably under way in the first half of the 16th century (1994:345; 1999b:197). He suggested that in this century, when NOT occurred immediately after an auxiliary, it could be pronounced with low stress and a centralized vowel. Rissanen referred to the portmanteau spellings of the type cited, but his main evidence was the changing order of pronominal subject and NOT with auxiliaries. Rissanen pointed further to the fact that in questions when the subject is a personal pronoun, NOT occurs directly after DO (and other auxiliaries) much more frequently than it does directly after a full verb. This distribution is seen in the Helsinki corpus for 1500–1570, and it is confirmed by figures from the much larger Oxford Shakespeare Corpus (1999b:197). He went on to say that "enclitic forms are typical of auxiliaries – the weakened forms of *not* were probably never appended to full verbs" (1994:345).

Ellegård's data showed a similar disproportion, though it is a less dramatic one: For 1500–1575 NOT + pronoun subject order occurs in 20.1% of instances of DO, and in 7.8% of instances of the finite lexical verb. But there is also an interesting diachronic difference, because it is clear that the placement of NOT is already developing differently in the two categories from the middle of the 16th century. The figures of Table 13 clearly show this divergence, which continues to widen. With DO, the development is making rapid progress; for verbs it is at best stable, more probably declining. This is further evidence for the status of a reduced form and its association with auxiliary DO. It strongly implies that Rissanen was right and that the development was a real fact about the language by the middle of the 16th century. The distribution of figures in Table 13 also supports his further suggestion that the reduced form did not occur with nonauxiliary verbs.

This opens the interesting possibility that evaluation of the contrast between interrogative inversions with DO and with lexical verb was triggered by the development of reduced forms of NOT alongside more fully stressed forms. These variants would have been differentiated not only by stress, by also by syntactic distribution (with the reduced forms being restricted to the position immediately

following an auxiliary) and presumably by style, with more fully stressed variants proportionately more common in careful styles. If evaluation, whether covert or overt, followed the establishment of this difference, it is straightforward to see which way around it would go; the more fully stressed variants would have been interpreted as appropriate for more careful styles, and the reduced variants as less appropriate for such styles. This could have led to the contrast in negative declaratives and the more complex patterning found in negative questions. The suggestion is necessarily speculative, but it seems not implausible.[12] We are, of course, familiar with later evaluations that stigmatize the occurrence of clitic forms such as *He'll* or *shan't* in formal speech or in writing.

CONCLUSIONS AND IMPLICATIONS

There is a clear reversal of direction of register variation in negative declaratives with NOT between the periods 1500–1575 and 1600–1710 in Ellegård's database. In the earlier period, DO is more common in texts of higher lexical complexity than in texts of lower lexical complexity. In the later period, the relationship is reversed. The later period also shows strong age-grading of a type compatible with an evaluation of the opposition between DO and lexical verb, which is disregarded among younger writers, but is more observed with increasing age. It seems reasonable to conclude that a new evaluative principle for register variation is introduced in the late 16th century, and that its effects are seen in texts of higher lexical complexity. The effects could be held to account for the major drop in overall incidence in the use of DO NOT in negative declaratives in Ellegard's data in the period 1575–1600, and there is no apparent need to appeal to further (specifically, grammatical) mechanisms to account for this fall. In contrast, texts of lower lexical complexity seem to show a steady development, which may reflect the underlying, more vernacular trend in the development of DO NOT. Negative questions show some of the same effects, and the incidence of DO + NOT + pronoun versus DO + pronoun + NOT may imply that evaluation focused on the sequence DO + NOT, or even that the cliticization of NOT is implicated in the evaluation.

It is clear for independent reasons that there was a reanalysis of DO in the second half of the 16th century. But the substantial overall dip in negative declaratives in Ellegård's data as we cross 1575 does not supply any further support for such a reanalysis, granted that it is a fundamentally stylistic phenomenon. Similarly, if 1600–1710 is a period in which evaluation prefers the simple finite over DO NOT, then the fact that the development of DO NOT seems to diverge from that of questions is not good direct evidence for a grammatical divergence. This line of argument was advanced by Kroch (1989:234) who claimed that DO underwent three independently evolving processes after 1575, in affirmative declaratives, in negative declaratives, and in question contexts, on the ground that each context shows a distinct rate of change. His general conclusion is absolutely correct; and this form of argument was accepted earlier, when it was agreed that

the distinct behavior of affirmative declaratives was evidence for grammatical change. But the status of negative declaratives is unclear, because we can now see that we need to take account of the contribution of register variation in estimating the apparent rate of change of DO. In a similar vein, we need to reexamine the evidence for Roberts' account (1993:303–306). He suggested that NOT occurs in SpecNeg until 1600, so that when obligatory DO support entered the language in the late 16th century, the incidence of DO with NOT represented the older optional DO, and declined in tandem with affirmative DO. Subsequently, NOT was reanalyzed as Neg^0, with which DO support is obligatory, and the incidence of DO recovered. But, again, if the decline of DO NOT in the last part of the 16th century is a stylistic matter, then it no longer counts as rather compelling evidence for a particular interpretation for the interface of two grammatical developments (obligatory DO support, and the reanalysis of NOT as Neg^0). Slade (2003) also took the decline of DO NOT to be a grammatical fact, and predicted it to be a consequence of the addition of a particular constraint ranking at the end of the 16th century, within an Optimality Theoretic model. This too needs reexamination, given the interpretation argued for in this article

NOTES

1. Figures for each text were based on a 600 word sample drawn from the database. DO and NOT were excluded (because Ellegård's sample was chosen to contain these items), and variation in spelling was eliminated to give a reliable measure of word length.

2. The program DataDesk (Velleman, 1995) has been used, because it allows for continuous variables. Tables and Figures in this article present results drawn from Ellegård's database unless the title explicitly says otherwise.

3. Other auxiliaries are omitted, as are verbs belonging to Ellegård's "*know* group," DO as a main verb, and some minor items consistently omitted by Ellegård. These all have a very low level of DO. Thus percentages, like those given earlier, are (frequency of DO)/(frequency of DO + frequency of simple finite nonauxiliary verb) with the further omissions just noted. These omissions hold throughout the discussion of negative declaratives. Figures for negative questions include members of Ellegård's "*know* group," but share the other omissions. I have also omitted data from the Plumpton letters (taking them to be northern), and instances where NOT precedes the finite verb. Thus my figures are somewhat lower than Ellegård's.

4. The appropriate equation for 1655 is:

$$(\exp(\ln((do/total)/(1 - do/total)) - (date - 1655) * rate)$$
$$/(1 + \exp(\ln((do/total)/(1 - do/total)) - (date - 1655) * rate)))$$

5. This number is larger than Ellegård's number of texts, because some have been split (e.g., Congreve (one novel, one play); Dryden (early versus late)). In Figure 3 and scatterplots for negative questions, the number is lower because some smaller texts have been amalgamated to give more robust figures.

6. This running average is a "lowess." It takes a proportion of the data as a window (here 35%) and produces an average by weighting values more highly as they are closer to the point being established. The running averages given in later figures in this article are all lowesses set at 30% or 35%. See Velleman (1995:33/11–12, 33/16–18) and Cleveland (1979).

7. Actually, the appropriate significant regression in this case is between the square root of DO% and lexical complexity, because this has an approximately normal distribution, as DO% itself does not; here $p = .027$. But I give the graph for the untransformed variable.

8. My interpretation in the table is based on Nurmi's figures, which imply that the two categories are very close in 1570–1640. She kindly informs me that the percentage given for non-oral texts in this period is in error.

9. Breton was born 1545. See Nurmi (1999:173–174) for figures and graphs which imply that the group of individuals born 1520–1539 differ from those born 1540–1559 and later, in that they did not

show a drop in usage in the early 17th century, unlike those born later. Given that the boundaries here are arbitrary, Breton might, in effect, have belonged to the earlier group.

10. In texts of higher lexical complexity from 1600–1710, 60/76 (79%) of question tokens are from two authors (Jonson and Hawkins).

11. An alternative grammatical mechanism for part of this development is conceivable. Let us accept the position of Han (2001) and Han and Kroch (2000) that there are two positions for NegP, one between TenseP and MoodP, the other between AspP and VP, where MoodP dominates AspP. For them, the rise of periphrastic DO is a consequence of the loss of V-movement from Mood to Tense in the 15th and 16th centuries, but a consequence of the loss of V-movement from V to Asp from the end of the 16th century. Then suppose further that (1) the higher NegP increasingly has NOT in the head position, so that DO incorporates with it, and a possible question order is DO + NOT + pronoun subject, alongside the DO + pronoun subject + NOT, which results when NOT is in SpecNeg and there is no incorporation; and (2) the lower position has proportionately much less head NOT, and much more SpecNeg NOT. This would be expected if modals are typically generated above this position. Then as the loss of V-movement from V to Asp begins, it will result in the replacement of a proportion of examples with finite lexical verb in negative questions by examples of DO + pronoun + NOT, giving of a higher overall proportion of the category DO + pronoun + NOT. This might be responsible for the changing proportions of DO + pronoun subject + NOT versus DO + NOT + pronoun subject discussed in this section and shown in Table 12, though we apparently need to assume some independent reason for the more general decline of DO in negatives, and for the stylistic difference shown in Table 11.

12. This might imply that a similar development could be traced with modals. I do not know whether this is the case.

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