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Democratization and Linguistic Complexity: The Effect of Franchise Extension on Parliamentary Discourse, 1832–1915

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We consider the impact of the Second Reform Act, and the doubling of the electorate it delivered, on the linguistic complexity of speeches made by members of parliament in Britain. Noting that the new voters were generally poorer and less educated than those who already enjoyed the suffrage, we hypothesize that cabinet ministers had strong incentives—relative to other members—to appeal to these new electors with simpler statements during parliamentary debates. We assess this claim with a data set of over half a million speeches for the period between the Great Reform Act and Great War, along with methods for measuring the comprehensibility of texts—which we validate in some detail. The theorized relationship holds: ministers become statistically significantly easier to understand (on average) relative to backbenchers, and this effect occurs almost immediately after the 1868 election. We show that this result is not an artifact of new personnel in the House of Commons.

Few topics have featured as prominently in applied political science research as the causes and consequences of democratization (e.g., Acemoglu and Robinson 2005; Boix and Stokes 2003; Huntington 1968; Lipset 1959; Przeworski et al. 2000). Of particular interest is the (optimistic) notion that with franchise extension and competition comes increasing political responsiveness and accountability for citizens (e.g., Bartolini 2000; Przeworski 2009). And within this large literature the changes to Britain in the middle of the nineteenth century have captured a great deal of scholarly attention (e.g., Bagehot [1873] 2011; Gash 1952; Seymour 1915; Trevelyan 1922; Woodward 1962), with primary focus on the passing of the relevant legislation in 1867 (e.g., Himmelfarb 1966; Moser and Reeves 2014; Smith 1967; Walton 1996) and its effects on politicians and voters (e.g., Berlinski and Dewan 2011; Camp, Dixit, and Stokes 2014; Cox 1987; McLean 2001). In part, this is because the "Westminster system" that resulted has been widely emulated for its stability and decisiveness (e.g., Lijphart 1999; Rhodes and Weller 2005), and there is thus a natural interest in uncovering its development and possibly charting its future course. This is especially true of its characteristic institutions of ministerial responsibility and fierce front-bench competition in parliament and in the electorate.

In keeping with this interest, in the current article we seek to understand how suffrage extension affected the behavior of backbench members of parliament (MPs) relative to ministers during the Victorian period. Our central idea is that members of the governing executive—the cabinet—had new incentives after the expansion of the electorate: they were required, as leaders of their parties, to appeal to a poorer, less educated median voter. We contend that they did so, in part, via simpler linguistic expressions in their parliamentary speeches. Meanwhile, because backbenchers increasingly understood that citizens were "voting for the party, rather than for the man" (Cox 1987, 136), we argue that these MPs were under considerably less pressure to adjust their speaking style.

We are hardly the first to investigate the new incentives for legislators and their principals introduced by voting reform. In the long term, the eventual rise of the parliamentary Labour party as an electoral force representative of working-class interests (see, e.g., Cox 1997; Thompson 1963), along

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with the commensurate decline of the Liberals (Searle 2001), is probably the best known consequence of a larger franchise (when considered alongside other reforms such as the introduction of the secret ballot). By contrast, and with some exceptions (see, e.g., Hurst [1965] on the effects of the secret ballot in Ireland), analysts find relatively little evidence of immediate change to other markers related to MP activity: these include roll call cohesion (Eggers and Spirling 2014c), Liberal vote share (Berlinski and Dewan 2011), the socioeconomic backgrounds of cabinet personnel (Berlinski, Dewan, and Van Coppenolle 2014), and party orientation in the electorate (Cox 1987). On the one hand, these null findings are surprising: the almost doubling of the electorate via the Second Reform Act to include poorer, less educated voters was certainly predicted (or feared) by contemporaneous actors to have consequences for the nature of both substantive and descriptive representation (see, e.g., McLean [2001] for a discussion of the "Adullamites"). On the other hand, scholars of the period do not typically have access to the kinds of fine-grained data that make investigating possibly subtle shifts in behavior straightforward. Compare this situation, for example, with the study of American politics—in particular regarding the "Homestyle" of Members of Congress, where researchers can either follow contemporary members and record their interactions (e.g., Fenno 1978), experiment on them (see Grose [2014] for an overview) and their constituents (e.g., Larson 1990), or work with the large amounts of text produced by such elected officials in communication with their constituents and others (e.g., Grimmer 2013). Similarly, researchers interested in comparative politics for the contemporary period can use manifestos (e.g., Benoit, Laver, and Mikhaylov 2009; Budge et al. 2001), parliamentary speeches (e.g., Slapin and Proksch 2008), and detailed election studies with roughly similar questions across nations to estimate the degree to which different systems and different times respond to voter needs (e.g., Powell 2000). Thus, while we have very strong priors for this vital period in the Westminster system's history, testing our hypotheses is prohibitively difficult, and our empirical findings look ambiguous at best—and confusing at worst.

Diagnosing the problems with extant studies of the effects of democratization is not difficult; solving these maladies is far from trivial. Put crudely, scholars are typically restricted by limited data on elite—that is, MP—responses to suffrage expansion: studies are either intensive with coverage of short time periods (e.g., Berlinski and Dewan 2011; Schonhardt-Bailey 2008) or more extensive in terms of numbers of observations but necessarily less fine-grained in terms of both the information and inferences that are possible (e.g., Cox 1987). In the former case, researchers face the obvious difficulty that suffrage expansion may not have immediate consequences for politician behavior—perhaps because some degree of "learning" must take place. In the case of the broad studies, though impressive in scope they are likely to miss subtle, small changes to the way that individual agents perceive the situations they face and adjust their actions accordingly. In both cases then, there is a danger that effects that do exist are "missed." Putting aside these specific issues of study scope, it is far from obvious where we should look for evidence of new incentives and behaviors: ideally, we would have a large number of observations from which we can plausibly measure "responsiveness" directly and in a way that allows us to compare both across individuals and across time.

In this article, we make progress where other attempts have faltered and show evidence consistent with our contention of a differential behavioral effect on ministers versus backbenchers. We do this in a way made possible with an analysis of a data set of hundreds of thousands of speeches—along with MP covariates—from the House of Commons between 1832 and 1915 (see Eggers and Spirling 2014b). We focus on speeches as outputs precisely because they allow politicians to respond instantly to changing circumstances, without the various implementation lags one must allow when studying policy or party-system shifts. Further, in contrast to the 24-hour news cycle politics of the present day, parliamentary speeches (and reports on them) were the primary way that voters of the time monitored the actions of their representatives. Speeches have another advantage: they are an equitable resource insofar as, subject to recognition rules in the Commons, anyone can (and did) undertake them, allowing us observations for essentially the entire population of MPs. In summary then, speeches are very much a key place, if not the only place, where we might see democratization having an immediate and noticeable effect.

Our innovation methodologically is to measure the "comprehensibility" (or complexity) of the utterances using well-known metrics from education research that take into account the number of syllables relative to the number of words found in documents (see Flesch 1948). These are straightforward to calculate and have been used elsewhere in the study of speech (e.g., Lim 2008), albeit not on so many texts. These scores are combined with techniques that allow multiple individuals, making multiple speeches, to be compared over time as their roles in the chamber change. In particular, we show that almost immediately after the Second Reform Act, cabinet ministers altered their speech in a way that made those speeches simpler to understand for the median member of the electorate—that is, someone poorer and less educated than had previously voted. This finding provides

crucial support for earlier hypotheses regarding the leadership role that cabinet members increasingly played (relative to backbenchers) in appealing directly to popular opinion, such that their parties could win national elections (Cox 1987; Jenkins 1996; Rush 2001). In terms of point predictions, our estimates imply that, controlling for length of speech and other member-level variables, being a minister after 1868 was equivalent to moving from around the 48th to the 60th percentile of comprehensibility in the chamber as a whole: with predicted values approximately 10% larger than backbenchers. This finding is robust to the usual standard error corrections and to alternate specifications and measurement strategies. Importantly, we are able to rule out the possibility that the change in language is due to new personnel arriving in the House over time: rather, it is the result of new incentives for those already there.

Although the techniques we use here are not new, we are applying them to a large data set and in an innovative way. With that in mind, we spend some time below exploring their details and validating their use, before moving to our results and conclusion. Prior to that, however, we set the substantive scene for our study: the advent of the Second Reform Act in 1867.

APPEALING TO THE NEWLY ENFRANCHISED

The Representation of the People Act of 1867—colloquially known as the Second Reform Act—has attracted much scholarly attention on its origins and passing (e.g., Cowling 1967; Himmelfarb 1966; McLean 2001; Moser and Reeves 2014; Smith 1966), its details, and its effects (e.g., Aidt, Daunton, and Dutta 2010; Berlinski and Dewan 2011; Canandine 1999; Laski 1928; Smith 1967).[1] The features of interest for our purposes are twofold: first, the massive expansion of the franchise from around one to two million men.[2] Second, the reduction in the property requirement needed for voting (see, e.g., Walton [1996] for details of the change). In practice, and importantly for our work here, the Act "brought substantial working-class majorities to the electoral registers of almost all the boroughs" (Walton 1996, 35), and in this way suffrage expansion was disproportionately greater in urban areas than elsewhere. Indeed, by using figures from Bowley (1937) and Mackenzie (1921), Berlinski and Dewan (2011, 7) note that "it is clear that the extension of the franchise gave the vote to urban unskilled workers."

Table 1. Number of Literate and Illiterate Men in Mitch (1992) Sample, by Occupational Class

	Occupational Class				
	Prior Voters		New Voters		
	I	II	III	IV	V
Literate	144	383	1,625	536	513
Illiterate	0	8	229	78	343

Note. The "prior voters" are those classes likely already enfranchised prior to the Second Reform Act; the "new voters" are those classes more likely to be part of the newly extended franchise.

A salient feature of these new voters was that they were, on average, less educated (and less literate) than preexisting holders of the suffrage. To see this, consider calculations from Mitch (1992), who obtains a large sample of marriage certificates for the period 1869–73 and measures male literacy from the ability of grooms to sign their own names. The class status of the men is inferred from their father's occupation, listed on the same certificate. In the Mitch (1992, 24–25) approach, there is a hierarchy of five socioeconomic classes, the latter three of which are characterized as "petty shopkeepers, skilled manual trades, mining, most transport occupations," "semi-skilled manual labor," and "unskilled labor," respectively.[3] In table 1 we provide figures for all five of these groups,[4] in terms of the number of individuals recorded as literate and illiterate in each. As can be seen from the table, the top occupation class (I and II) have very few individuals (around 1%) who lack literacy skills. By contrast, grouping the lower three classes together, we see illiteracy rates at around 20%. These proportions are statistically significantly different ($p < .01$).[5]

If we interpret the lower labor classes as being composed of those joining the suffrage as a result of the Second Reform Act (which accords roughly with the distinctions made by Berlinski and Dewan 2011), we have clear evidence that these "new voters" were less educated and less literate than those already part of the franchise. Although we cannot observe this directly, it seems safe to further assume that those that were literate in classes of low overall literacy had lesser proficiency in reading and writing that the literate in classes

1. The Act itself dealt with English and Welsh matters; Scotland and Ireland saw reforms via the Representation of the People (Scotland) Act and Representation of the People (Ireland) Act, both of 1868.

2. For reference, the 1871 census recorded a total population of around 26 million for England, Scotland, and Wales combined. Around 12.6 million were males (of any age).

3. For completeness, class I are those occupations types that are "titled, high public office, military officers"; class II are "professions, commerce, clerical, farmers."

4. Calculated from table 2.3 in Mitch (1992).

5. This result is robust to including class III as one of the "prior voter" groups; separately, it is robust to dropping class V from the analysis altogether.

of widespread literacy. That is, we suppose that the binary indicator of literacy hides continuous variation whereby literate voters prior to 1868 were on average better able to read than the literate who joined the franchise after the Second Reform Act. This matters for our causal story below, in which speeches are reported to electors mostly in written (i.e., newspaper report) form; thus, we require that the literate among the new voters struggle with complex linguistic expressions more than the literate who were already voting.

Ministers as the focus of electoral competition

A related consequence of the franchise expansion was the development of new political behavior by electors; in particular, "voting for the party, rather than for the man" at the ballot box, with citizens increasingly "using their votes to determine what did matter: party control of the executive" (Cox 1987, 136). Whether or not the rise of the "party orientated electorate" was caused simply by the expanded suffrage per se is debatable (see Cox 1987, 94–95), but there is little doubt that it focused attention on the cabinet and its members as the key actors in politics and the ones responsible for winning (or losing) elections. Commensurate with this new role as the locus of voter choice was an ongoing increase in partisan cohesion (beginning in the 1850s) in roll call voting, with leaders in the House of Commons able to discipline their troops at levels approximately equal to those in modern British politics (see Eggers and Spirling 2014c). Crucially for our account, backbenchers had much weaker incentives than ministers to adjust their language. This is because, at a time when national party appeals began to matter more than local connections or family name, those without cabinet rank were no longer as important as they previously had been for winning their own seats: it was their leaders on the front benches who would be the deciders of election success or failure for everyone in their party. Of course, this does not mean that backbenchers did not make any attempts to curry favor with their local electors; rather, our position is that as the franchise expanded, such individual MP efforts were (*a*) less effective than they once had been as voters increasingly responded to leaders at Westminster and their "brands" and (*b*) much more costly—perhaps prohibitively so in some seats—than previously.[6] To clarify further, the claim here is not that backbenchers were utterly unaffected linguistically by the Reform Act; instead, our theory predicts that their rate and total magnitude of change would be less than their leaders.

Our central idea is that these forces—new, less educated voters, and the "triumph of partisan politics" (Jenkins 1996, chap. 6) in the electorate and in parliament—meant that the Westminster executive was faced with fresh challenges and opportunities. In particular, the cabinet was required to adopt strategies such that it could appeal to electors and compete successfully for power at the ballot box. While others have investigated these strategies as they pertained to election spending (e.g., Camp et al. 2014; Hanham 1978), and some have specifically investigated the emergence of early manifesto-style addresses such as in the Midlothian Campaign (see Kelley 1960; Matthew 1997), we turn our attention to the changing nature of speeches in the House of Commons itself.

Observational implications

Our hypothesis is twofold: first, that cabinet members reduced the linguistic complexity of their speeches after the Second Reform Act; second, that the average change in complexity for ministers was larger than the average change for those not serving in the cabinet.[7] Ministers altered their speech to ensure that the newly increased electorate—with its lower average educational level—could understand and be convinced by executive speeches. Put more crudely, democratization resulted in the "dumbing down" of rhetoric and argument by ministers in a way designed to win votes at the ballot box. Whether this proposed mechanism is convincing depends on the plausibility of several links in the causal chain. First, readers may question the extent to which parliamentary speeches were in fact disseminated to the public at large. For the period under study, this is not a concern: indeed, Victorian Britain was notable for "universal press coverage" of Commons activity and "the explosive expansion of the press in the middle of the century" (Cox 1987, 54–55). This press penetration extended to poorer voters, especially after the repeal of taxes that had kept prices artificially high until the middle of the century. Thus, by 1861 the cheapest of the weekly sheets, including those aimed specifically at working-class voters such as *Lloyd's Newspaper* and *Reynold's News* had circulations of 412,000 and 150,000, respectively (Hewitt 2013, 105). Furthermore, there is little doubt that parliamentary speeches did indeed make the news. To get a sense of political reporting, we inspected archived copies of *The Penny Illustrated Paper*, an inexpensive pictorial publication produced from 1861 to 1913.[8] We looked specifically at the first sixth months of 1886, a period in which

6. See Camp et al. (2014) for discussion of the United States and United Kingdom in comparative perspective on this point.

7. We give an explicit "difference in differences" formulation of the problem below.

8. Available via library subscription to the *Gale Digital Collection*. Neuberg (1977, 224) notes that it had a circulation of around 200,000 by 1885, while Martin (2006, 23) points out that it "targeted the working classes and tended to cover the kind of information that interested them".

Prime Minister Gladstone attempted to pass the first Irish Home Rule Bill before resigning when he failed to do so. Searching the records for "Gladstone" as a keyword returns results (see app. A for more details; apps. A–F available online) in which the Prime Minister's speeches from the dispatch box are quoted verbatim (January 30, April 17, April 24) along with utterances from Irish Nationalist Charles Parnell (April 17) and Liberal Unionist Joseph Chamberlain (June 19). Separately, the article carried information regarding the composition of Gladstone's cabinet (February 6), MP John Bright's views on the bill (March 27), and Gladstone's subsequent "manifesto" on the proposed fate of Ireland (June 19). The fact that newspapers relayed political debate was not lost on politicians of the day: for example, commenting on the implications of a discussion regarding women's suffrage in 1873, radical MP John Bright opined from the backbenches that "The substance of this debate will be carefully reported in the newspapers, the report will go to every town and village in the United Kingdom, and to every English-speaking country under British rule" (cited in Jenkins 1996, 18).

Second, it is clear that members themselves were acutely aware that the expansion of the suffrage would bring less educated (if not necessarily illiterate) voters into the electorate; Robert Lowe, leader of the "Adullamite" Liberal MPs skeptical of the Second Reform Act noted that those who would be newly enfranchised exhibited "venality . . . ignorance . . . drunkeness" and were in general "impulsive, unreflecting and violent people" (cited in Saunders 2011, 206). Among more sympathetic MPs, the debate was not over whether the pool of voters to whom politicians must appeal would change but rather the extent of those changes (see e.g., McLean [2001, 66–67] on the "rating" required for enfranchisement). Third, there is evidence that party leaders were aware of the need to appeal to these new voters, albeit in somewhat limited ways that did not alienate other members of the electorate. Thus Disraeli—or at least his Home Secretary Richard Cross—embarked on a series of union and labor reforms in the 1870s (St John 2010, 151–53), while Salisbury actively pursued the "respectable" suburban but working class "villa vote" (see Shannon 1996). On the Liberal side, the party made early, if perhaps ineffectual, attempts in some cities to "recruit candidates with working-class appeal" prior to the 1868 election (Moore 2006, 25), and the rise of "New Liberalism" thereafter is a specific example of an ideology that sought to embrace new voters and their concerns (Sykes 1997).[9]

All told then, senior politicians on the government side of the House of Commons had strong reasons to adapt their policies and language in the aftermath of the Second Reform Act in a way that was less true of their backbench colleagues in their own party and among the opposition. We should thus expect that any change toward simpler speech was larger in magnitude for ministers than it was for others. Assessing this claims requires that we investigate their speeches over time. It is to our data on this that we turn before explicating our measurement strategy.

DATA

Our data consist of speeches made in the House of Commons between 1832 and 1915. Thus, the Second Reform Act of 1868 occurs approximately half way into our time series, allowing a large window before and after in which to assess any effects on speech style. The speech data are matched to individual MPs, which is then matched to various covariates including party of election, cabinet status, and competitiveness of constituency elections. These measures and the data are described by Eggers and Spirling (2014a) (which draws on Butler and Butler 1994; Cook and Keith 1975; Craig 1974, 1989). For the data set in its original form, there are 860,192 speeches for 4,233 MPs, with an average of 203 speeches per member. For our analysis below, we restrict ourselves to members running for either the Conservative or Liberal party in elections, excluding various idiosyncratic versions of those labels, along with nationalist parties and the (early) Labour party. What remains are 675,997 speeches, from 3,613 members, for an average of 187 speeches per MP. We are confident that restricting our data is appropriate for at least two reasons: first, because only the Liberals or Conservatives could plausibly form the cabinet during the period under study and thus these parties constitute the key actors for our work; second, our findings below are robust to including those other parties as part of the opposition. We make very few further "adjustments" to our data set. In particular, we impose no minimum length on speeches (empirically, the minimum number of words is 1, the maximum is 11,000, with a mean around 248) and remove only one session from our analysis: the very short (just 129 speeches) first session after the indecisive 1892 general election, at which time Salisbury awaited a no confidence vote before resigning as Prime Minister. In some cases, we have covariate cases missing for (multiple observations on) MPs, and we drop those cases from our regressions, giving $N = 670{,}091$.

In time-series terms, our data are divided into parliamentary "sessions" each of which last approximately one year and which collectively comprise "parliaments" (which

9. See also Jennings (1962, 426) on Chamberlain's "Radical Program" for the 1885 General Election.

begin after general elections). In the period under study, different sessions within the same parliament have different parties in cabinet because Victorian politicians did not always go to the country for a new popular mandate after their Prime Minister resigned or lost the confidence of the House. An example of this would be the ascension of Disraeli (who followed Lord Derby to the premiership) after Lord Russell's Liberal government fell in 1866—without any intervening election. Because ministers (then as now) could leave office at any time with no more general consequence for the session itself, our measurement of who is a minister in any given session is relatively inclusive. That is, the metric includes anyone who served at least one day in the cabinet during that session. Having described our data, our next task is to provide a metric for measuring and comparing the comprehensibility of speeches made in the House of Commons.

METHODS AND MEASUREMENT

Starting at least with Sherman's (1893) "Objective Study of English Prose and Poetry," scholars of literature and education have been interested in the notion that texts could be statistically analyzed and their "readability" measured. Although this key quantity of interest has been variously defined—depending in part on the relevant researcher's motivation (e.g., Dale and Chall 1949)—at its core, readability refers to the comprehensibility of a text, literally, the ease with which it may be understood by a reader with varying levels of education. A number of metrics have been proposed for assessing comprehensibility (e.g., Dale and Chall 1949; Gunning 1952; Lively and Pressey 1923; McLaughlin 1969) with that of Flesch (1948) being the most famous and widely used (Klare 1963). Flesch's (1948) formula, given in equation (1), yields a score for any given body of text that is known as the Flesch Reading Ease (FRE) statistic. In the original application from which it was derived, the value of the statistic was found to have a lower bound of 0 and an upper bound of 100, though this need not be the case in other data sets.[10] Though we will use the score directly in what follows, we note that educational researchers typically convert this output to a (minimum) number of years of US schooling—known as a Flesch-Kincaid Grade Level—that a student would require to find a given document comprehensible (see Kincaid et al. 1975):

$$206.835 - 1.015\left(\frac{\text{total number of words}}{\text{total number of sentences}}\right) - 84.6\left(\frac{\text{total number of syllables}}{\text{total number of words}}\right). \quad (1)$$

Inspection of equation (1) suggests that the Flesch score is not difficult to calculate using modern processing programs, assuming some machine-readable version of the text exists.[11] As can readily be seen, for a fixed number of words in a document, increasing the number of syllables of those words and grouping the words into fewer sentences both increase the complexity of the text in question.

Guidelines for interpreting the statistic may be found in several sources (including Flesch 1949, 149–50); Cann, Goelzhauser, and Johnson (2014, 663) give the following: "Texts with FRE scores ranging from 0 to 30 are considered very difficult to read, 31 to 50 are difficult, 51 to 60 are fairly difficult, 61 to 70 are standard, 71 to 80 are fairly easy, 81 to 90 are easy, and 91 to 100 are very easy." To anchor these categories conceptually, note that Cann et al. (2014) place the average academic political science article at around 33, on a par with judicial opinions, while the *New York Times* has a mean FRE of about 48 and childrens' books such as *Peter Pan* and *The Wind in the Willows* have FRE scores approaching 80. Giving context for these scores outside of the school setting, Dalecki, Lasorsa, and Lewis (2009, 6) calculate that "85 percent of Americans today can read at the 50–60 reading ease level, 72 percent at the 30–50 level, and 28 percent at the lowest (0–30) level."

As suggested by the citations above, this article joins a literature in social science that makes use of Flesch scores. It is also not the first piece to make the assumption that tools designed originally for measuring ease of reading can be meaningfully applied to texts that were spoken. For example, Jansen (2011) considers the clarity of central bankers' answers to questions at legislative hearings. Closer to the subject matter of the current article, Lim (2008) considers the evolution of rhetoric in Presidential speech-making since the founding of the republic. As a practical matter of course, the most common method by which parliamentary speeches would come to the attention of voters at the time would be via written reports in newspapers.

10. The formula results from a study undertaken by Flesch in which he regressed the average grade level of school children who could answer at least 75% of multiple choice questions regarding comprehension of texts they read on a constant and the two bracketed variables in the equation. In that context, a score of 100 means that the document could be understood by a student with a fourth grade education and thus could be described as "barely functionally literate" (Flesch 1948, 225).

11. Indeed there are several online calculators for this task, and it is included as standard in some word processing software. Here we use the implementation given by Rinker (2013) for the R statistical environment.

FRE scores for parliamentary speeches

Applying the formula implied by equation (1) to the parliamentary speeches for our study requires some preliminary preprocessing decisions on how to deal with the texts. While we do not stem the documents, or remove stop words, we do convert some parliamentary terms of art that contain period punctuation: thus, "Hon." becomes "Honorable," "Rt." becomes "Right," "Mr." becomes "Mister" and so on.[12] This allows more accurate calculation of the number of sentences in a speech, since it avoids miscounting periods. We then split each speech into sentences using the usual punctuation marks plus semicolons and vertical bars, which are used in the early periods of our data to break up long utterances.[13] Finally, we strip whitespace (other than single spaces) and drop speeches that contain no alphabetic characters (these typically arise when members give answers as numbers).

Looking over the entire period, the distribution of FRE statistics for our speeches is given in table 2. We note that mean and median are both around 52, with a standard deviation around 20. We note that the minimum (−301.80) and maximum (205.80) imply a range larger than in the original Flesch (1948) study, although the boxplot (left) and histogram (right) in figure 1 suggest that such values are outliers: note that the bulk of the distribution is between 0 and 100.

In figure 2 we report the (by session) mean speech comprehensibility for cabinet (square points) and noncabinet (circular points) MPs over the period under study. We also include smoothed loess lines to capture general trends. The main observation is that, somewhere around the 1860s, the average cabinet speech becomes more comprehensible than the average noncabinet one, whereas prior to that time the means had been very similar. Immediately then, we have some (albeit) crude evidence in favor of our hypothesis above.

The source of these patterns is naturally of interest. We know from equation (1) that the FRE score for a given speech is increasing in two (nonconstant) components: one pertaining to the average sentence length and one pertaining to the average number of syllables per word. For our preliminary finding above—that cabinet speeches became relatively (and absolutely) less complex after the Second Reform Act—we considered only the aggregate (i.e., combined) effect of changes to these quantities. To get a sense of what drives the underlying patterns, consider figure 3 where, for our historical period, we plot the (mean) average number of sentences for cabinet and noncabinet speeches and the (mean) average number of syllables per word for the same.

Table 2. Summary of FRE Statistics for Speeches in Our Data

Distribution	Value
Minimum	−301.80
First quartile	42.59
Median	52.25
Mean	52.63
Third quartile	62.14
Maximum	205.80
SD	19.94

Note. FRE = Flesch Reading Ease; SD = standard deviation.

An immediate observation from the figure is that, somewhere around 1868, the mean sentence length for ministers fell from around 27 words to around 21 words by the mid-1880s. While there was also a decline for backbenchers, it was not nearly so precipitous. The ministerial decrease does not appear to only be a function of sentence length, however. Studying the right panel, we see a decline in average syllables per cabinet word too. This recovers somewhat by the turn of the century, while the trend for the backbenchers is generally upward. All in all, the patterns here suggest that ministerial speech got simpler because cabinet sentences got shorter, while backbencher words (almost simultaneously) had more syllables. This latter fact adds some credibility to our underlying claim that noncabinet MPs became harder to understand in a relative sense, although we will return to the notion of possible "new roles" for parliamentarians after our more formal results section.

Given the length of the period under study, it would be surprising if speeches had not changed in ways other than their comprehensibility and composition. In fact, they became on average shorter: cabinet and noncabinet speeches had a mean length of around 500 words in the immediate aftermath of the First Reform Act and were reduced in length in a fairly smooth and consistent fashion over time. By the turn of the twentieth century, both cabinet and noncabinet speeches reach a low of around 150 words (on average). Figure 4 displays these trends clearly. A natural concern might be that any changes to comprehensibility of speeches (measured by the FRE statistic) are an artifact of this shortening. On inspection, we doubt this is the case: the correlation between speech length and reading ease is very weak, at around 0.04. This is true regardless of whether we include "outliers" (as identified in the boxplot of fig. 1) in the calculation. In any case, though, we include speech length as a variable in some of our regressions below.

12. We also drop some terms with periods, like "St."

13. Subsequent inspection suggests this decision makes no difference to the relative distribution of speech scores, outside of some outliers for noncabinet members. And in any case, our main results are robust to the removal of outlier speeches.

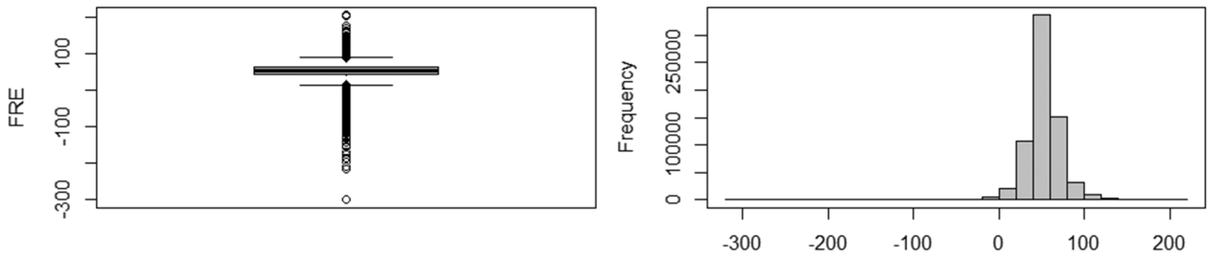

Figure 1. Distribution of FRE statistics for parliamentary data: boxplot and histogram. The *x*-axis for the latter is the FRE statistic of the speech. Note that the bulk of the FRE statistics are between 0 and 100.

Validation

An obvious concern about the use of Flesch scores is that though they are well validated outside of this application, they do not measure linguistic complexity for our period. In table 3 we a range of scores for speeches occurring in the first session of 1885. While the first three speeches are made by noncabinet members, the last two are utterances from ministers. Though this is in no way a test of our general propositions above, we see that the ministers here—who are responding to queries raised by others—tend to be "punchier" and more pithy in their speech. Furthermore, one can well imagine that working class voters would find it easier to follow the simpler speeches than the longer ones.

Another way to assess the validity of our approach is to consider the scores given to contemporaneous texts that are specifically not parliamentary speeches (and thus not in our data) but are aimed at voters in general. To the extent that the relative scores given to those documents are in line with our priors regarding their intended audiences, we have evidence that the metric is a reasonable one. Here we look at two members serving over approximately the same period: Keir Hardie, a Labour MP (with intermittent service between 1892 and 1915) and Arthur Balfour, a Conservative member (between 1874 and 1922). For Hardie, we use the text of eight books that he wrote between 1905 and 1911 published primarily by the (then) Independent Labour party and presumably aimed at working class voters.[14] For Balfour, we use (seven) essays for mid-brow magazines and lectures given to various university audiences between 1882 and 1891, presumably aimed at a more middle class audience (Balfour 1893).[15] For each of the works, we calculate the Flesch scores and report their distributions in figure 5. As can readily be seen, Balfour's writings with a median score of around 46 are considerably more complex than Hardie's with a median of approximately 61.[16] Parametric and nonparametric tests of means ($p < .01$) confirm this observed difference.

RESULTS

We begin with our estimation of the session-by-session regressions. That is, for each time period in our data, we regress the comprehension scores for the speeches on the cabinet status of the MP in question (a binary variable) with a series of controls—party, competitiveness of constituency, and the word count of the utterance itself.[17] Our estimated coefficients on cabinet are displayed (with 95% confidence intervals) in figure 6. The solid horizontal line marks zero. Our first observation from the figure is that the point estimates begin below zero and around the 1860s rise into positive territory and stay there for the remaining time periods in the data. In words, being a cabinet minister is initially associated with making speeches that are (on average) more difficult to comprehend than those of other members; subsequently, cabinet speeches are easier to understand. Obviously, in many cases, the confidence intervals cross the zero line, but a general pattern is apparent.

To clarify the timing of the change, we use the session-by-session coefficients on cabinet status and regress these

14. The focus of our analysis below are Liberal and Conservative members; consequently, Hardie is not in the parliamentary speech data. We make use of him here precisely because we know that as a Labour member he wrote for an (almost) exclusively working class audience during this period, whereas a Liberal member might well have been appealing more broadly in class terms, in keeping with that party's supposed electoral appeal at this time. Thus Hardie provides a purer validation test here.

15. Bibliographical details of the texts can be found in appendix B.

16. In passing, we note that Charles Dickens's fiction of this period has a mean score of around 77, suggesting that the politicians in question were writing and speaking in a considerably more complex way.

17. For each given session, we are estimating a regression of the form

$$\text{FRE}_i = \alpha + \beta_1 \text{cabinet}_i + \gamma \mathbf{Z} + \varepsilon_i,$$

where FRE_i is the FRE score of the ith speech, cabinet_i is the cabinet membership status of the MP making the ith speech; $\mathbf{Z}$ refers to a set of control variables—party, competitiveness of seat, and word count—for which γ is the set of coefficients and ϵ_i is an error term. Because members make multiple speeches per session, we cluster the standard errors at the MP level.

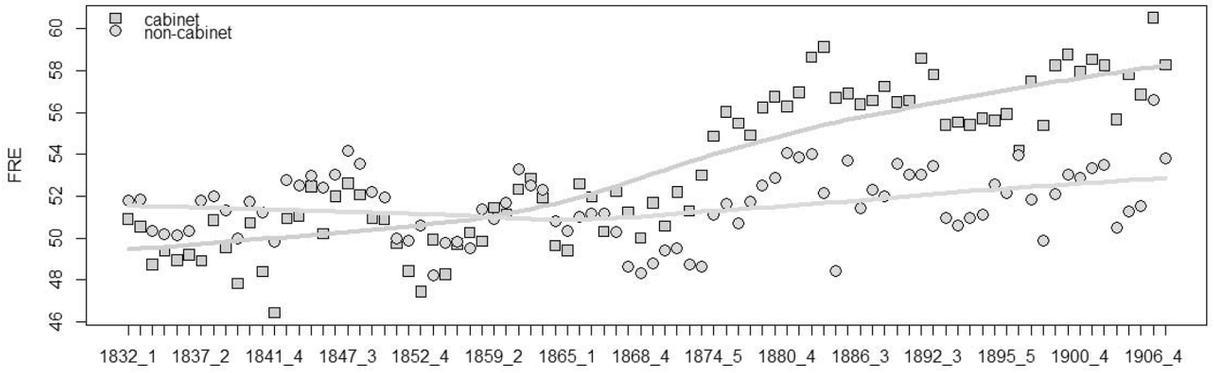

Figure 2. Flesch readability score (mean) over time, for both cabinet and noncabinet MPs. Solid lines are from loess regressions

on their session numbers (with the first session of our data being session "1," the second being session "2," and so on) while simultaneously estimating the break points in this relationship in the sense of Bai and Perron (2003) (as implemented by Zeileis et al. 2002). Using standard defaults, we obtain one break point as the optimally fitting model with that break dated at the first session of 1868—in line with our theory.[18] We consider the robustness of change point in more detail after introducing our "main" regression results below.

This prima facie evidence is helpful and is in line with our main hypothesis. Nonetheless, readers may reasonably object that it is inefficient and possibly misleading to break up the data on a session-by-session basis, especially if subsequent structural break tests ignore the estimation uncertainty in the coefficients—as they do here. A more philosophically appropriate test then is to combine all the sessions and assess the possibility of time-specific effects directly. With that in mind, we now re-estimate the regression with the inclusion of an interaction term involving the product of a member's cabinet status and a dummy that takes the value "1" for any session after the 1868 election—the point at which we hypothesize the change occurred.[19] We do this with and without the controls. The results of those regressions, with standard errors again clustered by MP,[20] are presented in table 4.

For completeness, we begin with the version with the controls (second column). Note first that there is no statistical significance attached to the "Liberal MP" variable: that is, the party identification of the member does not seem to be systematically associated with complexity. The same goes for "competitiveness" of constituency, which is a measure of the average number of candidates running in a seat at the general election (and has a mean of around 1.5 for our period). We observe that the coefficient on "word count" (literally, the number of words in the speech) is statistically significant but negative: that is, longer speeches are (on average) easier to comprehend than shorter ones. Before getting to the main variables of interest, we note that the two models have essentially identical fit statistics: the adjusted-R^2 of the restricted model is 0.0084 while adding the extra variables on the right-hand side pushes this only to 0.0103. We thus focus on the simpler version—without controls—for interpretation purposes since those extra variables add little to the analysis (implying that our main finding below is reasonably robust).

Putting aside the uncertainty estimates for the moment, we see that the point predictions are as we would expect given our hypothesis. A minister prior to the Second Reform Act has a lower average comprehensibility ($\hat{y} = 50.6$) than one serving after that date ($\hat{y} = 56.6$), and the difference is around 6 points on the FRE scale. By contrast, the difference for a backbencher between serving before ($\hat{y} = 51.4$) and after ($\hat{y} = 52.1$) is around 0.7 of a point on the scale.

18. See appendix C for the full model comparison table.

19. To clarify, the regression being estimated has as its dependent variable the FRE score of the ith speech and may be written as

$$\text{FRE}_i = \alpha + \beta_1 \text{cabinet}_i + \beta_2 \text{reform}_i + \beta_3 (\text{cabinet}_i \times \text{reform}_i) + \gamma \mathbf{Z} + \varepsilon_i,$$

where cabinet_i is the cabinet membership status of the MP in the ith observation, reform_i is a dummy pertaining to whether the MP is speaking before or after the 1868 election, and $(\text{cabinet}_i \times \text{reform}_i)$ is simply the interaction of the two; $\mathbf{Z}$ refers to a possible set of control variables for which γ is the coefficient and ϵ_i is an error term. Written this way, the regression may be interpreted as a "difference in differences," in the sense that attention focusses on comparing the magnitude of change in cabinet behavior before and after the reform with the (presumably smaller) change in noncabinet behavior over the same period.

20. Note that we do not use MP-level fixed effects due to the fact that only 3% of our MPs ever change roles; that is, the cabinet variable takes one value for almost all MPs at all times meaning that fitting fixed effects would generally not allow one to estimate the effects of cabinet vs. noncabinet status.

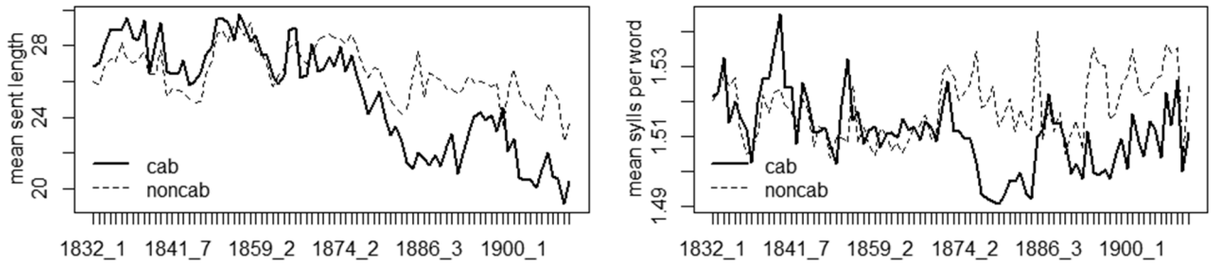

Figure 3. (Mean) sentence length and (mean) syllables per word, cabinet vs noncabinet

Clearly, in both absolute and relative terms, cabinet members went through a larger positive shift in their linguistic behavior than their noncabinet colleagues and the implied difference in differences is around 5 points. To get a sense of the substantive effect of the reform, consider a hypothetical backbencher promoted to the cabinet in the first session of 1868. The coefficients imply that he would make his average speech ceteris paribus around 8% easier to understand (on the FRE scale) relative to his colleagues still on the backbenches. This is nontrivial, corresponding with a change from around the 48th to the 60th percentile in the score distribution (for speeches made after the 1868 election).

Returning to uncertainty estimates, in figure 7 we provide an estimated marginal effect plot (taking into account the standard error clustering), and we see that our priors find support: while ministers—if anything—are slightly less comprehensible relative to backbenchers prior to reform, they are clearly more understandable after.

Robustness: Timing and data quality

Above we showed that when looking session by session, the immediate period after the 1868 general election was a break point (indeed, the only break point) in the time series. We conducted several further tests on our data—described in some detail in appendix C—to corroborate this claim and to rule out possible "pretrends" in the time series. First we re-estimated our "main" regression model based on data "local" to the hypothesized change point initially using five sessions before and after the first session of 1868 and then 10 sessions before and after. The central findings on the difference in the differences between cabinet and noncabinet MPs remains intact. Second, we estimated a simple regression of FRE on cabinet status for all data prior to the first session of 1868; the results implied that, if anything, cabinet speeches tended to be less comprehensible than noncabinet speeches prior to the Second Reform Act. Finally, we perform an explicit "placebo" test by treating the last session of the 1865 parliament (i.e., prior to the electoral reform) as a proposed change date. The regression that resulted had a similar but lower adjusted-R^2 than the original model, thus leading us to conclude that it does not offer a more plausible period for any break in the data generating process that occurred.

A separate issue for our analysis is the fact that FRE scores are, in general, much more variable for short speeches than long speeches. This is unfortunate from a statistical perspective because it is presumably the shorter speeches that contribute most to key differences we observe between ministers and noncabinet members, while simultaneously these are the observations about which we are least certain in a sampling sense. With this in mind, we conduct four further versions of our "main" regression to verify that our conclu-

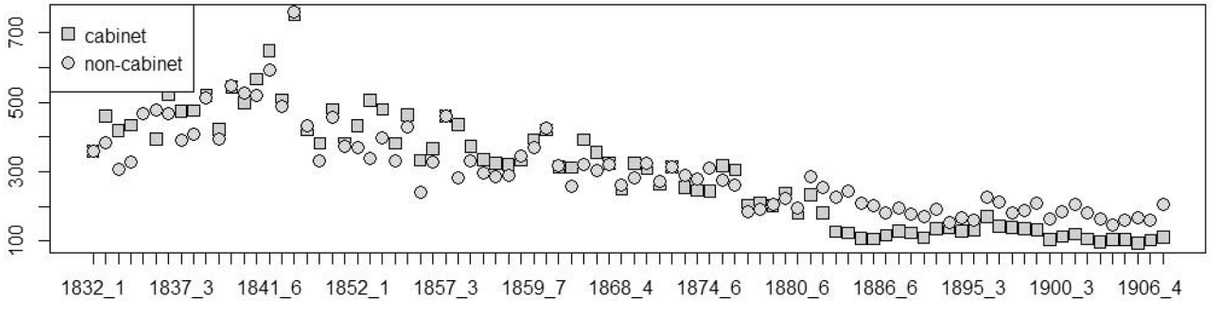

Figure 4. Average length of speeches for cabinet and non-cabinet MPs over time. The *y*-axis is the (mean) number of words per speech

Table 3. Samples of Speeches, from the First Session of 1885, with a Variety of Comprehensibility Scores: From the Least to the Most Easy to Understand

Word Count	Sentence Count	Syllables	Score	Role	Speech
44	1	86	−3.18	Noncabinet	Asked the Under Secretary of State for the Colonies, whether Her Majesty's Government have arrived at any practical decision with regard to the establishment of emigration bureaux for the purpose of promoting, as promised early in the Session, a systematic system of State-directed emigration?
28	1	45	45.45	Noncabinet	Can the right Hon. Gentleman give the House any information as to when the Royal Commission on the Depression of Trade is likely to report on this subject?
30	1	39	66.41	Noncabinet	Said, he wished to know how many years ago these bayonets were made, and whether the name of the firm and the date of their manufacture were stamped upon them?
11	2	15	85.89	Cabinet	I will consider that. I think there will be no objection.
9	1	10	103.70	Cabinet	That I cannot say til I have seen it.

sions regarding the impact of the Second Reform Act are robust. In the first two, we limit ourselves to short speeches (fewer than 100 words) and then long speeches (more than 100 words). In the third specification, we use only speeches that are not outliers.[21] Finally, we estimated a weighted regression where the weights are simply the length of the speeches. In appendix D, we report the results of these enquiries in more detail: it suffices here to note that the implied difference in differences between ministers and nonministers is robust in terms of the hypothesized direction.

Ruling out "new types"

Thus far, an implicit assumption for our work has been that the change to cabinet ministers' utterances was (primarily) a product of individuals responding to new incentives in the electorate. An alternative hypothesis is that, in fact, the Second Reform Act introduced new "types" of individuals to the House of Commons with different latent features and that it the changing make-up of the chamber that yields the results we saw above. There are at least two ways to investigate this possibility, to which we now turn.

First, we consider all individuals who served in a cabinet position at least once after the 1868 election, that is, after the Second Reform Act took effect. Using a paired *t*-test with ministerial office as the treatment, we compare their mean speech comprehensibility when in the cabinet with their average when serving as a backbencher. The mean difference uncovered is (an increase of) 2.58 on average, which is statistically significant ($p < .01$). Since this test keeps the individuals themselves constant, and combined with the fact that cabinet ministers serving in this period are not disproportionately more likely to have been elected to parliament after 1868 relative to backbenchers they serve alongside,[22] it provides strong circumstantial evidence that cabinet office (after 1868) had some effect regardless of the fixed characteristics of the MPs involved.[23]

To put this finding on even surer footing, we now turn to a more systematic study of fixed effects. In particular, restricting the data to cabinet members, we regress the session mean comprehensibility score on a session dummy and then on a session dummy plus fixed effects for the MPs. The idea here is that if the regression with the MP fixed

21. A speech's length is an outlier if it is greater than the upper quartile multiplied by 1.5 times the interquartile range or less than the lower quartile multiplied by 1.5 times the interquartile range.

22. To clarify, we potentially have confounding here if ministers in the post-1868 period are more likely than contemporaneous backbenchers to have been elected in or after the 1868 election. This is not the case: if anything, the reverse is true—67% of the cabinet ministers had their first session in parliament after the 1868 election, compared with 93% of those not serving in the cabinet.

23. To verify that, in fact, joining and leaving the cabinet had the expected effect on a given (well-known) individual, we investigated the case of William Gladstone—who spent six decades in the House of Commons. Our findings for Gladstone are generally in line with our theory, especially after the Second Reform Act. See appendix F for more details.

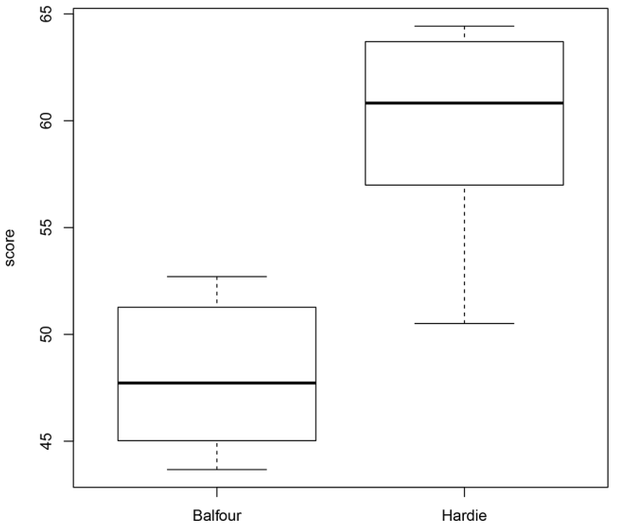

Figure 5. Boxplot showing difference between complexity of works by Keir Hardie (Labour party) and Arthur Balfour (Conservative party).

effects has different coefficients for the time dummies, we may conclude that the latent features of individual cabinet members are important for explaining the data we saw. In figure 8 we present a plot of the coefficient on the session dummies for both regressions, with their 95% confidence intervals. The dashed lines represent the intervals for the fixed effects case, and the solid lines are those without member effects. In every case, these intervals overlap: that is, we have no evidence that adding member fixed effects matters relative to the more general time dynamic portrayed above.

Ruling out "new roles"

Another plausible mechanism for the decreasing relative complexity of cabinet speeches—separate to any pressure from voters—is that ministers began to operate in a new legislative environment incidental to the Second Reform Act, and that changes to the nature of ministerial speech are an artifact of these fresh organizational imperatives. It is certainly true that the historical literature has discussed the nineteenth century as a period in which the Commons' agenda altered. On the other hand, those events do not line up with 1868 as a change point: indeed, it the 1880s onward that are typically emphasized. For example, until the 1880s, questions to ministers came before all other business, but from 1881 questions to the Prime Minister came at the end of the day (Jones 1973). Furthermore, 1882 saw the advent of a new Speaker power to "close" debate in response to Irish obstructionism at that time (e.g., Dion 1997). By the turn of the century, government dominance of the agenda was essentially in its modern form as a result of Balfour's "railway timetable" reforms (see discussion in Chester and Bowring 1962).

One way to assess possible changing roles for the cabinet—including more agenda control—is to analyze their relative share of speeches in the House of Commons over time. In figure 9 we do just that. There the thick solid line is the proportion of speeches made by the cabinet; the dashed line corresponds to those made by backbenchers. We would be concerned if, around 1868, there was a sudden and permanent increase or decrease in these relative quantities. This is not the case. Indeed, examining the time series for change points (in the sense of Bai and Perron 2003) reveals that if there is a break in the data generating process, it occurred at the first session of 1885. Visual inspection of the figure suggests the same finding: one can readily see that the cabinet enjoys more "air time" after that session.

While this is heartening news for the originally proposed causal mechanism and its origins in the Second Reform Act, it may nonetheless be the case that this late century uptick is driving the main post-reform act result from our regressions. To check this, we exclude all data from the first session of 1885 onward and rerun our analysis for the shorter period. Comfortingly, the results are essentially identical to

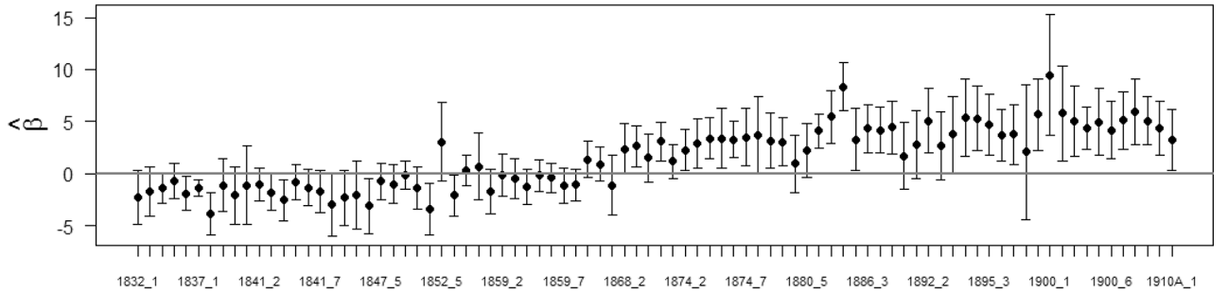

Figure 6. Estimated $\hat{\beta}$ on cabinet status in session-by-session linear regressions (with 95% confidence intervals). Solid horizontal line marks zero. Note the general rise in coefficients from below to above zero, around 1868.

Table 4. Table of Estimates for Regression of Comprehensibility of Speech on Cabinet Status and Time Dummy (for Second Reform Act) and Interaction between the Two—with and without Controls

	Reform Act Interaction	With Controls
(Intercept)	51.3976 ***	51.5920 ***
	(.2176)	(.5634)
Cabinet member	−.8189*	−.7803
	(.4176)	(.4271)
Reform Act dummy	.7172*	.5172
	(.3371)	(.3591)
Cabinet × Reform Act	5.3060 ***	5.2251 ***
	(.7195)	(.7172)
Liberal MP		.4511
		(.3785)
Word count		−.0013 ***
		(.0001)
Competitiveness		.0230
		(.3705)
N	670,091	670,091
R^2	.0084	.0103
Adj. R^2	.0084	.0103

Note. Dependent variable is comprehensibility of speech. MP clustered standard errors in parentheses.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

those from our "main model" above and are displayed (with clustered standard errors) in appendix E.

A second way to assess a possibly "new" role for ministers immediately after 1868 is to consider the order of speeches, in terms of the types of members making them, around the time in question (see Eggers and Spirling [2014a] for a similar approach). We estimate a series of logistic regressions with each speaker's (binary) cabinet role predicted by the previous speaker's role. We then calculate the predicted probability that a minister speaks after a nonminister for each session. If this probability changes in a "once-and-for-all" way after the Second Reform Act, this implies that ministers are fulfilling a different role—either engaging somewhat more or somewhat less in floor debate than previously. Fortunately, this is not what we find: in figure 10 we report the predicted probabilities and their confidence intervals for the period between the 1852 and 1880 general elections. Crucially, the sessions before and after the 1868 election have very similar point estimates, with confidence intervals that often overlap.

All told then, we find little evidence—historical or empirical—of contemporaneous changes to ministerial roles that render as spurious our "main" results above.

DISCUSSION

Observers of modern democracies speak anxiously of the "dumbing down" of political discourse (Lim 2008). They fear that important yet subtle debates and distinctions are increasingly lost and that elected officials no longer lead opinion in a thoughtful way. A more optimistic take on recent trends is to regard the simplification of political language as helpful (or perhaps vital) for the engagement of citizens with increasingly constrained time budgets and interests outside of governance (e.g., Temple 2006). Whatever the truth, as political scientists we have strong reasons to hope and to believe that politicians respond to voters as much as voters respond to their representatives. This is true in both theory (e.g., Meltzer and Richard 1981) and in empirical work (e.g., Canes-Wrone, Brady, and Cogan 2002) that stresses the importance of congruence between the preference held by constituents and the actions taken by politicians. A natural consequence of this logic is that when new types of citizens join the electorate—in the modern period, typically via immigration (e.g., Tam Cho 1999)—officeholders will compete for their support and alter their platforms in a way that reflects this underlying change.

Here we studied this very broad phenomenon for an historically important period: the Victorian age of democratization in Britain. Unlike other studies that relied on shorter periods or coarser data at higher levels of aggregation, our findings were unambiguous: cabinet members, after a doubling of the electoral roll in 1867, began to make parliamentary speeches with different properties than before. In particular, entirely in fitting with predictions from the literature, their utterances became easier for the median member of the electorate to understand. This median voter was a man of the working class, with less access to education

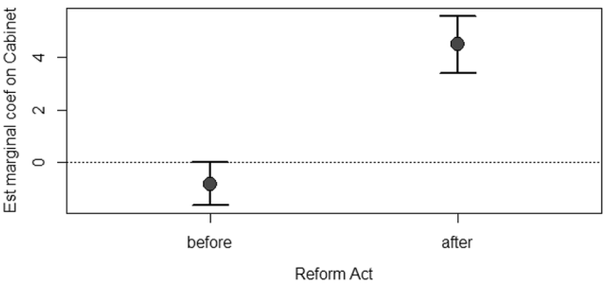

Figure 7. Estimated marginal effect of cabinet membership on speech comprehensibility, before and after the Second Reform Act.

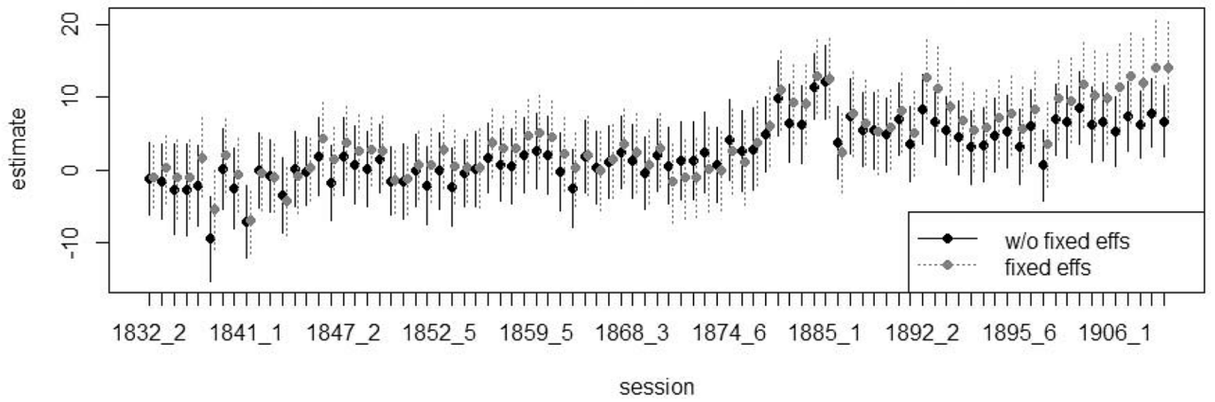

Figure 8. Comparing model with and without member fixed effects; *y*-axis is $\hat{\beta}$ on the relevant session dummy; *x*-axis denotes session. Vertical lines denote 95% confidence intervals.

(and literacy) than had previously been the case in the electorate. As far as we know, our article is the first to provide systematic evidence of a "reform effect" on the language used in parliament. Crucially, we noted that this change was not due to new types of MPs—with different priorities or experiences—entering the Commons after electoral reform. Indeed, our auxiliary analysis suggested that it is the same members acting in new ways upon finding their way to the front bench that is responsible for the decrease in complexity in speeches.

As is inevitable with observational data, it is no easy task to be confident about the causal process that undergirds an empirical pattern. Certainly, our findings are not artifacts of superficial changes to speech records; for example, it is not simply that ministers make shorter speeches over time, which is then picked up (artificially) in our complexity metric. Furthermore, we have reason to believe that incentives to simplify presentation are strongest for cabinet members; in Westminster systems they are held accountable for government policy, and their performance—especially on the economy—is the best predictor of future general election success. Unsurprisingly then, it is ministers who most sought to appeal to voters. On the other hand, showing evidence consistent with a theory is not the same as showing that the theory is correct. In particular, we do not know whether ministers consciously altered their linguistic style and what precisely the impetus for this was: perhaps Prime Ministers such as Disraeli and Gladstone, who seemed acutely aware of the new electoral calculus (see, e.g., McLean [2001] on Disraeli's introduction of a new dimension to British politics), took the lead and advised their colleagues to speak more simply (or promoted those from the backbenches that could). No doubt there was a period of ministerial "earning" as the effects of the Reform Act in the constituencies became clearer over time. Alternatively, the stimulus may be been less direct—perhaps a result of civil service professionalization and the increasing role of the bureaucracy in serving and advising ministers in terms of their relationship with the

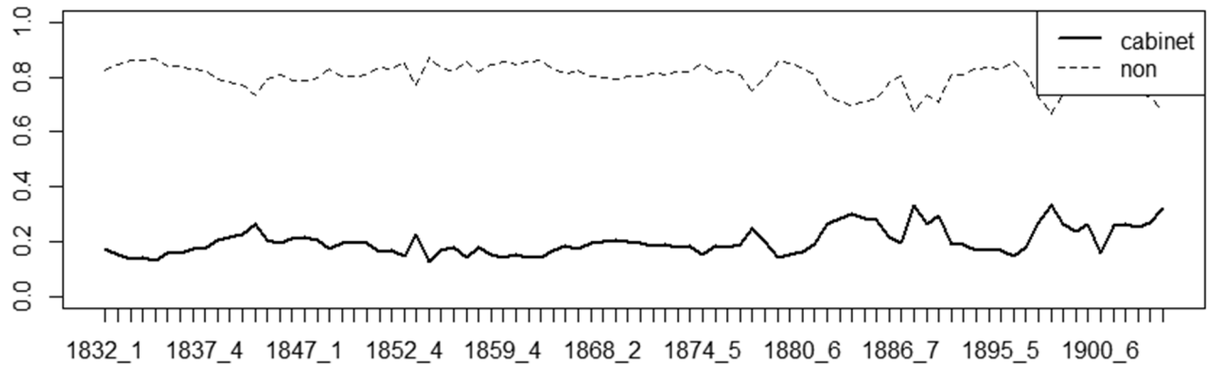

Figure 9. Proportion of all speeches in House of Commons made by cabinet and noncabinet members over time

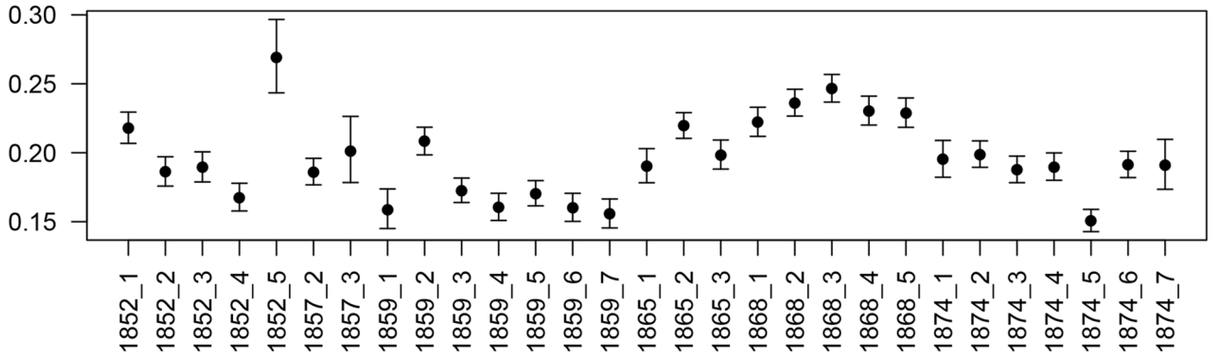

Figure 10. Probability that a minister speaks immediately after a nonminister for the period between the 1852 and 1880 general elections

House.[24] Finally, it is possible that the relative decline in ministerial complexity is a consequence of a more direct and aggressive ministerial questioning dynamic as the opposition front benches employ interrogatives that are inevitably longer than the "punchy" answers they receive.[25] Understanding the precise mechanism requires more fine-grained data than we have here,[26] though studying modern speech writing and speech giving by politicians may help us understand how they think about the audience to which they must appeal.

Moving beyond the United Kingdom, similar methods might be useful for studying, say, the development of the "Second Party System" (see, e.g., Jenkins and Stewart 2012) and "Jacksonian Democracy" in the United States with its new emphasis on voters over political elites. In particular, researchers might explore whether the latter increasingly spoke in ways comprehensible to the median elector at this time. Of course, the tone or complexity of speeches is only one part of what it means for parliamentarians to be "responsive" to voters. More important for material welfare is policy. Here the extent of linguistic complexity is likely less helpful than a study of both topics of debate (as in Quinn et al. 2010) and of bills that became acts (relative to those that did not). Again, textual methods can be helpful, and the speeches and related data we have used provide the beginnings of a resource to get at such quantities of interest. We leave such efforts for future work.

24. E.g., Richard Crossman argues that ministers dealing with their civil servants find themselves constrained in terms of the plausible policy choices they may chose (and presumably then present to the electorate; see Crossman 1975).

25. We do not mean that the simplification of language by cabinet members is a mere "artifact" of the rising importance of questions-and-answers: rather, the intended inference is that this new speech dynamic allowed central actors to appeal to the new electorate and in so doing rewarded shortened, simplified, robust partisan points over more long-winded, philosophical inquiries.

26. Including membership information for the Shadow Cabinet, which was not recorded during the Victorian period.

ACKNOWLEDGMENTS

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