Party Cohesion in Westminster Systems

Inducements, Replacement and Discipline in the House of Commons, 1836–1910*

Andrew C. Eggers† Arthur Spirling‡

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Abstract

We consider the historical development of a characteristic crucial for the functioning and normative appeal of Westminster systems: cohesive legislative parties. To do this, we gather the universe of the twenty thousand parliamentary divisions that took place between 1836 and 1910 in the British House of Commons, construct a voting record for every Member of Parliament serving during this time, and carry out analysis that aims to both describe and explain the development of cohesive party voting. In line with previous work, we show that—with the exception of a chaotic period in the 1840s and 1850s—median discipline was always high and increased throughout the century. We use novel methods to demonstrate that much of the rise in cohesion results from the elimination of a rebellious 'left tail' from the 1860s onwards, rather than central tendency shifts. In explaining the aggregate trends, we use panel data techniques and note that there is scant evidence for 'replacement' explanations that involve new intakes of members behaving in more disciplined ways than those leaving the chamber. We offer evidence that more loyal MPs were more likely to obtain ministerial posts, and speculate that this and other 'inducement'-based accounts offer more promising explanations of increasingly cohesive parties.

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†Lecturer, London School of Economics. email: a.c.eggers@lse.ac.uk

‡Associate Professor, Department of Government, Harvard University. email: aspirling@gov.harvard.edu (submitting and corresponding author for journal)

1 Introduction

Strong party discipline is a defining feature of legislative politics in Westminster systems (Lijphart, 1999; Spirling and McLean, 2007; Kam, 2009*b*). In modern times, parliamentary divisions ('roll calls' in American parlance) are characterized as predictable affairs insofar as members of parliament (MPs) can be expected to vote in line with their parties' explicit orders, and rebellion against the 'whip' is a relatively rare event (see Cowley, 2002, for an overview). One consequence of party cohesion, in tandem with an electoral system that yields a 'manufactured majority' of seats (Bogdanor and Butler, 1983), is that governing parties almost never lose votes important to their legislative agenda. This arrangement facilitates the particular vision of "responsible" government (Birch, 1964; Gamble, 1990; Campbell and Wilson, 1995; Richards and Smith, 2002) and "accountability" (in the sense of Powell, 2000) that has allowed unusual longevity and diffusion of the Westminster system from its home in Britain to other polities (Rhodes and Weller, 2005; Rhodes, Wanna and Weller, 2009; Godbout and Hoyland, 2013).

Given the centrality of cohesive parties to both the functioning of the Westminster system and to the normative justification of its use, it is unsurprising that a great deal of political science attention has been lavished upon them. Starting as early as Lowell (1902), scholarship has focussed on the 19th century as a transitional time during which party cohesion—and thus the Westminster system—took hold in the House of Commons (e.g. Bagehot, 1873/2011; Trevelyan, 1922; Aydelotte, 1963; Berrington, 1968; Cromwell, 1982; Cox, 1987; Jenkins, 1996; Rush, 2001; Schonhardt-Bailey, 2003). While researchers have come to a clear consensus that discipline did indeed increase, the central question of *why* MPs became more cohesive has lacked a convincing answer. Though we are not without theories (with Cox, 1987, being perhaps the most comprehensive), demonstrating conclusively which particular mechanism explains the patterns we see has remained an elusive goal. To the extent that understanding parliamentary discipline in modern times is an important goal in political science (e.g. Norton, 1978; Diermeier and Federsen, 1998; Cowley, 2005; Benedetto and Hix, 2007) and to the extent that modern politics relies upon it (e.g. Economist, 2012), there is obvious interest in improving on this state of affairs. This paper represents such an improvement.

At its core, the question is this: did parties become more cohesive primarily because MPs who were previously less disciplined became more disciplined as the century progressed; or was the increase in cohesion due to more disciplined 'types' of individuals entering the Commons while less disciplined 'types' left it? The former story implies that change was mostly due to factors acting on already existing members—like increasingly efficient whipping, a greater sense of solidarity, better agenda control by leaders, or responsiveness to a party orientated electorate. We denote this an 'inducements' account, insofar as extant members were encouraged to behave differently. The latter story suggests that it is newcomers to the chamber who brought with them cohesion, while a rebellious 'old guard' were replaced by natural wastage. We denote this a 'replacement' account, insofar as it is turnover of latent traits that drives any changes in cohesion. It is both fundamental and difficult to determine how much each of these mechanisms contributed to the development of cohesive parties in the House of Commons. That previous work has been unable to decisively resolve the issue is in part due to methodological shortcomings, but mostly due to issues of data availability. Here, we solve both problems using novel statistical methods, panel data analysis, and a new collection of roll call records.

Our data consists of every one of the 20,262 roll calls cast between 1836 and 1910. We have a record of *every* member's decision on *every* division. By combining this voting information with a new data set that incorporates extensive individual-level covariates on the thousands of MPs serving during this time, we are able to explore discipline in more nuanced ways than previous studies that used aggregated statistics (e.g. Lowell, 1902; Berrington, 1968; Cox, 1987) ever could. Similarly, we are able to chart the changes to cohesion over a much broader period than the intensive, but time-limited studies (e.g. Adyelotte, 1954; Cromwell, 1982; McLean, 2001; Schonhardt-Bailey, 2003) preceding ours. In short, we have the best of both worlds: micro-data as a long time series. In the first part of the paper, we characterize cohesion over time using 'relative distribution methods' (Handcock and Morris, 1998, 1999) that give a more complete account of how cohesion changed from parliament to parliament than is possible with means and standard deviations; these methods

are both non-parametric and robust to outliers (a common occurrence in histories of legislative voting). In the second part of the paper we use panel data methods to assess the possible mechanisms by which the changes in cohesion actually occurred.

We find that voting cohesion began high in the 1830s, waned in the 1840s, and then grew thereafter. This observation is commensurate with earlier accounts, though we are able to be much more precise in our discussion of changes. Importantly, we show that cohesion was always relatively high in absolute terms for the average MP, and that, by the 1860s, almost all growth in aggregate party-line voting is due to the disappearance of a rebellious 'left tail' from the distribution of cohesion. We then show that this 'folding in' of the rebel tail is *not* due to new blood entering the House of Commons or from older less-malleable types retiring out. Instead, we demonstrate that sitting MPs became more cohesive as the Victorian age wore on, examining ministerial promotion as one 'inducements'-based explanation for increasingly cohesive voting. This pattern is consistent with theories of 'endogenous' change, whereby internal reforms and incentives (in the sense of e.g. Cox, 1987; Kam, 2009*b*) matter more than sociological adjustments to MP stock (in the sense of e.g. Rush, 2001).

Our paper continues as follows: in Section 2 we review the literature pertinent to the question at hand and set up our empirical tasks; in Section 3 we introduce the data with which we will address those tasks. In Section 4 we use relative distribution methods to characterize the change in cohesion over time. In Section 5 we assess the role of replacement and in Section 6 we evaluate one possible 'inducements' channel. Section 7 concludes.

2 Literature and Orientation

The period between the first Reform Act of 1832 and the fourth Reform Act of 1918 has been cited by scholars as the key period of development for the both the Westminster system and the British polity as a whole (e.g. Cox and Ingram, 1992; Jenkins, 1996; McLean, 2001; Rush, 2001). Central to this story is increased party cohesion in legislative voting, and scholarly interest has focussed on two

distinct but related questions therein. First, researchers have sought to document *how* roll call behavior changed in a descriptive sense; second, they have sought to understand exactly *why* it did so.

Scholars have described changing cohesion using different tools (e.g. Lowell, 1902; Berrington, 1968), typically concentrating on aggregate measures like 'proportion of divisions in which the two (major) parties opposed one another', or on simple averages—like the proportion of divisions in which given MPs voted with their whips (e.g. Cox, 1987). Cox (1987, 26) summarizes the consensus finding of the literature when he notes that "For the Conservatives, there is a sharp decline [in the 1830s and 1840s]... a plateau in the 1850s; and a sharp recovery [later]..." Meanwhile, "[f]or the Liberals, there is a much smaller decline... followed by an erratic increase". While univariate averages are helpful for outlining general trends, using them alone conceals other potentially important distributional features that characterize the nature of the change over time. For example, measures of central tendency say nothing *per se* about the relative proportion of the most and least cohesive members, nor about the magnitude of their loyalty and rebeliousness respectively. This means, for instance, that we cannot know whether a period of 'low' party unity is due to a chamber of mostly 'independent' voting haphazardly from the whips' perspective (Jenkins, 1996, Ch3) (resulting in a high variance spread around the mean), or due to a small but persistent group of 'rebels'—such as the 'Fourth Party'—who actively seek to vote against the party line (a left tail that drags mean cohesion down). In short, being able to describe the evolving features of roll call voting in a more complete, subtle and precise manner allows researchers to know both 'where to look' for possible causal relationships, and how to interpret any evidence they do find.

Whatever the descriptive statistics, it is unclear exactly *why* cohesive parties increasingly became the norm. Explaining the pattern obviously requires understanding how MPs both sorted themselves into parties and why they behaved the way they did once sorted. As previewed above, one way to organize work on emerging unity is into 'inducement' or 'replacement' accounts, the former dealing with encouragements for MPs with given (possibly latent) characteristics to behave in different ways, the latter focussing on the role of new MPs entering the chamber over time. Scholars

in the 'inducement' tradition have, for example, emphasized the importance of material interests as prompts for voting in the 1840s (Adyelotte, 1954; Schonhardt-Bailey, 2003) with others noting the importance of rhetorical manipulation in the same period (McLean, 2001). Related literatures point to electoral reform as ushering in the "triumph of partisan politics" in the later Victorian era, with polarization of the electorate having knock-on consequences for the British House of Commons itself (Jenkins 1996, see also Cox and Ingram 1992). With a view to the broader time period and more attention to internal reform, Cox (1987) provides a rare example of work that relies on data collected over a relatively long period to link external changes to suffrage and individual motivations for division behavior. His particular emphasis is on Cabinet promotion opportunities as a return for good behavior in the Commons, and is perhaps the prototypical 'inducement' account (see e.g. Benedetto and Hix, 2007, for modern work echoing the central point). Connected to such reasoning is a broader literature emphasizing the importance of campaign finance arrangements in a more modern democratic age (Hanham, 1978; Kam, 2009*a*; Stokes, 2011) and the need to avoid falling into bad odor with a central party administration.

In contrast to these researchers, scholars of 'replacement' have pursued a different tack, noting changes in the personal socioeconomic circumstances of MPs during this period, without particular attention to 'interests' *per se*. Rush (2001), for example, describes the pronounced decline in members with aristocratic links as the century wore on (see also Cannadine, 1996), and their commensurate replacement with businessmen and representatives of the (legal) professions (see also Lawrence, 1998, on the changing demographics of radical MPs). The fundamental logic of such work appears in some of the accounts above too, albeit implicitly: Cox (1987, 68–79), for example, notes the importance of 'ministerial ambition' in explaining why men would seek to enter parliament in the second half of the Victorian age, with obvious implications for 'types' of MPs likely to appear over time. Note too that accounts that rely on period-specific or class-specific inducements, like Schonhardt-Bailey (2003), would presumably accept that as different individuals with different interests enter the Commons, voting behavior is likely to change. In part due to its obvious simplicity and plausibility, 'replacement' stories have featured in accounts for roll call behavior change

well beyond Westminster systems: the 'conditional party government' model (Aldrich and Rohde, 2000) relies in part on a post-Watergate surge of liberal Democrats in the House replacing more conservative southern representatives.

For foregoing scholars, deciding which account of the changes is more plausible (or in what proportion)—'inducements' vs 'replacement'—was a tall order since it implicitly required records of individual MP behavior, over a long time period. That is because the debate can only be resolved by knowing whether MPs became more disciplined over their career in the House, and whether or not that evolution outweighs (in a statistical sense) the effect of new members entering the Commons who were 'already' more cohesive in type. Put more simply, we need panel data and previous (sometimes Herculean) efforts have either limited the scope to one or two parliaments with records at the MP-level (e.g. Adyelotte, 1954; McLean, 2001; Schonhardt-Bailey, 2003), or have looked at a longer time period in aggregate terms without attention to unit (MP) level variation (e.g. Lowell, 1902; Berrington, 1968; Cox, 1987). Studies that collect disaggregated voting data over a considerable period (e.g. Cromwell, 1985) do so by covering only certain years or certain divisions.

As we explain in the next two sections, we address shortcomings of previous work in terms of descriptive analysis and inference: we bring to bear the entire universe of MP decisions on roll calls for the period 1836–1910 and new methods to study them. This allows us to better present the changing pattern of aggregate behavior, and to model cohesion 'within' MP careers. Thus, if we believe that latent preferences are essentially fixed, and we can show that cohesion increases are not the product of replacement effects, we can go some way to showing that an inducements story—in which whipping, rhetoric, agenda control, leadership and other such factors became more powerful motivational tools—is more convincing. More formally, our intention is to

1. fully characterize the changes in party division behavior during this period, using methods that allow us to comment on the 'overall' nature of unity over time, and the relative and absolute solidarity of the most and least cohesive Members;

2. examine the association between being entering or exiting the House of Commons and voting

cohesion. If the replacement story has merit, fresh entrants ought to, *ceteris paribus*, be more disciplined than the mass of colleagues they join who are already incumbents and departing MPs ought to be less so;

3. examine the association between seniority and cohesive voting for individual MPs over time. 'Inducements' theories imply that the average individual MP should vote more in line with his party as his career progresses from parliament to parliament; if 'replacement' theories have merit, this seniority effect should be small relative to the increase in average cohesion across members from parliament to parliament;

4. examine with more complete data a question previously raised and studied by Cox (1987): did party cohesion increase partly because ministerial posts were offered to more loyal party voters? [1]

We explain each of these tasks more fully in subsequent sections. Before doing so, note that while we recognize there are other inducement mechanisms for good behavior, and in the Discussion we return to what those might be, this paper focusses particularly on ministerial promotion for several reasons. First, because both the theory (e.g. Dowding, Berlinski and Dewan, 2012) and empirics on cabinet personnel management (e.g. Benedetto and Hix, 2007) are well studied, and part of a mature literature thought to explain much of the discipline seen in the House of Commons. Second, despite some pioneering efforts to measure 'leadership' via less observable channels like the use of rhetoric (e.g Moser and Reeves, 2013), obtaining reasonable estimates of the effects of other inducement mechanisms is prohibitively difficult—especially for historical data.

[1] In modern politics, the threat is not simply that an MP may not be promoted for shows of disloyalty, it is that one may be deselected as a candidate. For the period under study, assessing such a threat is very difficult because we do not have the quantity or quality of historical records necessary to know why candidates ran (or did not run) for office at particular elections. Nonetheless, it is perhaps worth noting as *per* e.g. Cox (1987) that the effect of local or central party organizations on candidate selection was not a strong influence until the start of the twentieth century (see also Hanham, 1978, ch 7).

3 Data

The current project requires roll call votes of MPs in the House of Commons, or 'divisions' as they are known for this particular legislature. As noted, several authors have collected roll calls for the 19th century prior to our efforts, but we are much more comprehensive in both depth and breadth. In particular, we gathered every individual roll call vote for the entire period 1836–1910.

Our raw data was supplied by the *History of Parliament Trust*, which had commissioned a major digitization project to convert published division lists into machine-readable data. We linked MPs to covariates in a relational database, as described in Eggers and Spirling (Forthcoming*a*), and we were compelled to undertake substantial disambiguation of names such that the roll calls could be used for the purposes below. Appendix A provides more details.

For reasons that will become clear below, we required the identity of the head ('Chief') whips for the two major parties (Liberals and Conservatives for this period) at any given time. We found annual listings of their identities in Cook and Keith (1975) and Butler and Butler (1994) but the precise resignation and promotion dates for these officers is unknown. While it is generally the case that government Chief Whips are made parliamentary secretary to the Treasury, obtaining dates for this office proved no easier. And, in any case, there is no such aliased information for Chief Whips of non-government parties at this time. Some scholars have made efforts to expand and refine this information (e.g. Sainty and Cox, 1997), though in such schemes (educated) speculation is required to 'fill in' missing details, especially for secondary whips assisting the head official. As a result, we occasionally found months of roll calls within years in which the particular whip cited in aggregate records did not appear to be serving as an MP (and we therefore assume he was not in fact whip during this period). To circumvent this problem, we simply assumed that anyone who at some point served as a Chief Whip during the parliament in question represented the 'party line' when voting in the way we specify below—that is, as a 'teller'. Thus, if there were two Tories serving as Chief Whips in Parliament A, we check the names of both against the relevant division lists (actually, the tellers) in order to update our understanding of the Conservative party position

on a given vote. Empirically, treating all Chief Whips who served in a period as embodying the party line on a given day may be correct or not (there is no way of knowing with current data), but it is unlikely to cause problems for our measure below since, presumably, those who become Chief Whip vote in a way congruent to that office immediately preceding or after their occupation of it.[2]

A natural concern for scholars of parliaments pertains to the selection process by which propositions make it to a vote at all. By definition, our division records from the *History of Parliament Trust* do not include those unobserved cases which were put to a voice-vote (only), or were never pursued in the lobbies. It is certainly the case that there are both more divisions over time, and that there are more divisions that the government (and the opposition) whip over time: starting with around 30% of the 300-700 divisions in the early period, rising to around 70% of the 700 or so votes per parliament by the 1850s, and up to 90% of 1500-odd divisions by the end of the data. Ultimately though, notice that our approach compares only like-with-like: that is, we are calculating the cohesion of parties based on those votes on which the Chief Whip acts as a teller. Thus, while it is true that 'private members' found it much harder to introduce business after the Cabinet's agenda reforms during the 19th and early 20th centuries (see Cox, 1987, ch6), any private member bill without explicit government backing should not appear as one of our data points.

3.1 Measuring Cohesion

Measuring the cohesiveness of voting in British parliamentary parties is a long-standing methodological problem (see, e.g., MacRae, 1970, for an overview). In contrast to most work on the subject, our goal in the current paper is to shed light on the way that *individuals* behave with respect to their parties, and not on the *aggregate* behavior of parties *per se*.[3]. We begin from the premise that an MP votes in line with his party when he votes in line with his party Chief Whip acting as a 'teller' for that division (see Cox, 1992, for discussion of this indicator of discipline). Our measure

[2] In principle, we might have situations where Chief Whips 'tell' in opposite directions, though this doesn't seem to be the case from inspection of our records, so we are not concerned with miscodings in this sense.

[3] In this literature, the Rice Index is the most commonly used such approach, defining the discipline on division i as $\frac{|\text{ayes}_i - \text{noes}_i|}{\text{ayes}_i + \text{noes}_i}$ (see Desposato, 2003, 2005; Hix, Noury and Roland, 2005, also). In early work, Lowell (1902) uses the proportion of roll calls in which at least 90% of one party are voting in opposition to 90% of another.

of cohesion for a given MP at a given time is a simple proportion: the number of times he votes the same way as his whip ('aye' when the whip votes 'aye', 'no' when the whip votes 'no'), divided by all the times both he and his whip state a preference (i.e. we ignore missing values in terms of calculating both the numerator and denominator of this measure. A 'discipline distribution' or 'cohesion distribution' as alluded to below is then just the aggregated set of all these individual scores, for all the relevant MPs in a session. We give more details of our measurement procedure in Appendix B.

Implicit here is the assumption that the Chief Whip receives orders directly (or as if directly) from his party leader. It could conceivably be the case that a particular roll call was not whipped, yet the Chief Whip chose to vote as a teller. In that case, voting with the Chief Whip overstates the degree of discipline pertaining to a specific MP. But such occurrences are likely to be extremely rare, since Chief Whips are agents of leadership throughout this period and unlikely to waste time on more frivolous matters that come up for roll call votes. Alternatively, it may be the case that, on a particular whipped roll call, the Chief Whip (or one of his close associates) fails to act as teller for some idiosyncratic reason, yet gives instructions to the MPs (which we cannot observe). To the extent that voting behavior in such circumstances is similar to that in other whipped votes (and/or these circumstances are rare), excluding these cases does not affect our results.

As our comments above imply, we restrict the analysis that follows to the two major governing parties at this time: the Conservatives and Liberals. While we have information on MPs from other parties, we typically do not know their whipping arrangements, and the process is especially complicated for some 'break-away' factions like the Liberal Unionists who spend some time in the 19th century voting with the Conservative whip. Thus our restriction avoids such ambiguities.

In Figure 1, we report boxplots representing the distribution of cohesion scores for MPs over time. Recall that the fundamental unit of analysis is a given MP's agreement with his whip (i.e. we are not plotting aggregated division-based scores here). The thick black bars represent medians,

and it is clear that while median behavior becomes less disciplined in the 1840s,[4] it approaches 1 thereafter, reaching its zenith at the end of the data.[5] The grey points represent outliers who are least disciplined: MPs whose record puts them into the tail of their parliament's distribution. Looking more closely, we see something of a discrete shift in cohesion around 1868, wherein the median 'jumps' to just shy of its maximum, while the implied spread of discipline begins to pack much more tightly around the average. While the pattern observed—more disciplined MPs packed more tightly around a higher median—is relatively intuitive, we wish to be more formal about the changes to the distributions. In the next section, we do just that.

4 Describing the Rise in Party Cohesion

Our first task is to compare the distribution of MPs, in terms of their obedience, unity or cohesiveness, over time. To do this, we employ 'relative distribution' methods, in the sense of Handcock and Morris (1998). The essence of the approach is to transform the data from two distribution—here, sets of MP agreement scores—into one distribution, such that we can compare them clearly and easily. The resulting 'relative distribution' consists of the percentile rank that every member in the first dataset would have in the second. Suppose, for example, that we broke up our data from Figure 1 into two parts: all the cohesion scores for the parliaments prior to 1874 as the first part, and the cohesion scores for the parliament of 1874 as the second part. Inspection suggests that, generally speaking, those in the earlier period would have relatively low percentile ranks in the second. We can be formal about this comparison and Handcock and Morris (1998) suggest two ways to do so.[6] First, we can decompose any distribution change between two sets of data into a *location* and *shape* change component—and we can compare the relative contribution of each

[4]The 1840s is a well documented time of relative tumult in the House of Commons, as Peelites left the ruling Tory party to join Whigs and Radicals in the Liberal party, just after the passage of the corn laws (see McLean, 2001, for discussion). One consequence was that for a period, nominally Conservative MPs could in principle be part of more than one voting bloc in the chamber, with obvious consequences for cohesion as we measure it.

[5]For comparison, the Blair government of 1997–2001 and the Coalition government after 2010 has a similar median (where agreement is measured as proportion of times MPs vote on the same side of the issue as their whip, rather than via the Chief-Whip-as-teller metric) to the latter part of the data set under study here.

[6]Readers are likely familiar with more basic techniques for comparing distributions such as the Kolmogorov–Smirnov test. While useful for determining whether, in fact, distributions differ, such approaches can say little about *how* distributions differ.

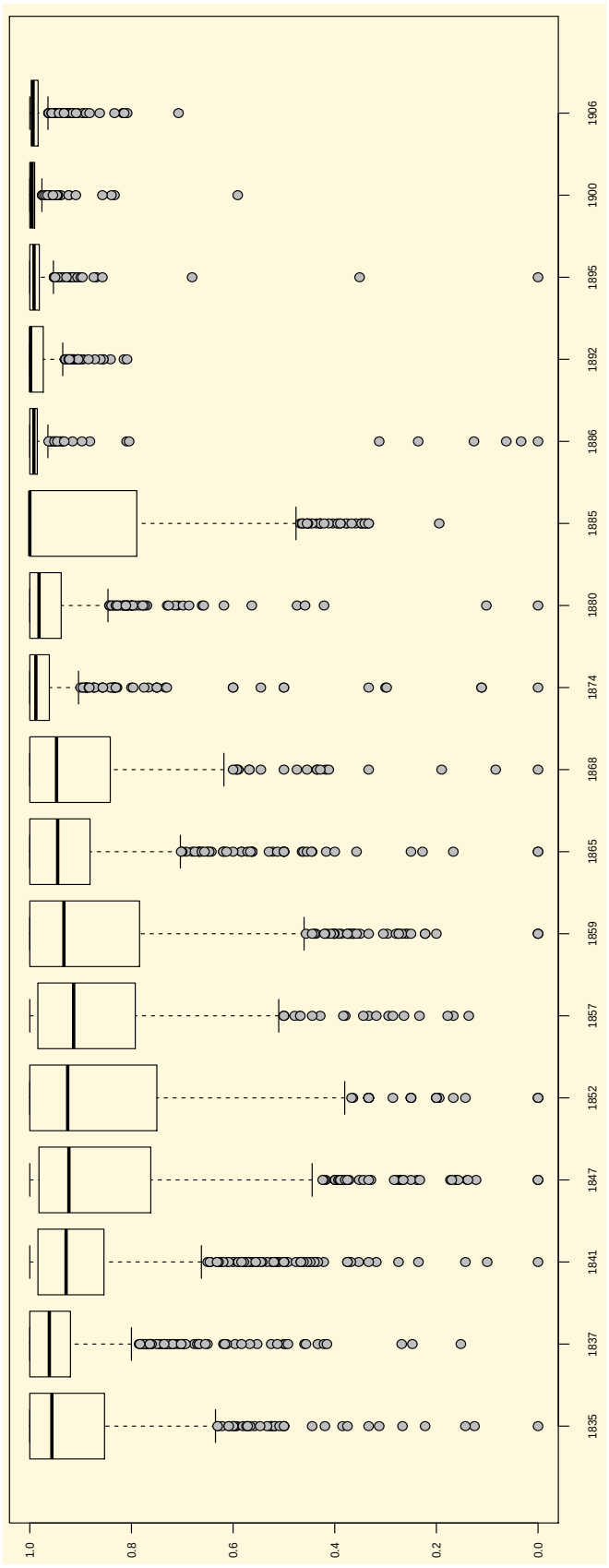


Figure 1: Aggregate discipline in the House of Commons over time: distributions of individual scores. Measure bounded between 0 and 1, with 1 being most disciplined.

part. The 'location' component refers to the notion of a 'shifted' distribution, and answers the question "how much of the change between this era of MPs and another era of MPs is due to an increased or decreased median cohesion score?". Any variation in the distributions that remains *after* taking into account this location change is due to changes in 'shape': that is, changes to the way that the distribution looks as a whole—including its spread, its range and so on. In Appendix C we give some more intuitive details on these ideas. The other summary measure Handcock and Morris (1998) suggest pertains to the 'polarization' of the distribution over time. In our context, their 'polarization index' refers to the extent to which there is more mass in the tails of the current parliament distribution than in the previous parliament. Thus, when the index is rising, we are in a situation where MPs are increasingly different from one another: spread over the support of the whip scores increasingly thinly. By contrast, when the index is falling, MPs are increasingly gathering around the central tendency, whatever that location measure might be. Here we will focus on the polarization in the left tail specifically, since this represents the least cohesive, most 'rebellious' MPs in our data. A nice feature of these summary measures is that they are robust to outliers—an important characteristic in the context of voting in parliament. Such approaches are implemented in the `R` statistical environment by Handcock and Aldrich (2002), and we use their software in what follows.

Note that the approach here is non-parametric, meaning that one does not need to commit to, for example, looking at mean and variance shifts under the very dubious assumption that whip agreement scores are normal. A drawback, as with other non-parametric approaches, is that relative distribution methods tend to require large amounts of data. A second caution is that relative distribution approaches are sensitive to 'heaping' (Handcock and Morris, 1998, 89), which refers to the tendency of social scientific data to 'pile up' in certain places on the continuous spectrum. Here, as time goes on, more and more MPs appear at, or close to, a whip agreement score of 1 (that is, full agreement). As a result, the accuracy of the entropy calculations—a measure of the distance between two distributions that can be decomposed into location and shape components—can suf-

fer.[7] To ameliorate this problem, we compare a shifting *set* of parliaments in cohesion terms, thus maximizing our sample size and helping to smooth across transitions.

In Table 1 we report the estimated entropy difference between distributions, and then the location and shape components that contribute to it. Technically, the location and shape components should sum to the entropy for that transition, though here this is not quite the case for all entries. Likely, this discrepancy is due to the heaping and 'small' data problems we mention above. In each case here, we are comparing two sets of parliaments: all those before a certain date, to all those after that date. For example, the entry for 1847 treats the parliaments of 1835, 1837, 1841 and 1847 as one distribution, and then the parliaments of $1852, 1857, 1859, 1865, \ldots, 1900, 1906$ as the second distribution. Similarly, the entry for 1900 compares the distribution of all parliament up to and including that one with the parliament of 1906.

In Figure 2, we provide a barplot of the same information as in Table 1 except that we impose the rationality constraint that all negative values (of location contribution) are in fact zero, and then treat the sum of the location and shape contributions as the total entropy. The [red] broken line is the proportion of the entropy that is due to the shape distribution alone. The black horizontal line marks the point at which half of any change is due to the shape. When the broken line is above this point, as it is after the 1850s, the majority of any alteration to MP discipline arises from the shape rather than median shifts. As can be seen from the plot, at the beginning of the century, location and shape changes have approximately equal responsibility for any changes to the cohesion distribution. After the 1850s, the shape contribution rapidly increases; by the end of the century, only the shape is changing.

[7]Note that, as used here, the 'entropy' is simply the Kullback-Leibler divergence, written D, between two distributions. We can denote the reference distribution (which is the 'original' set of scores to which the newer ones will be compared) as F_0, the comparison distribution (the newer set of scores) as F, and the location adjusted distribution (simply the reference distribution which has been 'moved up' to have same median as the comparison distribution) as F_A. Then, in principle, the entropies we need for the location and shape components can be obtained via $D(F; F_0), D(F; F_A)$ and $D(F_A; F_0)$. In fact, this cannot be done directly, but there are related identities that allow one to proceed in practice.

parliament of	Total entropy	location contribution	shape contribution
1835	1.26	1.13	0.82
1837	1.15	1.07	0.81
1841	1.13	1.05	0.78
1847	1.16	1.06	0.86
1852	1.18	1.01	1.03
1857	1.27	1.03	1.16
1859	1.34	1.02	1.03
1865	1.45	1.09	1.38
1868	1.58	1.09	1.46
1874	1.55	1.05	1.47
1880	1.64	1.05	1.56
1885	1.78	0.93	1.71
1886	1.58	0.75	1.66
1892	1.55	0.49	1.73
1895	1.45	0.28	1.75
1900	1.09	-0.15	1.51

Table 1: Relative contribution of median and shape to changing distribution of whip agreement scores over time. Note that the location and shape contributions do not always sum to the (total) entropy. This arises from the data requirements of relative distribution methods not being fully met: note though, that the relative contribution of the components can be still be compared in *absolute* terms.

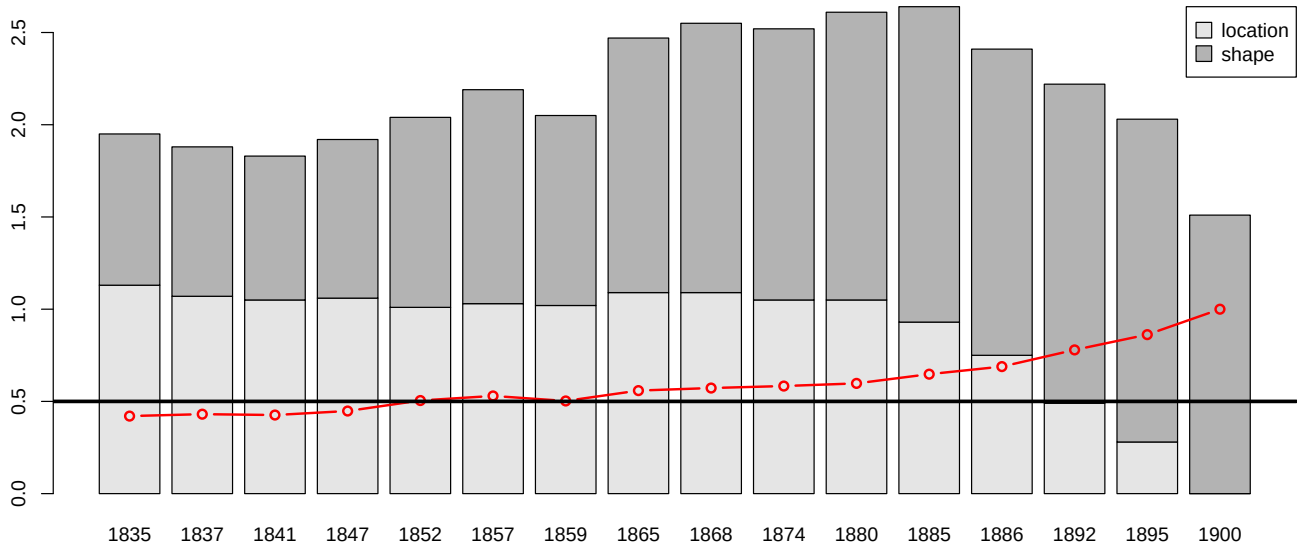


Figure 2: Barplot showing relative contribution of location and shape components in relative distributions for parliaments in the data. Red broken line demarcates proportion of entropy due to shape contribution: note that it rises over time. The solid black horizontal line shows the '50%' mark for shape contribution: when the [red] broken line crosses this horizontal line, more than half of the distributional change is due to shape changes. Notice that the bar for a given year reflects the comparison of all parliaments to that date, with all that come after.

As noted above, we can also study the 'polarization' of a distribution: essentially a measure of what proportion of the distribution's mass is in the center relative to the tails. Handcock and Morris (1998) suggest several such measures, wherein a positive value implies more mass in the tails, and a negative value implies more concentration around the median. Here, we focus on the polarization of the left tail. In terms of data arrangement, we now compare each 'current' parliament to all previous ones.[8] Thus, 1847 is compared to the parliaments of 1835, 1837 and 1841 considered as one set of observations. Our idea here is to describe what happened to the most rebellious parts of the parties in the new parliament. Figure 3 presents the results. There, points represent the lower tail index. When black, the points imply that the index is statistically significant (meaning that any observed polarization of the distribution—i.e. deviation in shape from the previous parliament—is sufficiently unlikely to have occurred by chance). As the solid lowess line combined with the points makes clear, after an initial period of flatness or possible increase, rebellion is decreasing over time.

[8]The findings do not change substantively if we alter how many parliaments are being compared at once, but the 'performance' of the nonparametric techniques is better—in terms of the issues mentioned above concerning accuracy of the entropy calculations if we proceed this way.

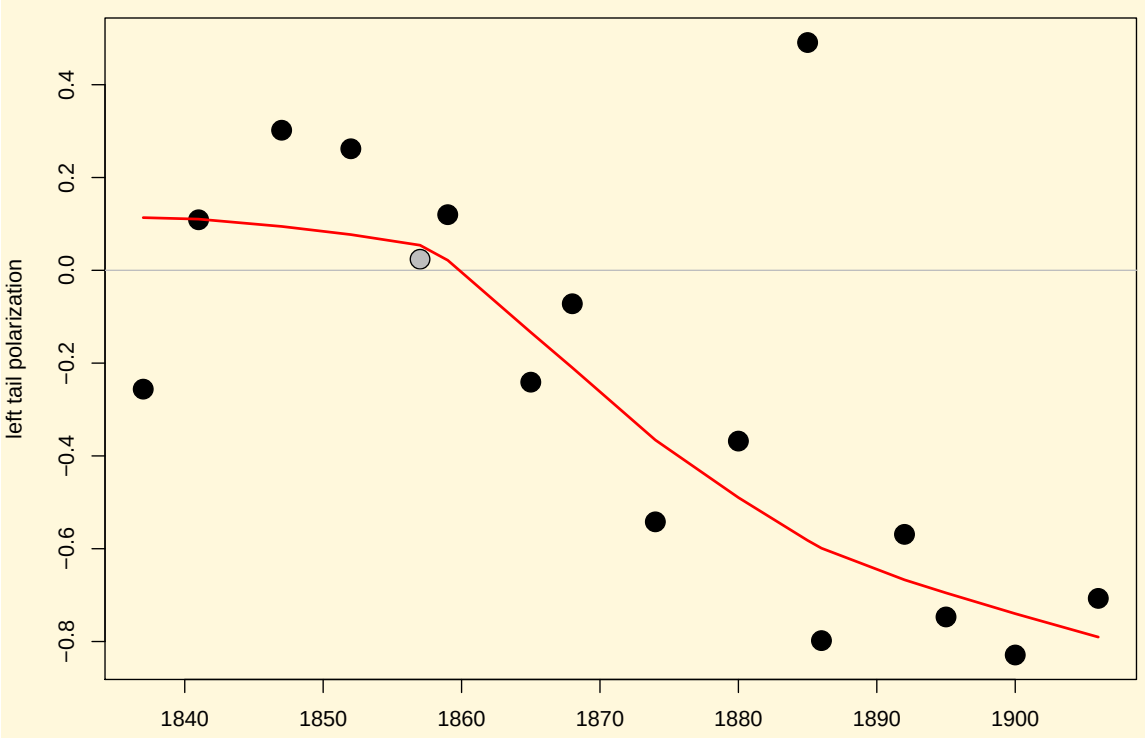


Figure 3: Share of the discipline distribution change in the left tail, over time. Comparison is current parliament to all previous ones at that point. Points represent index; black symbols imply effects with $p < 0.01$, gray symbols imply otherwise. Note the solid lowess line and points imply first increasing (or at least non-decreasing), then decreasing over time and implying that the left tail is 'emptying out'.

In practice, this means that rebeliousness becomes more common early on, but the rate of change no longer increasing by the 1850s, and is actively decreasing (i.e. there is less mass in the rebel tail) by the 1860s.

Figure 3 and Table 1 tell a consistent story: as the 19th century progressed, the distribution of the discipline in the House of Commons became increasingly concentrated around its average and away from the left tail. Put otherwise: for whatever reason, over the period we study the norm was almost always to vote as the whips did; what changed was that a smaller and smaller proportion of MPs did not obey this norm. In the next section, we delve into possible mechanisms for this change.[9]

[9]Note that as a robustness check, we disaggregated our cohesion data to a year-by-year basis, and conducted standard structural break tests for various quantiles of the distributions in question. Our results are discussed in

5 Assessing the Role of 'Replacement'

If the decline of the rebellious left tail documented in the previous section is due to the replacement of undisciplined MPs by other MPs who were more inclined to vote with their party, then analysis of MPs' cohesion scores should reveal several patterns. In this section we assess the evidence for these patterns using panel data. We first compare the cohesion scores of new entrants to Parliament and MPs who were on their way out of the chamber ('joiners' and 'leavers') to that of other MPs; we then use member fixed effects to decompose the long-term increase in cohesion into the part that could plausibly be due to replacement and the part that must be due to other factors.

5.1 Comparing "joiners" and "leavers" to other MPs

If the 'replacement' story explains the decreased number of 'rebels' in the left tail, we ought to see that (1) 'joiners' (that is, MPs who have recently entered the chamber) vote with the party whips more diligently than other MPs of their party, and (2) 'leavers' (that is, MPs leaving the chamber) vote with the party whips less diligently than other MPs of the party.[10] We now carry out some simple tests of that prediction. In what follows, recall that each MP contributes a separate observation to the data; thus the analysis below deals with multiple observations for each MP, who is (possibly) serving in multiple parliaments. Before reporting our results, we acknowledge that our models below are 'stripped down' to core variables, primarily pertaining to fixed effects for members and parliaments. Of course, we acknowledge that underlying such fixed effects, and in addition to them, other more subtle forces are likely at work in determining how cohesive particular MPs are—including their district characteristics and personal connections and histories in the legislature. Other work has made in-roads into studying such associations (see Eggers and Spirling, Forthcoming*a*), and this is not our focus here.

Appendix D and are exactly consistent with our account here insofar as we see the median and 95th percentile close to (or converging) on one throughout the entire period, while the 5th percentile rapidly converges towards one from the 1860s onwards.

[10]We recognize that differences among 'joiners', 'leavers', and other MPs could be explained in other ways. For example, some 'leavers' may have been encouraged to leave because of their disloyal voting; some 'joiners' may be vote in a disciplined way because they hope to impress party leaders. Still, we find this a useful starting point for assessing the role of replacement.

Table 2 reports regression results in which each MP's per-parliament cohesion score is regressed on indicators for whether an MP is a 'joiner' or a 'leaver' in a given parliament as well as an indicator for the parliament, an indicator for the MP's party, and the interaction of the two.[11] In column (1) the only RHS variable aside from parliament and party dummies is an indicator for whether the MP is in his first parliament; the coefficient indicates that new MPs were if anything *less* cohesive overall than other MPs (controlling for the parliament, the MP's party, and the interaction between the two). When we interact our "Joiner" indicator with a dummy for pre-1852 (column 2), we see that joiners were no more or less likely to vote cohesively during the post-1852 period in which average cohesion increased; if joiners voted less cohesively on average, it was only during the early period when average cohesion was stable or declining. Columns (3) and (4) indicate that MPs in the last parliament of their career ("Leavers") voted somewhat less cohesively than other MPs; in column (4), the "main effect" of being a leaver is negative and significant at the .05 level. When we include both indicators in the regression (Columns 5 and 6), we cannot reject the null hypothesis that joiners and leavers were indistinguishable from other MPs in terms of voting cohesion. Not only are the effects not statistically significant, but the magnitudes of the point estimates are very small, suggesting that the cohesion scores of joiners and leavers did not differ from other MPs by more than about half a percentage point on average.

In analysis reported in Appendix E, we carry out the same regressions separately by party to allow for the possibility that replacement may have explained rising cohesion in one party but not the other. We find some evidence that Conservative joiners were marginally more cohesive than other Conservative MPs in the later period (with a coefficient of .01), but this is counteracted by evidence that Liberal joiners were *less* cohesive. In neither party are leavers less cohesive than those who continued on. Taken together, these regressions do not tend to support a replacement story: only for Conservatives do we find that joiners voted more cohesively, but the opposite is true for Liberals and these effects (and all other point estimates) are fairly small in magnitude.

[11] Thus the comparison is whether a given MP who is e.g. a 'joiner' votes more cohesively than other MPs of the same party in a given parliament.

Table 2: Are new MPs (joiners) more disciplined? Are exiting MPs (leavers) less disciplined?

	(1)	(2)	(3)	(4)	(5)	(6)
Joiner	-0.0051†	-0.002			-0.0043	-0.0011
	[0.0027]	[0.0028]			[0.0033]	[0.0035]
Pre-1852		-0.0356***		-0.0382***		-0.0341***
		[0.0067]		[0.0071]		[0.0075]
Joiner × pre-1852		-0.0124†				-0.0129
		[0.007]				[0.0089]
Leaver			-0.0054†	-0.0064*	-0.0048	-0.0056
			[0.0029]	[0.0031]	[0.0036]	[0.004]
Leaver × pre-1852				0.0035		0.0036
				[0.0072]		[0.0088]
Joiner × leaver					-0.0019	-0.0022
					[0.0054]	[0.0058]
Joiner × leaver × pre-1852						0.0015
						[0.0146]
Adj. R^2	0.222	0.222	0.217	0.217	0.217	0.217
N	10350	10350	10086	10086	10086	10086
Parliament dummies, party dummies, and interactions	✓	✓	✓	✓	✓	✓

NOTE: Dependent variable is a member's cohesion score in a parliament. Heteroskedasticity-robust standard errors (in brackets) are clustered by member and parliament. Guide to significance codes: *** indicates $p < .001$; ** indicates $.001 < p < .01$; * indicates $.01 < p < .05$; and † indicates $.05 < p < .1$.

5.2 Assessing the role of replacement through member fixed effects

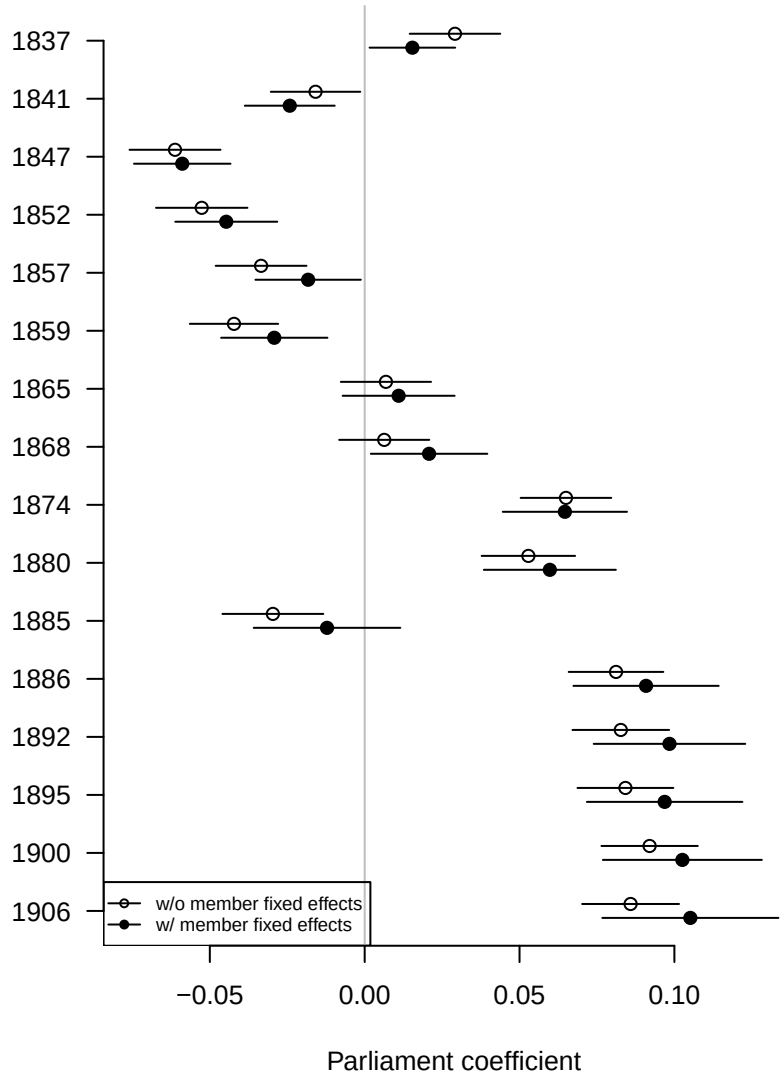
We now adopt a slightly different approach to assessing the role of replacement in explaining the increase in party cohesion in this period. Above we looked looked for more or less cohesive voting among MPs who enter or exit the Commons. Here our approach is to use member fixed effects to soak up MP-specific variation in cohesive voting; if replacement is an important part of the story, these member fixed effects should explain much of the long-term increase in average cohesion. In this section we assess whether this is the case.

Our first approach is to see if the inclusion of member fixed effects reduces the magnitude of estimated time effects. We ran two regressions; in the first, members' cohesion scores (parliament-by-parliament) are regressed only on parliament dummies to get a measure of average cohesion in each parliament; in the second we carry out the same regression but we add member fixed effects. If replacement helps explain the increase in cohesion over time, the parliament dummies should be smaller in magnitude in the second regression, indicating that some of the over-time variation in average cohesion is explained by the replacement of undisciplined MPs by more disciplined ones. Figure 4, which plots the parliament dummies for the two regressions, indicates that this is not the case. If anything, the magnitude of the parliament effects is larger when member fixed effects are included. Consistent with the above analysis, this indicates that the sustained increase in party cohesion in the second half of the 19th century is not due to replacement effects. More formally, we can show that the average magnitude of the parliament dummies is actually about 4% larger when we include member fixed effects than when we do not, and we are unable to reject the null hypothesis that the magnitude of the parliament dummies is unaffected by the inclusion of member fixed effects.[12]

As an additional way of clarifying the role of replacement in explaining the change in disci-

[12] For each of m repetitions and for each of the two regressions, we draw a set of parliament effects from a multivariate normal distribution that is centered at the regression estimates with the corresponding variance-covariance matrix. (This is a quasi-Bayesian procedure similar to the one used in King, Tomz and Wittenberg (2000).) Across simulations, the average size of the parliament effects is 4% larger on average in the regressions with member fixed effects; in only about 1/3 of the simulations are the parliament effects smaller in the regressions with fixed effects.

Figure 4: Do member fixed effects soak up the over-time variation in cohesion?



NOTE: Point estimates and .95 confidence intervals plotted for the "parliament effect" corresponding to each year shown. The white circles show the estimates for a regression in which cohesion scores are simply plotted on parliament dummies. The black circles show the estimates for the same regression with member fixed effects added. The fact that member fixed effects do not soak up much of the over time variation is an indication that replacement does not account for the long-term increase in cohesion.

pline over this period, we carry out regressions to see how much of the overall change in cohesion can be linked to changes *within an MP's career*. The reasoning is similar to that in the comparison of parliament effects above. If replacement is an important part of the explanation for the long-term increase in cohesion in the House of Commons, then we might expect to find that the cohesion score of the average MP does not change much within his career; if replacement is the key, then the important thing is the replacement of MPs who were intrinsically rebellious by MPs who were inclined to vote with their party. One implication of the replacement story, then, is that any increase in cohesion within an MP's career should occur at a lower rate than the increase in overall cohesion. If this is not the case – that is, if the average individual MPs became more cohesive at the same annual rate as the legislature overall – then this would seem to leave little room for replacement as an explanation of the long-term increase in cohesion.

To carry out this test, we regress members' cohesion scores (one for each parliament) on their seniority at the beginning of the parliament. By including member fixed effects, we measure the "effect" of seniority within an MP's career; the MP's baseline level of cohesiveness is soaked up by the fixed effect. These regression coefficients are reported in columns (1) and (2) of Table 3. For comparison, in columns (3) and (4) we report parliament level regressions where, having calculated average cohesion for MPs in each parliament, we regress cohesion on "seniority" (meaning years since 1835) in the same way as in the member-level regression.

The remarkable result of this analysis is that during the second half of the 19th century (the period during which overall cohesion increased) the change in cohesion within MPs' careers is essentially the same as the change in average overall cohesion during the same period: the coefficients on "Seniority" in Table 3 in columns (1) and (2) are basically the same as the corresponding coefficients in columns (3) and (4). One implication of this is that *all* of the increase in average cohesion between 1852 and 1906 can be explained by changes that happen within individual MPs' careers. Between 1852 and 1906 the mean cohesion score in parliament increased from 0.87 to 0.98, a gain of 0.11. But that is almost exactly equal to the implied gain from our model for an MP serving

Table 3: How does the change in cohesion within an MP's career compare to the change in aggregate cohesion over time?

	Member level		Parliament level	
	(1)	(2)	(3)	(4)
Seniority	0.0016***	0.0024***	0.0017***	0.0027***
	[0.0002]	[0.0002]	[0.0004]	[0.0005]
Pre-1852		0.0469***		0.1069**
		[0.007]		[0.0322]
Seniority × pre-1852		-0.0037***		-0.0091**
		[0.0005]		[0.0033]
Adj. R^2	0.007	0.013	0.495	0.694
N	10350	10350	17	17
Member fixed effects	✓	✓		

NOTE: Dependent variable in columns (1) and (2) is a member's cohesion score in a parliament; "Seniority" in those columns refers to the time between the MP's entry into Parliament and the beginning of a given parliament. Dependent variable in columns (3) and (4) is the average cohesion score of MPs (Liberals and Conservatives only) in a given parliament; "Seniority" in those columns refers to the time elapsed between 1835 and the beginning of the parliament. Standard errors (heteroskedasticity-robust and clustered by member and parliament in columns 1 and 2) in brackets. Guide to significance codes: *** indicates $p < .001$; ** indicates $.001 < p < .01$; * indicates $.01 < p < .05$; and † indicates $.05 < p < .1$.

(hypothetically) the entire period under study: that is, $\hat{\beta}_{post-52} \times (1906 - 1852) = .13$, with a .95 confidence interval of $[.10, .15]$.[13]

To be clear, our argument is *not* that cohesion increased in the 19th century because average seniority increased. In fact, average seniority was if anything declining over the period we examine, with steady increases only in the period before the 1850s when cohesion was falling. Rather, our claim is that, because individual MPs got more disciplined at about the same rate as the body as a whole, replacement effects must have been minimal. To see the point more clearly, consider an alternative situation in which *all* of the increase in cohesion over time is due to replacement. In that case, if we ran a regression like the one reported in columns (1) and (2) of Table 3 we would find a coefficient of zero on "Seniority", with all of the variation in cohesion being soaked

[13]Note that based on the interaction term in Table 3 ("Seniority × pre-1852") the drop in cohesion up to 1852 may have been due in part to replacement, as the drop in cohesiveness within MPs' careers appears to have been slower than that of cohesiveness overall.

up by the member fixed effects. What we find instead is that the member fixed effects evidently explain relatively little of the change in cohesion over time, and thus that cohesion in the House of Commons appears to have increased because MPs in the House of Commons voted more cohesively over the course of their careers (for whatever reason), not because undisciplined MPs were replaced by those who voted more cohesively.[14]

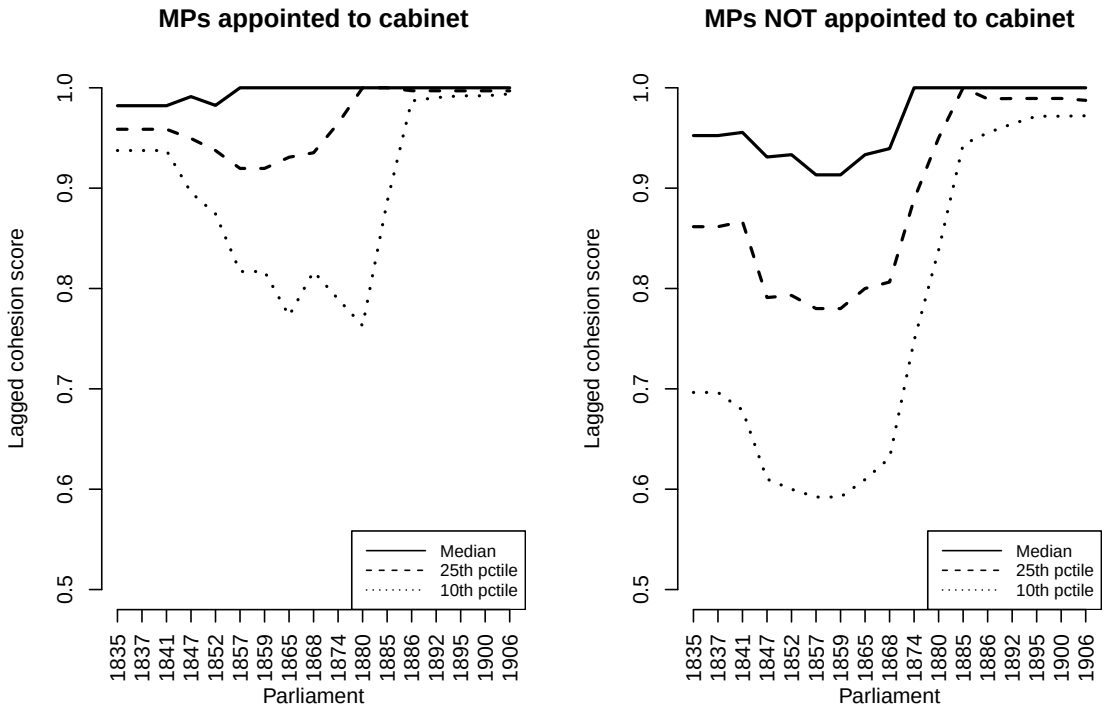
6 Assessing an Inducements Story: Promotion to the Cabinet

We now turn to assessing one channel through which MPs may have been induced to vote with their party whips: the reward of office. Cox (1987) addresses this question by comparing the prior voting records of MPs who were awarded ministerial posts to those of MPs who were not awarded ministerial posts. In analysis of Liberal ministers 1880-1885 and Conservative ministers 1874-1880, he obtains mixed results, concluding that if ministerial ambition explains increasing party cohesion it likely does so only for the 1890s and early 1900s. In this section we revisit the question with far more extensive data—every parliament, as opposed to one parliament per party.

As an initial cut, we compare the distribution of cohesion scores over time among those appointed to cabinet positions and those not appointed to cabinet positions. Figure 5 depicts, for each parliament, three quantiles (.5, .25, and .1) of cohesion scores for those subsequently appointed to the cabinet (left panel) and those not appointed to the cabinet (right panel). In particular, we look at all cases where an MP has not previously served in a cabinet office before that parliament and the MP's party formed a government during that parliament; we then calculate the median (or 25th percentile or 10th percentile) cohesion score for these cases in the *previous* parliament (to reflect the idea that MPs may be promoted in a given parliament for loyal voting in the previous parliament). To smooth out the presentation, we compute the quantiles depicted for a given year based on data from all parliaments elected within 15 years of that year.

[14]It should be noted that the foregoing analysis adopts a fairly restrictive view of how replacement effects might work. A broader view would recognize that an MP might vote more cohesively over the course of his career in part because of changes in who *else* was sitting the legislature.

Figure 5: Are more cohesive voters more likely to be promoted to the cabinet?



NOTE: Figure indicates quantiles of cohesion scores for MPs who were appointed to the cabinet in a given parliament (left panel) and those who were not (right panel). To smooth the data each estimate is based on all parliaments within 15 years of the parliament indicated. MPs are included in the analysis only for parliaments in which they have not previously occupied a cabinet office and their party formed a government at some point during the parliament.

Figure 5 clearly shows that MPs who were not appointed to the cabinet voted less cohesively. Up until the 1870s at least, the median non-appointed MP consistently had a lower cohesion score than the median appointed MP (solid lines), and the same is even more clearly true for the other quantiles. (By the end of the century cohesion is high enough that it is difficult to distinguish between the two groups of MPs.)

The initial comparison in Figure 5 of MPs who were and were not appointed to the cabinet suggests that party leaders may have rewarded loyal voting with subsequent appointments to cabinet office. Of course, these comparisons of distributions do not control for other differences between MPs who

are and are not appointed.[15] It could be, for example, that MPs who vote more cohesively are also more senior or have served in other official capacities, and that controlling for these differences the appointees and the non-appointees may not differ in voting cohesion. We address some of these concerns in Table 4, which reports regressions in which a binary indicator for whether an MP was appointed to a cabinet position in a parliament is regressed on the MP's lagged cohesion scores, other characteristics of the MP, and (in column 4) parliament dummies.[16] (Again, we restrict attention to MPs who had not previously held cabinet office and who belonged to a party that formed a government in a given parliament.)

The results of Table 4 are consistent with party leaders offering cabinet positions somewhat more readily to MPs who had previously voted more loyally with party whips. The lagged cohesion score is a statistically significant predictor of the appointment decision in all specifications at the .1 level or better. (Note also the low adjusted R^2 across models.) The best predictor of whether an MP will be appointed to cabinet office is whether the MP was previously appointed to non-cabinet office;[17] the coefficient on age indicates that, conditional on an MP's seniority, younger MPs are somewhat more likely to be promoted to the cabinet.

It should be noted that the magnitude of the "effect" of cohesion on subsequent promotion to the cabinet is modest: a rebellious MP with a cohesion score of .5 who subsequently votes with perfect loyalty would increase his predicted probability of promotion by just 1 or 2 percentage points depending on the model. Put differently, those who were and were not promoted differed in previous cohesion by a substantively small amount. Still, the lure of a ministerial post was large enough that even a small preference for MPs with relatively loyal voting records may have been

[15] They also do not control for the ideology or policy preferences of MPs: it could be that more cohesive voters were more likely to be promoted because prime ministers sought cabinet members who were more ideologically aligned with themselves. We currently have no measure of ideology independent of voting, and as such it is impossible to distinguish ideological alignment from cohesive voting.

[16] These are OLS regressions. The results are almost the same in terms of substantive magnitude and statistical significance if we instead perform logistic regressions and transform the results to marginal effects calculated at the means of RHS variables.

[17] In separate analysis (reported in Appendix F), we fail to find a similar relationship between disciplined voting and subsequent promotion to a non-cabinet office.

Table 4: Are more cohesive voters more likely to be promoted to the cabinet?

	(1)	(2)	(3)	(4)
Lagged cohesion score	0.0436***	0.0409***	0.0241*	0.0212†
	[0.0117]	[0.0105]	[0.0103]	[0.0111]
Seniority	0.0011***	0.0011***	0.0002	0.0001
	[0.0002]	[0.0002]	[0.0002]	[0.0002]
Age	-0.001***	-0.001***	-0.0005**	-0.0005**
	[0.0002]	[0.0002]	[0.0002]	[0.0002]
Post-1868		0.0573	0.0648	0.0586
		[0.0494]	[0.0489]	[0.0577]
Lagged cohesion × post-1868		-0.0482	-0.0577	-0.0519
		[0.0515]	[0.0509]	[0.0575]
Previously held non-cabinet office			0.0991***	0.1003***
			[0.0133]	[0.0134]
Adj. R^2	0.007	0.009	0.068	0.069
N	3695	3695	3695	3695
Parliament dummies included				✓

NOTE: Dependent variable is 1 if an MP occupied a cabinet office in a given parliament. MPs are included only if they have not previously occupied a cabinet office and only if their party formed a government at some point during the parliament. Heteroskedasticity-robust standard errors (in brackets) are clustered by member and parliament. Guide to significance codes: *** indicates $p < .001$; ** indicates $.001 < p < .01$; * indicates $.01 < p < .05$; and † indicates $.05 < p < .1$.

enough to apply decisive pressure on MPs to fall in line behind the party whips. A fuller record of data on voting and ministerial promotion thus indicates that ministerial posts were indeed awarded to more loyal MPs and, contrary to the specifics (though in line with the thrust) of Cox (1987)'s conjecture, the difference is particularly evident before the 1870s when MPs were more rebellious on average.

However modest the effect size, given that the evidence for replacement is dubious, the fact that cohesion increased over time suggests that ministerial promotion became more valuable—or more responsive to behavior—during the period under study. Explaining exactly why this was is beyond the scope of our current efforts. Nonetheless, several reasons may be posited, all interrelated: as the electorate became increasingly 'party orientated' in the sense of Cox (1987), it was the party leaders (rather than local MPs) who were *de facto* at the forefront of voters' minds when they went to the ballot box. One consequence presumably was that Prime Ministers became more powerful as previously local power bases were eroded: able to pick members for their ministerial 'team' subject to their performance in divisions, without hindrance from notable backbenchers or their allies. Simultaneously, party voting also encouraged a reorganization in Commons procedure, with more power residing in the cabinet and the opposition front bench (Eggers and Spirling, Forthcoming*b*). It is to be expected that being a minister (rather than a relatively impotent backbencher) was increasingly a prize worth seeking—whatever behavior it required.

6.1 Summary

The foregoing sections showed that

1. average cohesiveness—measured as the proportion of times an MP voted with his whip—was always relatively high, with a baseline of around 0.9 in 1830s. It dipped in the 1840s, and increased to present day levels thereafter;

2. almost all the aggregate increase in cohesion in the second half of the 19th Century is a product of the left tail of discipline scores receding into the main mass of the distribution: that is, rebels became fewer and further between over time;

3. contrary to a 'replacement' story, incoming MPs in this period were not more disciplined than other MPs nor were exiting ones less so;

4. contrary to a 'replacement' story and consistent with a number of 'inducement' explanations, the average annual increase in cohesion *within an MP's career* after 1850 is about the same as the average annual increase in cohesion *of Parliament as a whole* in the same period.

5. consistent with one 'inducements' story, MPs who were promoted to the cabinet in a given parliament tended to have voted more cohesively in the prior parliament.

7 Discussion

Legislative party cohesion is important for the functioning of Westminster democracies. Without it, the link between government policy-making and voter preferences is broken. Understanding how this institutional arrangement came about has therefore been of critical importance to scholars of history and political science (e.g Cox, 1987; Jenkins, 1996; Adelman, 1997). Here, we undertook a massive data collection task to shed light on the central question of *why* members became more disciplined. We gathered over 20,000 roll calls from the period 1836–1910, and we matched them to individual MP records so that *intra*-career changes in cohesion could be calculated for the first time. We compared accounts focussing on 'inducement' effects—deriving as we think they do from a possibly large number of different sources, and encouraging (already serving) MPs to behave differently in roll calls—to more sociological 'replacement' effects, by which new personnel with new characteristics might drive the patterns we saw. In practice, we were able to reject essentially all claims that the latter school of thought might offer; by implication, endogenous sources of party cohesion offer more promising explanations. We showed that, at a descriptive level, what drove cohesion was the 'folding in' of the rebellious left tail of the cohesion distribution, and that this general pattern was consistent over time and was not a product of cohort entry and exit dynamics.

Our findings have several broader implications. To the extent that an inducement account is correct and that parties' changing strategies played a major role here, there is evidence of 'institutional

learning', in the sense that actors both learn from and effect the rules and procedures around them. Our focus here on the importance of formal and informal rules of behavior, albeit within a broader context of a changing electorate and other constraints, places this paper within the 'rational choice institutionalism' literature (see Shepsle, 2006, for an overview). Second, we find that the House of Commons has been remarkably robust to new entrants in terms of procedure; this may mean that for certain 'new' types of individuals serving as MPs (e.g. women, (Norris and Lovenduski, 1995; Lovenduski, 2005)) to have the hoped-for impact on the norms of behavior there, other changes to structural incentives may be required (see also Cowley, 2002, on this point). Third, whatever the *electoral* consequences of the Second Reform Act (1867) (see Smith, 1967; Feuchtwanger, 1968; Jenkins, 1996; Berlinski and Dewan, 2011), the *legislative* effects of this surge in the franchise appear to have been minimal in terms of 'average' behavior as *per* Figure 1; parties were already on the way to modern practice by the 1850s. This is in line with suggestions from others Cox (1987), albeit we have more comprehensive data to make the point.

We have documented one channel through which backbench troops may have been brought into line (the lure of ministerial promotion), and the evidence we found was modest but tangible. Of course, there are numerous other plausible theories, ranging from socialization (Rush and Giddings, 2011), to partisanship (Jenkins, 1996), to increasing rhetorical skill by leaders (Moser and Reeves, 2013), to agenda control by the executive (Dewan and Spirling, 2011) or some combination of the above (Kam, 2009*b*). It is difficult to investigate such accounts with the kind of individual-level analysis we have done here because these mechanisms tend to operate on entire cohorts of MPs; this makes it difficult to isolate the effects of a given mechanism from those of other over-time changes that took place simultaneously. Such aggregate-level forces are undoubtedly important for explaining the patterns we have documented here, however, and future scholars may draw on our data to make progress. One option is to use parliamentary speech to get a sense of how MPs talked about votes over time, and possibly to see how this language compared with actual voting behavior for individual MPs. Here, extant qualitative historical work on leaders and the culture of the Commons at the time may provide a useful resource (e.g. Cooke and Vincent, 1974; Blake,

1985; Coleman, 1988; Hawkins, 2007). We leave such efforts for future work.

A The Division Data

We received the data as a set of page images and corresponding `xml` files (one for each division); as in the original printed source, each file included basic information about the division along with the names of the MPs voting 'Aye' and 'No' and the names of tellers for each side. There are some 20,262 of these roll calls. Although the `xml` provides useful structure to the raw data, the original source did not identify the party of each MP, nor are MPs identified in a standardized way in either the original source or the `xml` markup. Thus without further processing this remarkable data source is of limited use in systematically analyzing voting cohesion or anything else. We therefore endeavored to link the voting data to our broader database, which includes every MP serving in this period along with his electoral records, speeches in parliament, and key background information including party, dates of birth and death, dates of service, constituency names, offices held, etc. Merging the named MPs in the division lists to their records in our database required a combination of custom automated processing and manual attention (ultimately requiring hundreds of research assistant hours); the merging is not straightforward because MPs are not identified in standard ways in the division lists (whether due to irregular conventions, misspellings, or OCR errors). The covariates themselves come from various standard sources, such as Craig (1989), Craig (1974), Walker (1978), Cook and Keith (1975) and Butler and Butler (1994). In this way, each vote in each division list becomes part of a record on a given individual. By the end of the cleaning process, both automatic and manual, we had identified the MP being around 99% of the votes cast in each parliament; that is, we were uncertain who the MP cast was in around one percent of the 4,791,282 vote decisions in our data.[18]

B Measurement of Discipline

To clarify how our cohesion measure works, consider the following example. Let $\mathbf{w}$ be the composite whip voting vector expressed as the side he 'tells' for. That is, the vector of decisions on all the roll calls in a given parliament. The whips may 'tell' in one of three ways: for the 'aye' side (coded as 1), for the 'no' side (coded as 0), or not at all (coded as NA). In what follows, suppose there are a total of five roll calls and that the whips' voting profile is

$$\mathbf{w} = [\underbrace{1, 0, \text{NA}}_{\text{whip}_1}, \underbrace{1, 1}_{\text{whip}_2}].$$

Recall that we sometimes have more than one MP serving as a Chief Whip, and we infer the whipping vector from whomever holds that position and acts as teller at some point during the parliament. Here, the vector is implicitly divided into two parts: one MP serving as Chief Whip for the first three votes, another for the last two. Suppose a Member of Parliament i's voting vector is

$$\mathbf{m}_i = [1, \text{NA}, 0, 1, 0].$$

Since MP i only votes in four divisions, the denominator for any agreement scores is four. Here, i votes in the same way as the whip 'tells' on two of the four roll calls on which he attends, and thus

[18] In principle, essentially all of the votes can be linked to an individual; we viewed 1% missingness as acceptable for our purposes.

i's agreement score is $\frac{2}{4}$. Notice that missingness in the MP's vote vector does not count against the MP's disciplinary record. Similarly, when the whips' votes are missing, that roll call is removed from consideration in terms of agreement. The distribution of all such MP's agreement scores is denoted as the "discipline distribution" or "cohesion distribution". Note that this aggregate distribution must be bounded between zero and one. Importantly, while the whip vector is party-specific, the aggregate discipline distribution merges the parties. That is, the aggregate distribution has as its ith component the agreement score between the i MP and *his* party-specific whip, though that party can be different from MP to MP. This means that a Tory MP with the same discipline score as a Liberal MP in a given parliament may have voted in exactly the same way in practice as the Liberal ('ayes' and 'noes' in the same places), or in the exactly converse way, or something in between. What matters is how often he follows his whip, and whips in different parties may in some circumstances agree (in voting terms) on specific issues. Finally, note that we do not require both party whips to vote in a given division for it to be included in our analysis—so long as one enters a preference, that division enters the data for his party.

C Relative Distributions: Intuition

Relative distribution methods allow changes in the distribution of cohesion to be due to location shifts, shape changes, or some combination of the two. In Figure 6 we make these ideas clearer in graphic form. In the top panel (a), the distributions of cohesion differ only in their location: that is, the light colored density is exactly the same as the dark colored one, but shifted 'up' to the right. The proportion of members in the left or right tails is identical in both: there has been no change in shape. In the middle panel, labelled (b), the locations (the medians) of the densities are the same, but the shapes are different: literally, the proportion of members in the tails in the light colored density is obviously larger than for the dark colored one. Finally, in the bottom panel, labeled (c), we report two cohesion densities that differ in terms of both location *and* shape: the dark colored one has a lower spread, and its median is clearly lower also. The idea of the methods, intuitively, is that if we see a change as pictured in panel (c) we may break it down into two sets of movements: a location shift (as in the top panel), and a shape change (as the middle panel). The entropy measurements tell us how much a move like that pictured in panel (a) or panel (b) captures the total movement as seen in panel (c).

D Verifying Changes in Disaggregated Data

Here we verify that the changes seen in the boxplots hold in less aggregated data. In Figure 7 we consider division discipline on a *year-by-year* basis, using the same whipping and coding rules as above. There, we show three indicative quantiles—the 5th, 50th and 95th percentile—over time. As can be seen very obviously from the lowess lines at the top of the graph, the most loyal (the 95th percentile) and the median are both close to one throughout our period. The least loyal MPs, the 5th percentile, start out around 0.6, fall a little in the 1840s, and then surge in cohesion from the 1860s onwards: by the close of the century they are virtually indistinguishable from the 95th percentile. For both the 5th and 50th percentile, a structural break test (in the sense of Bai and Perron, 2003) finds a (single) change point around 1873–1874 (that is, the beginning of the first parliament after the Second Reform Act).

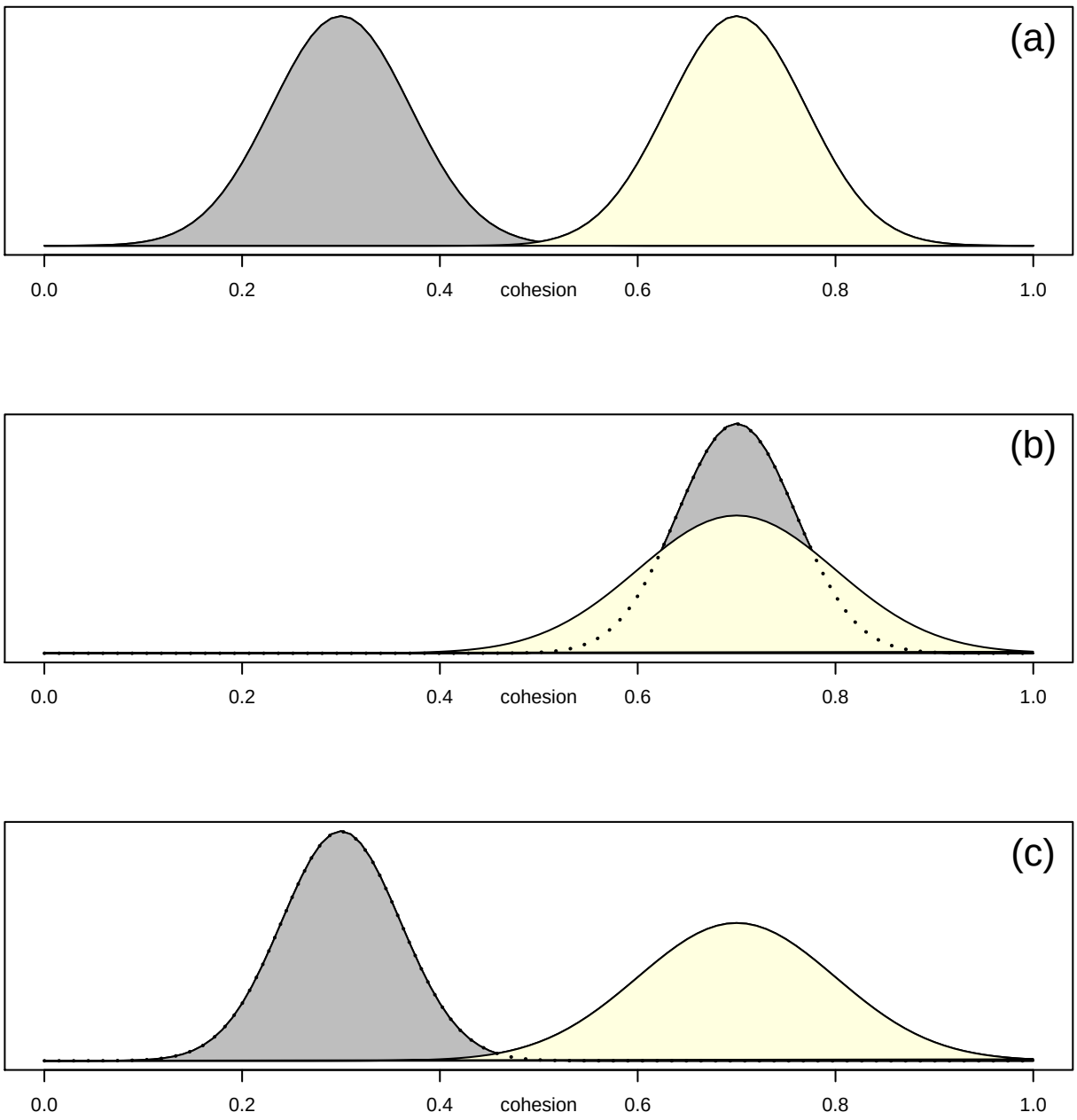


Figure 6: Relative distribution methods: if we observe a change from the dark to the light density in panel (c), we can estimate the extent to which this is the consequence of a location shift (panel (a)), and/or a shape change (panel (b))

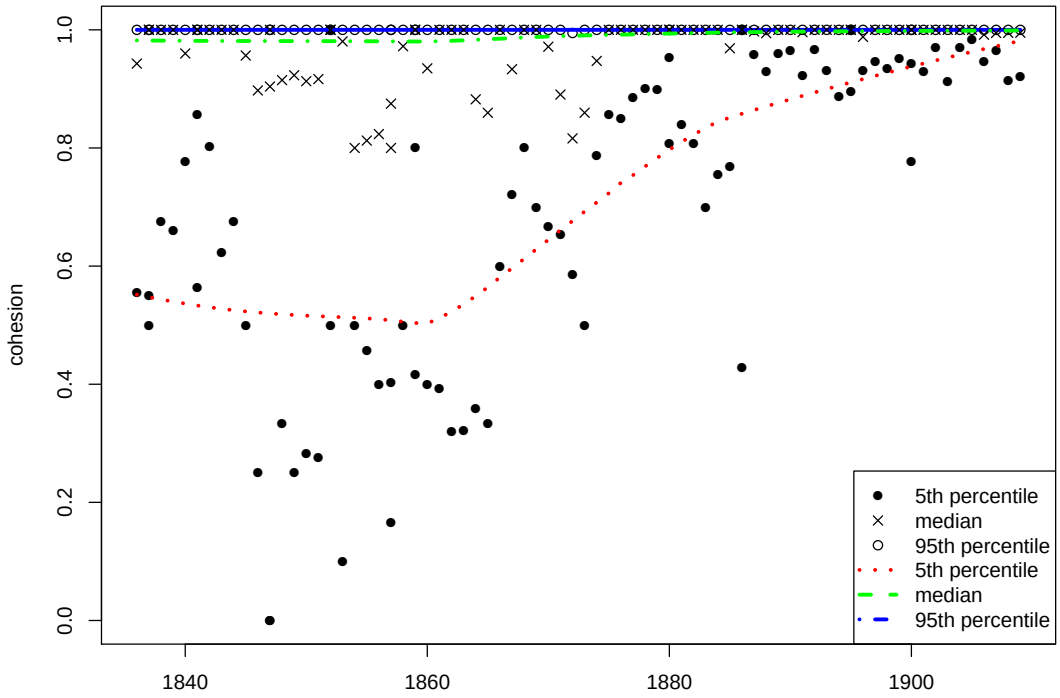


Figure 7: Cohesion in parliament on a year-by-year basis. Lines (lowess fit) trace 95th, median and 5th percentile over time. Note that the 5th percentile (dotted [red] line, solid points) increases markedly during the 1860s.

E 'Joiners' and 'Leavers' by Party

Table 5 reports analysis in which each Conservative MP's per-parliament cohesion score is regressed on parliament dummies and indicators for whether an MP is a 'joiner' or a 'leaver' in a given parliament. In column (1) the only RHS variable aside from parliament dummies is an indicator for whether the MP is in his first parliament; the coefficient of .005 indicates that new Conservative MPs' cohesion scores were only very slightly higher than those of other Conservative MPs (controlling for the parliament), and the effect is not statistically significant at the .1 level. When we interact our "Joiner" indicator with a dummy for pre-1852 (column 2), we see that Conservative joiners in fact voted somewhat more cohesively during the post-1852 period during which cohesion increased overall in Parliament. Columns (3) and (4) indicate that MPs in the last parliament of their career ("Leavers") were not more or less likely to vote with party whips. (The point estimate is negative but not significant at the .1 level.) When we include both indicators in the regression (Columns 5 and 6) the patterns hold: "Joiners" were somewhat more likely to vote with party whips (after 1852), but we cannot reject the null that "Leavers" were just as likely as other MPs to vote with the whips.

Table 6 reports the same analysis for Liberal MPs. The main takeaway is that Liberal "joiners" tended to vote *less* cohesively throughout the period we examine, which is the opposite of the finding for Conservatives and contrary to the replacement story. In column (4) we find suggestive evidence that "Leavers" were somewhat less likely to vote with party whips during the period after 1852 when overall cohesion was increasing. When both indicators are included (columns 5 and 6) we again find that Liberal joiners voted less cohesively but we fail to reject the null that leavers voted as cohesively as other MPs.

F Non-Cabinet Promotion As Inducement?

In Figure 8 and Table 7 we extend the analysis of Section 6 by focusing on promotion to non-cabinet offices such as Junior Lord of the Treasury, Solicitor-General, or Under-Secretary of State for the Home Department. We use the same procedures as above, except here we restrict attention to MPs who had never held any office before (cabinet or otherwise). Comparison of the distributions of cohesion in Figure 8 shows far less of a difference between appointees and non-appointees. Regression analysis in Table 7 similarly indicates a more modest relationship between cohesion and appointment than was true for the cabinet, with a borderline significant "effect" only when we focus on the period before 1868.

Table 5: Are new MPs (joiners) more disciplined? Are exiting MPs (leavers) less disciplined? Conservatives only

	(1)	(2)	(3)	(4)	(5)	(6)
Joiner	0.005	0.0118***			0.0046	0.01**
	[0.003]	[0.0029]			[0.0038]	[0.0037]
Pre-1852		-0.0355***		-0.0342***		-0.0292***
		[0.0067]		[0.0073]		[0.0079]
Joiner × pre-1852		-0.0252**				-0.0186†
		[0.0086]				[0.0105]
Leaver			-0.0058	-0.0038	-0.0062	-0.0051
			[0.0036]	[0.0037]	[0.0047]	[0.005]
Leaver × pre-1852				-0.0072		-0.0014
				[0.0095]		[0.0118]
Joiner × leaver					0.0011	0.0043
					[0.0065]	[0.0062]
Joiner × leaver × pre-1852						-0.0153
						[0.0189]
Adj. R^2	0.125	0.126	0.123	0.123	0.123	0.124
N	5225	5225	5134	5134	5134	5134
Parliament dummies	✓	✓	✓	✓	✓	✓

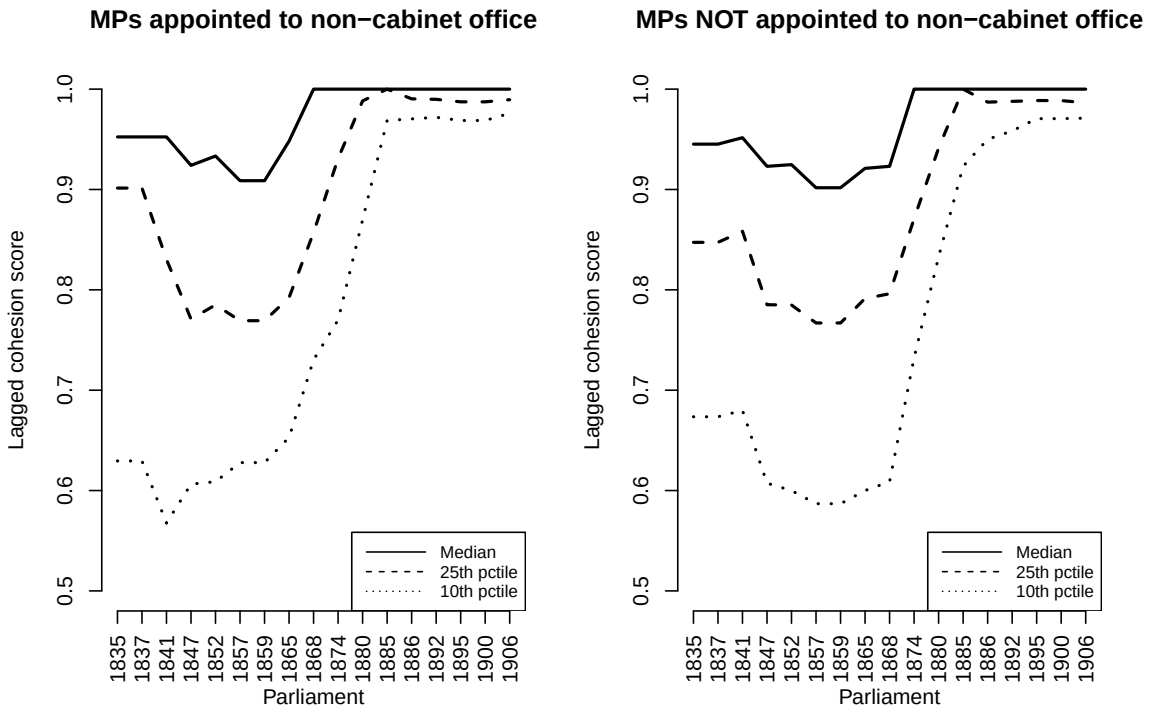
NOTE: Dependent variable is a member's cohesion score in a parliament. Heteroskedasticity-robust standard errors (in brackets) are clustered by member and parliament. Guide to significance codes: *** indicates $p < .001$; ** indicates $.001 < p < .01$; * indicates $.01 < p < .05$; and † indicates $.05 < p < .1$.

Table 6: Are new MPs (joiners) more disciplined? Are exiting MPs (leavers) less disciplined? Liberals only

	(1)	(2)	(3)	(4)	(5)	(6)
Joiner	-0.0157***	-0.0157**			-0.014*	-0.0123*
	[0.0044]	[0.0048]			[0.0056]	[0.0061]
Pre-1852		-0.133***		-0.1485***		-0.1478***
		[0.0092]		[0.0091]		[0.0096]
Joiner × pre-1852		-0.0001				-0.0098
		[0.0113]				[0.015]
Leaver			-0.0051	-0.0089†	-0.0036	-0.006
			[0.0044]	[0.0049]	[0.0055]	[0.0061]
Leaver × pre-1852				0.0143		0.0081
				[0.0107]		[0.013]
Joiner × leaver					-0.0038	-0.0077
					[0.0088]	[0.0097]
Joiner × leaver × pre-1852						0.022
						[0.0226]
Adj. R^2	0.2	0.2	0.186	0.186	0.187	0.187
N	5125	5125	4952	4952	4952	4952
Parliament dummies	✓	✓	✓	✓	✓	✓

NOTE: Dependent variable is a member's cohesion score in a parliament. Heteroskedasticity-robust standard errors (in brackets) are clustered by member and parliament. Guide to significance codes: *** indicates $p < .001$; ** indicates $.001 < p < .01$; * indicates $.01 < p < .05$; and † indicates $.05 < p < .1$.

Figure 8: Are more cohesive voters more likely to be promoted to non-cabinet offices?



NOTE: Figure indicates quantiles of cohesion scores for MPs who were appointed to non-cabinet office in a given parliament (left panel) and those who were not (right panel). To smooth the data each estimate is based on all parliaments within 15 years of the parliament indicated. MPs are included in the analysis only for parliaments in which they have not previously occupied a cabinet office and their party formed a government at some point during the parliament.

Table 7: Are more cohesive voters more likely to be promoted to non-cabinet offices?

	(1)	(2)	(3)
Lagged cohesion score	0.0015	0.0101†	0.0101†
	[0.0085]	[0.0054]	[0.0056]
Seniority	0.0003*	0.0003*	0.0003†
	[0.0002]	[0.0002]	[0.0002]
Age	-0.0003**	-0.0003**	-0.0003**
	[0.0001]	[0.0001]	[0.0001]
Post-1868		0.0621	0.0686
		[0.0439]	[0.051]
Lagged cohesion $\times$ post-1868		-0.0612	-0.0694
		[0.0448]	[0.0519]
Adj. R^2	0.001	0.004	0.005
N	3150	3150	3150
Parliament dummies included			✓

NOTE: Dependent variable is 1 if an MP occupied a cabinet office in a given parliament. MPs are included only if they have not previously occupied a cabinet office and only if their party formed a government at some point during the parliament. Heteroskedasticity-robust standard errors (in brackets) are clustered by member and parliament. Guide to significance codes: *** indicates $p < .001$; ** indicates $.001 < p < .01$; * indicates $.01 < p < .05$; and † indicates $.05 < p < .1$.

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