

Direct Election and Senate Representation

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Abstract

The Seventeenth Amendment to the US Constitution established the direct election of US Senators, which recognized voters rather than state legislatures as senators' principals. Theories of electoral accountability suggest that this change would have increased the incentives for senators to pursue vote-seeking behaviors. We test this claim and evaluate how direct election affected political representation and legislative effort. In so doing, we improve upon the research design and measurement strategies used in previous scholarship while studying a more comprehensive set of indicators of legislative behavior. Our evidence is mixed. While we find no evidence that direct election increased senators' responsiveness to constituency preferences, our results also indicate that direct election reduced party unity and increased senators' rates of participation in the legislative process. These results are driven largely, though not exclusive, by replacement rather than adaptation. Our findings provide new evidence about the legislative consequences of the Seventeenth Amendment and the channels through which these changes were produced.

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Elections are the primary mechanism of popular control over government. By selecting candidates on the basis of which offers the more attractive policies and evaluating incumbents on how they perform in office, voters have the opportunity to choose their policymakers and the policies those officials pursue (e.g Barro 1973; Ferejohn 1986). Though the legislative branch is often viewed as the national institution closest to the American public, for more than a century the members of one of its chambers were chosen indirectly by state legislatures rather than directly by voters. As part of a suite of reforms in the early twentieth century intended to reduce party influence and strengthen mass democracy, the Seventeenth Amendment institutionalized direct election, making voters rather than state legislators the relevant principals for US senators.

In this paper, we leverage the ratification of the Seventeenth Amendment to the US Constitution to study the effect of direct election on officeholder behavior and political representation. Theories of accountability suggest that direct election would increase senator responsiveness to public opinion (e.g., Erikson, MacKuen, and Stimson 2002) and efforts to develop favorable reputations with voters (e.g., Cain, Ferejohn, and Fiorina 1987; Mayhew 1974). Some empirical scholarship finds support for these claims, concluding that direct election increased senators' participation rates (Meinke 2008) and responsiveness to state electorates (Gailmard and Jenkins 2009), reduced party strength in congressional voting patterns (Patty 2008), strengthened property rights over committee assignments (Lapinski 2000), and induced senators to develop more moderate voting records in pursuit of re-election (Bernhard and Sala 2006).¹ Other scholarship suggests the Seventeenth Amendment had little if any effect on senator behavior. For example, Riker (1955) argued that the Seventeenth Amendment simply codified what state legislatures had already enacted through the public canvass, while Schiller and Stewart (2015) concluded that the

¹Though not focused on representation or the behavior of individual senators, Lowande and Peck (2017) show that congressional investigations increased in the Senate after the Seventeenth Amendment, which could reflect the greater relevance of electoral considerations for overseeing the executive branch.

Seventeenth Amendment registered no independent effect since senators were already highly responsive to public opinion and engaged in vote-seeking activities prior to its ratification.

We revisit claims about the Seventeenth Amendment and study its effect on senator behavior from approximately 1880 to 1940. We conduct two primary sets of analyses. In the first, we examine how direct election affected senators' roll call voting records in terms of their responsiveness to public opinion and loyalty to their political parties. In the second, we study how direct election affected senators' provision of legislative effort using data on bill sponsorships and floor speeches. Across all our analyses, we empirically distinguish two possible channels, adaptation and replacement, through which direct election might have affected senator behavior. Using these data, we address limitations in research design and measurement found in prior studies of the Seventeenth Amendment. Perhaps most importantly, we leverage various samples of US House members, who were unaffected by the Seventeenth Amendment, to serve as untreated comparison units. This inclusion allows us to address several sources of potential confounding.

Overall, our results indicate that the Seventeenth Amendment had limited effects on senator behavior and political representation. We find no evidence that direct election increased Senators' responsiveness to constituency preferences; in fact, most results indicate that responsiveness decreased after the Seventeenth Amendment was ratified. However, we do find that senators were less responsive to the partisan composition of state legislatures and exhibited lower rates of party loyalty after direct election, particularly through replacement. We show that direct election increased senators' sponsorship of non-private legislation and the number of legislative speeches given by legislators. Our results provide some evidence that the Seventeenth Amendment affected legislative behavior, particularly through the replacement of indirectly elected senators with their directly elected successors, but suggest it was less transformative for improving constituency representation than previous accounts have concluded.

Direct Election and Incentives for Legislative Behavior

Proponents of direct election argued that constitutional reform was necessary to improve the political character and operations of the Senate. In arguing for reforming the process by which senators were chosen, Haynes (1906, 165-166) charged that “the Senate has ceased to be representative of the commonwealths or of the people of the United States... some of our greatest States continue to be represented by senators without a glimmering of statesmanship, men who owe their elevation to the arts of the ward politician...” Upon the ratification of the Seventeenth Amendment, Secretary of State William Jennings Bryan predicted momentous change in the class and characteristics of Senators, remarking that “instead of having the Senate filled up with representatives of predatory wealth who use their power to oppose the things that the people love—we will find that the honor of a position in that body will be reserved as a prize with which to reward those who have proven themselves capable of the discharge of public duties and men to be trusted with the people’s interests” (quoted in Haynes 1924, 252). By placing the power to select senators directly in the hands of voters rather than state legislatures, these accounts argued that the Seventeenth Amendment would democratize the Senate, reduce corruption, and improve representation (see Merriam 1920, 115).

Theories of electoral accountability provide a framework for evaluating these arguments and considering how the Seventeenth Amendment affected legislative behavior in the Senate.² By fundamentally changing the mechanism through which senators were selected and who had the power to decide whether to retain them in office, the Seventeenth Amendment would be expected to create incentives for senators to behave in ways that would generate support from their new principals: voters. These newfound incentives could have affected senator behavior in several ways. First, if voters evaluate senators based on how well they have represented their policy views, then we would expect direct election to increase senators’ responsiveness to constituency

²For an overview of models of electoral accountability, see Ashworth (2012).

preferences. Second, direct election may have increased senators' efforts to develop a personal vote through activities meant to cultivate favorable reputations with voters (Cain, Ferejohn, and Fiorina 1987; Mayhew 1974). To the extent that voters used different criteria to choose among senate candidates than state legislatures, the Seventeenth Amendment could also have influenced the composition of the Senate chamber. As such, both contemporary proponents of direct election and theoretical perspectives on political accountability contend that the direct election of Senators would produce significant change both in the characteristics of individual Senators and the behaviors undertaken in office.

Other perspectives, however, raise doubts about whether the Seventeenth Amendment produced the effects predicted by its advocates. These accounts emphasized three aspects of the process through which senators were chosen at the turn of the twentieth century. First, prior to the Seventeenth Amendment, state legislatures had relatively few means with which to sanction senators after they were selected (Schiller and Stewart 2015, 46). Frequent turnover among state legislators meant that newly appointed senators could be reasonably certain that the legislative coalition that originally placed them in office would not endure for the length of their term, thereby lessening incentives for senators to heed that coalition's preferences and heightening incentives to build a positive reputation with constituents and local party officials. Second, the popular canvass, in which Senate candidates secured endorsements from state legislative candidates and voters selected state legislators on the basis of whom they supported for the Senate (Riker 1955), may have provided the public with a means of registering their preferences over Senate candidates prior to the Seventeenth Amendment.³ More generally, senators who served prior to the Seventeenth Amendment sought multiple terms in office and engaged in activities intended to cultivate popular support (Schiller 2006; Sievert 2023; Stewart 1992), indicating that senators were already occupied with building personal reputations with their constituencies prior

³Schiller and Stewart (2015, 201) reject this notion, arguing that the popular canvass was not a proxy for direct election prior to the Seventeenth Amendment.

to direct election. These complementary perspectives suggest that the Seventeenth Amendment did not significantly change senators' incentives to seek support from their relevant principals. Finally, other accounts argue that reforms implemented in the years preceding ratification of the Seventeenth Amendment—most notably the state primary—substantially strengthened popular control over senator selection at the expense of state legislatures (Amar 1996; Brown 1922; Caffey 1905; Haynes 1924, 1938). This scholarship suggests that while the Seventeenth Amendment in principle could have substantially changed legislative behavior in the Senate, the context in which it was ratified limited its ability to register independent effects.

Evidence on the Effect of the Seventeenth Amendment

The existing quantitative evidence generally supports the view that the Seventeenth Amendment affected the composition of the Senate and the behavior of its members. In an early analysis, (Rogers 1926, 115-116) concluded that direct election had increased turnover within the Senate and led to the selection of Senators who otherwise would have been unlikely to be appointed by state legislatures. Other research finds some evidence that Senators elected post-Amendment were less likely to come from wealthy backgrounds or political dynasties, and were more likely to have prior government experience (Crook and Hibbing 1997). MacKenzie (2014), for example, shows that senators who served after the Seventeenth Amendment had greater previous political experience. Studying the partisan composition of the Senate, Schiller and Stewart (2015, chapter 6) show that the Seventeenth Amendment immediately increased the number of Democrats serving in the Senate and that these partisan advantages grew over time. And at the aggregate level, direct election strengthened the relationship between national partisan preferences and the composition of the Senate chamber (Crook and Hibbing 1997; Engstrom and Kernell 2007), although this finding may be confined to the South (Rogers 2012).

At the level of individual senators, other work concludes that the Seventeenth Amendment affected legislative behavior. This scholarship provides evidence of constituency-directed shifts

in Senators' roll call voting (Bernhard and Sala 2006; Gailmard and Jenkins 2009; Neiheisel and Djupe 2017), participation and bill sponsorship (Meinke 2008), policy priorities (Gray, Jenkins, and Potter 2022), and amendment activity (Carson et al. 2012). For example, studying the responsiveness of senators' roll call voting records to the ideological preferences of voters and state legislatures, Gailmard and Jenkins (2009) find that Senators became significantly less responsive to the preferences of the state legislature and more responsive to the mass electorate after ratification of the Seventeenth Amendment. Similarly, Bernhard and Sala (2006) conclude that the Seventeenth Amendment provided incentives for senators to moderate their ideologies and partisan commitments as their election neared, suggesting an awareness of the need to demonstrate constituency ideological "fit" in campaigning for re-election. On the other hand, while Neiheisel and Djupe (2017) also conclude that the Seventeenth Amendment affected legislative behavior in the Senate, they argue that it made senators more extreme rather than more moderate, which they attribute to the loss of a "moral regulator" in the selection of Senators as this process moved from state legislators to voters.

Other work finds that direct election changed how senators engaged in legislative activity. Meinke (2008) finds that elected senators sponsored bills and participated in roll call votes at higher rates relative to senators who were originally chosen by state legislatures in the same period. Similarly, Carson et al. (2012) show that direct election was associated with greater rates of amending activity in Senate, suggesting that senators perceived greater incentives for position-taking. Gray, Jenkins, and Potter (2022) take a different tack, arguing that the shift from indirect to direct election led Senators to adopt different sets of policy priorities. They show that elected senators were less likely to serve on foreign policy-focused committees and more likely delegate foreign policy autonomy to the executive branch due to voters' relative lack of information and interest in foreign policy issues. Finally, Lowande and Peck (2017) study the effect of the Seventeenth Amendment on overall levels of oversight activity in the Senate. They show that while there was no difference in Senate investigations between periods of divided and unified

government prior to the Seventeenth Amendment, after its ratification divided government was associated with a sizable increase in oversight activity.

Moreover, scholarship outside of the context of the US Congress and the Seventeenth Amendment is also relevant for considering the behavioral effects of direct election. For example, research in comparative politics shows that politicians are more likely to exhibit personal vote-seeking characteristics (Shugart, Valdini, and Suominen 2005) and greater attentiveness to local issues (Crisp et al. 2004) in electoral systems where voters have the opportunity to consider the attributes and experience of individual candidates. Micozzi (2013) studies the shift from indirect to direct election in the Argentine Senate in 2001 and finds that this change resulted in elected Senators submitting more legislation than their appointed colleagues, and particularly bills that targeted their home state. Collectively, this research suggests that institutional environments shape the incentives of political officials and is consistent with the expectation that direct election would have increased Senators' motivation to win the support of voters through various means.

We test the hypothesis that the Seventeenth Amendment increased Senators' vote-seeking behaviors. If direct election improved the character of political representation in the Senate and heightened incentives for senators to develop a personal vote, we expect to find that senators were more responsive to the preferences of their constituencies and expended greater effort in the legislative process. In evaluating these expectations, we make several contributions. First, we combine original data collection with existing data sources to study a more comprehensive range of dependent variables than is evaluated in any individual piece of research discussed above. Second, and more importantly, we address limitations in the measurement strategies and research designs used by previous studies. Critically, while previous scholarship distinguishes senators who served before and after the Seventeenth Amendment was adopted (Bernhard and Sala 2006; Carson et al. 2012; Gailmard and Jenkins 2009; MacKenzie 2014; Neiheisel and Djupe 2017) or compares behavior among senators who served at the same time but had been elected rather

than appointed (Gray, Jenkins, and Potter 2022; Meinke 2008), both research designs are vulnerable to potential sources of confounding. To the extent that legislative behavior or constituency demand for certain forms of representation followed secular trends, for example, these designs are unable to distinguish the effects of the Seventeenth Amendment from other influences on senator behavior that are correlated with time. Extending the approach from (Sievert 2023), we address this limitation by comparing the effect of the Seventeenth Amendment on the behavior of senators to the behavior of similar House members who were not affected by the change in electoral institutions.⁴ Third, and finally, we evaluate the potential channels through which representation in the Senate might have changed by distinguishing the effects of reduced moral hazard via changes in behavior within legislators and reduced adverse selection via voters' improved ability (relative to state legislatures) to select high-quality officeholders. This allows us to investigate potential theoretical mechanisms through which the Seventeenth Amendment might have affected legislative behavior in the Senate.

Data and Research Design

We study the effects of the Seventeenth Amendment on two aspects of legislative behavior: representation and effort. Both sets of outcomes have been used in scholarship that evaluates the effects of a variety of institutional changes on legislative behavior (e.g., Moskowitz and Rogowski Forthcoming) and have also been studied in previous scholarship on the Seventeenth Amendment (Bernhard and Sala 2006; Gailmard and Jenkins 2009; Meinke 2008; Schiller and Stewart 2015). Except where noted, our measures characterize legislative behavior from the 47th Congress (1881-

⁴Our strategy relates to, but distinct from, studies that supplement an analysis of Senate behavior before and after the amendment was ratified with an analysis of House behavior for the same time period (Gailmard and Jenkins 2009; Lowande and Peck 2017).

83) through the 77th Congress (1941-43).⁵ Our analysis includes data for all senators who served full terms in each congress during this period. For all our dependent variables, the unit of analysis is a senator i serving in congress c .

We evaluate representation primarily by studying the responsiveness of senator roll call voting records to constituency preferences. Our approach is generally similar to Gailmard and Jenkins (2009) but differs in a few key respects. We measure the ideological nature of legislative voting records using the first dimension of scores developed by Nokken and Poole (2004) and retrieved from Lewis et al. (2022). These scores characterize legislator ideology on the basis of roll call votes cast in each congress and range from -1 to 1, where more positive scores indicate more ideologically conservative voting patterns. Unlike NOMINATE scores (Lewis et al. 2022), which assume that legislators occupy a constant ideological position across their congressional careers, the Nokken-Poole procedure recovers estimates of legislative ideology separately for each congress. These estimates are particularly appropriate given our interest in testing whether the Seventeenth Amendment affected legislative behavior.

Nokken-Poole scores improve upon measurement strategies used in previous scholarship in several ways. They are more appropriate than the original DW-NOMINATE scores used in Gailmard and Jenkins (2009), which generate dynamic estimates of legislative behavior but constrain them to move in a linear fashion and at a fixed rate across time. This constraint makes it difficult if not impossible to identify whether some institutional change—here, the adoption of the Seventeenth Amendment—is plausibly associated with the timing of changes in legislative behavior. Bernhard and Sala (2006) attempt to address this issue by estimating W-NOMINATE scores separately for each congress. To make these scores comparable across congresses, they use the adjustment proposed by Groseclose, Levitt, and Snyder (1999). However, this procedure restricts legislators’ preferences from changing in ways that are correlated with time, which raises con-

⁵This is the same period studied by Gailmard and Jenkins (2009), although we include all congresses rather than only those corresponding with presidential election years.

cerns about the accuracy of the measures given that the Seventeenth Amendment may have generated constituency-induced changes in legislator preferences. In contrast, Nokken-Poole scores allow us to test whether individual senators modified their behavior upon and/or following direct election.⁶

Studying responsiveness also requires us to measure constituency preferences. Following common practices in the literature (Canes-Wrone, Brady, and Cogan 2002; Carson 2005; Moskowitz and Rogowski Forthcoming), we characterize the preferences of state constituencies using the two-party Republican presidential vote shares from the most recent election.⁷ Congressional Republicans were to the ideological right of congressional Democrats during this time period, meaning that if legislators were responsive to state preferences in general, we should expect a positive correlation between Republican presidential vote share and the conservatism of senators' voting records. If the Seventeenth Amendment increased senators' responsiveness to state electorates, we further expect that the correlation between state preferences and legislator ideology would increase following its adoption.

We also measure the quality of representation using senators' party unity scores. These scores

⁶In Table A.5, we show that our substantive conclusions do not change when using static NOMINATE scores rather than Nokken-Poole scores. Since static scores do not vary within legislators, we can only estimate models that include constituency fixed effects (i.e., the first four models from Table 1. When doing so, we obtain coefficient estimates very similar to those reported in Table 1, which provide no evidence that senators were more responsive to constituency preferences after the Seventeenth Amendment was ratified.

⁷For instance, we link a senator's voting record in the 1899-1901 legislative session (the 56th Congress) to their state's voting patterns in the 1896 presidential election. This contrasts with Gailmard and Jenkins (2009), who linked the same senator's voting record to the results of the 1900 election. We believe our approach is more appropriate given that theoretical accounts of responsiveness posit that changes in constituency opinion cause changes in elite behavior.

are based on party unity votes, which occur when at least half of Democrats in the chamber vote against at least half of Republicans. Party unity scores index the share of party unity votes on which a legislator has voted with his or her party. Thus, higher scores characterize legislators who exhibit more party loyalty in their roll call voting patterns. Previous research indicates that high levels of party loyalty can be an electoral liability for legislators (Carson et al. 2010). If direct election increased the importance of voter evaluations for legislators, then we expect that party unity decreased after the adoption of the Seventeenth Amendment.

We measure legislative effort with two publicly observable indicators that are frequently used in previous scholarship (Dal Bó and Rossi 2011; Fourinaies and Hall Forthcoming): bill sponsorship and floor speech activity. We measure bill sponsorship through original data compilation on Senate sponsors for the more than 130,000 bills introduced between the 46th (1879-1881) Congress and 71st (1929-1931) Congress. Based on the bill summaries, we construct a keyword-based dictionary method to identify private pension bills, private military bills, and private relief bills. The dictionary classification method yields an overall accuracy of 98.6 percent. For more details, on the Senate sponsorship data, classification, and validation, see Appendix Section A.1. We complement these data with similar data on bill introductions in the House from Finocchiaro and MacKenzie (2018). Their data cover the same time frame and bills are placed into similar categories.⁸ We measure the frequency with which senators gave floor speeches in the chamber using data from Gentzkow, Shapiro, and Taddy (2019). As we describe below, we use a variety of thresholds and measurement strategies to characterize speech activity. Across these indicators,

⁸We do not claim that our categorization scheme is exactly the same as Finocchiaro and MacKenzie (2018), since we use a different approach for categorizing the bills in our data. However, since the purpose of the House data is to provide a benchmark against which we assess changes in Senator behavior with the ratification of the Seventeenth Amendment, it is importantly primarily that any differences in how bills are categorized in the House and Senate are constant for the period we are studying.

we expect to observe higher rates of legislative effort following the Seventeenth Amendment if it increased the incentives for senators to cultivate a personal vote.

Empirical Strategy

We use the panel nature of our data to implement several research designs for evaluating the effect of the Seventeenth Amendment. In each design, senators who served after ratification of the Seventeenth Amendment are considered members of the treated group. The key difference across our designs concerns the nature of the comparison group.

First, we compare senators who served after the Seventeenth Amendment was ratified with senators who served before its ratification. This pre-post design is similar to that used by Gailmard and Jenkins (2009). However, due to the time-varying nature of our dependent variables, we are able to evaluate differences in behavior among (1) the same senators who served in both periods and (2) among different senators who represented the same state in both periods. The former allows us to examine whether the Seventeenth Amendment produced behavioral change (i.e., adaption), while the latter allows us to distinguish whether senators elected after the Seventeenth Amendment behaved differently from those elected before it (i.e., replacement).

Second, we compare changes in senator behavior before and after the adoption of the Seventeenth Amendment with the behavior of other legislators who represented the same constituency but were plausibly unaffected by the direct election of senators. Initially (following Sievert 2023), we include two groups of House members: those who were the single representative from a state with one House seat, and those who represented an at-large district in a state that elected one or more of its representatives through at-large districting plans. In both cases, representatives had exactly the same electoral constituencies and needed to secure statewide support to win. However, House members would not have been affected by the adoption of the Seventeenth Amendment, which allows us to implement a difference-in-differences design whereby we compare changes in senator behavior before and after the Seventeenth Amendment to changes in

House member behavior during the same period for representatives from the same state. However, because there are relatively few at-large representatives in any particular congress, we expand the comparison group to include all House members. This provides a substantially larger group of legislators and constituencies who experienced no change in the method of selection. This approach also expands the range—and, potentially, the dissimilarity—of constituencies and legislators included in the data.

Together, these research designs complement each other. For example, the pre-post design focused only on senators may be confounded by secular trends in legislative behavior. On the other hand, the designs involving House members may include comparison groups whose political experiences are different from senators and whom the parallel trends assumption is more tenuous. To the extent that each design produces similar results, however, would strengthen our confidence in our conclusions.

Given the discussion above, we estimate the average differences in representational outcomes between the pre- and post-amendment periods within units while controlling for common time trends and time-varying covariates. This strategy distinguishes the effects of direct election from other secular trends and time-invariant characteristics of legislators, states, and/or districts that may also affect legislator behavior. We estimate the following general model:

$$Y_{ic} = \lambda_i + \delta_c + \beta \text{Direct election}_{ic} + \mathbf{X}\Omega_{ic} + \epsilon_{ic}, \quad (1)$$

where Y is the relevant dependent variable and i and c index legislators and congresses, respectively. The main independent variable is an indicator, *Direct election*, that characterizes whether senators were elected directly by voters in their state. Because the Seventeenth Amendment was ratified in 1913, this indicator takes a value of one after that year and zero before.⁹ The coefficient

⁹This specification is preferable to one where this measure takes a value of one when a senator was elected under direct election. Electoral motivations should lead senators to anticipate how they will be viewed by the relevant principals, and theoretically we would expect a change in

β thus is the key parameter of interest. If the Seventeenth Amendment increased legislator effort, we expect to observe positive estimates for β .

We include unit fixed effects (λ_i) to control for observed and unobserved time-invariant attributes that may affect legislative behavior. In separate models, we estimate unit fixed effects for legislators and constituencies, which allow us to test different substantive hypotheses about the effects of the Seventeenth Amendment. For models that include only data on senator behavior, we thus estimate models that include fixed effects for either state or senator. The former allows us to hold constant the characteristics of states that might be associated with demand for particular forms of legislative behavior, and thus the estimate of β is identified using changes in the behavior of senators before and after the Seventeenth Amendment who represent the same state. The latter specification holds constant the characteristics of individual senators and thus the estimates of β reflect within-senator changes in behavior before and after the Seventeenth Amendment was ratified. We extend this general approach in models that include House members as comparison groups, where we alternately include district fixed effects (which is state for senators and district for representatives) and legislator fixed effects.

In some models that include only the Senate, as well as all models that include House members as comparison groups, we also include time fixed effects (δ_c) for each congress. This term accounts for secular trends in legislative behavior, legislative capacity, and constituent demands. In these models, we thus estimate the effect of the Seventeenth Amendment on senator behavior relative to House members. To the extent that estimates of β continue to be statistically significant in these models would provide evidence that the Seventeenth Amendment affected senator behavior over and above any common trends that affected both senators and House members. Finally, ϵ_{it} is a random error term, which we cluster on state in all models.

electoral institution to affect how senators position themselves for the next contest.

Results

We begin by studying how the Seventeenth Amendment affected senators' ideological responsiveness to the preferences of their new principals: the voters in their state. Following the approach from Gailmard and Jenkins (2009), we modify equation (1) slightly to accommodate this analysis and interact the indicator for *Direct election* with the measure of state presidential vote share in the most recent election. If senators were more responsive to the preferences of their constituency after the Seventeenth Amendment was adopted—that is, if senators compiled voting records that were more conservative as their state became more conservative—we expect to observe a positive coefficient on the interaction term.

Table 1 shows the results of this analysis. The first two columns include data only for senators. Column (1) shows results when including fixed effects for states, to which the model reported in column (2) adds congress fixed effects.¹⁰ Column (3) includes at-large House members, and column (4) includes all House members, where both models contain fixed effects for congress and constituency.

The results are highly consistent across each model specification. The coefficient for the constituency conservatism term is positive and statistically significant, indicating that senators and representatives from more conservative states have more ideologically conservative roll call voting patterns. More importantly, the coefficient for the interaction term is consistently negative and statistically significant in each model. Contrary to previous scholarship, this finding indicates that state electorates chose senators who were *less* responsive to constituency preferences after the Seventeenth Amendment was ratified.

Columns (5) through (8) show results for similar model specifications that include legislator rather than constituency fixed effects. With this model specification, the estimates characterize

¹⁰Note that there is no estimate in column (2) for the constituent term for *Direct election*, as it is perfectly collinear with time.

how changes in constituency preferences are associated with changes in an individual legislator’s behavior, and whether that relationship differed for senators following ratification of the Seventeenth Amendment. While the estimates for the constituent term are consistently positive, only one of the four is statistically significant and each of them is small in magnitude. These results suggest that legislators are generally ideologically consistent across time (as Poole 2007, argues) and often do not meaningfully change their roll call voting patterns as constituents’ preferences change. Moreover, the coefficients for the interaction term are all small in magnitude and negative in three of the four specifications.

Table 1: Responsiveness and the Seventeenth Amendment

	<i>Dependent variable:</i> Nokken-Poole roll call scores							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constituency Conservatism	0.656*	0.528*	0.563*	0.384*	0.014	0.026	0.054	0.070*
	(0.124)	(0.093)	(0.084)	(0.059)	(0.047)	(0.044)	(0.048)	(0.025)
Constituency Conservatism x Direct Election	-0.271*	-0.350*	-0.331*	-0.286*	-0.003	0.017	-0.003	-0.082*
	(0.104)	(0.095)	(0.080)	(0.073)	(0.061)	(0.059)	(0.066)	(0.038)
Direct Election	0.141*		0.193*	0.069	0.030		-0.005	0.033
	(0.055)		(0.043)	(0.054)	(0.030)		(0.064)	(0.022)
Congress fixed effects		X	X	X		X	X	X
Constituency fixed effects	X	X	X	X				
Member fixed effects					X	X	X	X
House comparison	None	None	At-large	All	None	None	At-large	All
Observations	2,598	2,598	3,006	12,906	2,598	2,598	3,006	12,906

Note: Entries are linear regression coefficients with standard errors clustered on states in parentheses. The dependent variable is members’ Nokken-Poole roll call score. Congress, constituency, and member fixed effects are included where indicated. Constituency fixed effects are based on state for Senators and at-large representatives and redistricting-specific House districts for all other representatives. “House comparison” indicates whether the comparison group included any House members, House members from at-large districts only, or all House members. * $p < 0.05$ (two-tailed tests).

In Table 2, we follow Gailmard and Jenkins (2009) and account for both the partisan composition of state legislatures—who selected senators prior to direct election—and the preferences of state constituencies. We include a measure of *Legislature conservatism*, which characterizes the share of state legislative seats held by Republican members, and its interaction with *Direct election*. These measures allow us to examine the joint variation in responsiveness to senators’ principals as the Seventeenth Amendment was ratified. Columns (1) through (3) parallel the first three columns of Table 1 (that is, they include constituency fixed effects) while columns (4)

through (6) parallel columns (5) through (7) and include legislator fixed effects. Note, however, that we do not estimate models shown in columns (4) and (8) of Table 1, as it is not feasible to link the composition of the state legislative seats to the geographic boundaries of districted House members in this period.

Several findings from Table 2 are especially notable. First, we continue to find no evidence that responsiveness increased after the Seventeenth Amendment. While the coefficients for the interaction between constituency conservatism and direct election are consistently positive, they are imprecisely estimated and none is statistically distinguishable from zero. Second, we find some evidence that senators' responsiveness to state legislatures decreased after the Seventeenth Amendment. The constituent term for *Legislative conservatism* is positive, indicating that senators from states with more Republican controlled legislatures exhibited more conservative voting patterns, and its interaction with the Seventeenth Amendment indicator is negative, indicating that this relationship was attenuated after direct election. However, these effects are limited to the models with constituency fixed effects but largely dissipate in the models containing legislator fixed effects. This indicates that changes in senator responsiveness to the partisan composition of state legislatures was driven largely by changes in the senators who represented a given state rather than by adaptation in a senator's behavior. Finally, even in the models with constituency fixed effects, senator responsiveness to state legislatures does not reduce to zero after the Seventeenth Amendment. Instead, the interaction terms indicate that responsiveness declined by 50 to 65 percent (e.g., 0.618–0.334 in column 3), suggesting that legislative voting records continued to be responsive to the partisan composition of state government even after the mechanism of selection changed hands.

The results in Table 1 and 2 provide no evidence that the Seventeenth Amendment increased popular control over the Senate. And while we do find some evidence that senator responsiveness to state legislatures reduced, it did not disappear entirely. These findings contrast with the arguments from proponents of direct election and the conclusions from recent empirical scholarship

Table 2: Responsiveness and the Seventeenth Amendment

	<i>Dependent variable:</i> Nokken-Poole roll call scores					
	(1)	(2)	(3)	(4)	(5)	(6)
Constituency conservatism	0.152 (0.113)	0.060 (0.099)	0.173 (0.097)	-0.013 (0.054)	-0.003 (0.056)	0.027 (0.064)
Constituency conservatism x Direct election	0.201 (0.150)	0.278 (0.156)	0.160 (0.157)	0.063 (0.070)	0.110 (0.083)	0.076 (0.088)
Legislature conservatism	0.701* (0.075)	0.667* (0.084)	0.618* (0.078)	0.044 (0.029)	0.052 (0.037)	0.054 (0.039)
Legislature conservatism x Direct election	-0.391* (0.070)	-0.415* (0.082)	-0.334* (0.086)	-0.061 (0.039)	-0.065 (0.044)	-0.060 (0.049)
Direct election	0.111* (0.054)		0.138* (0.059)	0.038 (0.030)		-0.029 (0.062)
Congress fixed effects		X	X		X	X
Constituency fixed effects	X	X	X			
Member fixed effects				X	X	X
House comparison	None	None	At-large	None	None	At-large
Observations	2,360	2,360	2,733	2,360	2,360	2,733

Note: Entries are linear regression coefficients with standard errors clustered on states in parentheses. The dependent variable is members' Nokken-Poole roll call score. Congress, constituency, and member fixed effects are included where indicated. Constituency fixed effects are based on state. "House comparison" indicates whether the comparison group included any House members or House members from at-large districts only. *p<0.05 (two-tailed tests).

(Gailmard and Jenkins 2009), offering little support for the view that the Seventeenth Amendment increased senators' ideological responsiveness to their constituents' preferences or improved the caliber of representation in the Senate.

Party Unity

While our results above provide no evidence that the Seventeenth Amendment increased senators' responsiveness to public opinion, proponents of electoral reform also claimed that it would reduce senators' loyalties to their political parties. We examine whether direct election provided incentives for senators to demonstrate greater independence from their party as their

future in office no longer depending on currying favor with political elites in the state legislature.

Table 3 shows the results of our analyses which use party unity scores from Lewis et al. (2022) as the dependent variable. The model specifications are similar to those from Table 1. The models reported in columns (1) through (3) each include constituency fixed effects. Column (1) shows results when analyzing only the behavior of senators while columns (2) and (3) also include House members along with fixed effects for each congress. In column (2), we include House members for whom we have data on the presidential vote share in their district as our comparison group. Our intention here is to use only those representatives whom we also included in Table 1 and 2 so that the results of our analysis of party unity can be compared to the findings from those tables. In column (3), we expand the comparison group to all House members. Columns (4) through (6) largely parallel those from the first three columns but substitute legislator fixed effects for constituency fixed effects.

In the first three models, we find that the Seventeenth Amendment reduced party loyalty in senator voting behavior. The coefficients are consistently negative and distinguishable from zero, indicating that the Seventeenth Amendment significantly reduced party loyalty among senators. The coefficients for the last three models, however, are inconsistently signed and considerably smaller in magnitude, and none of them is statistically distinguishable from zero. These patterns suggest that the decline in party loyalty was driven by voters selecting senators who subsequently exhibited lower levels of party loyalty rather than by within-senator changes in their partisan commitments.

Legislative Effort

We now examine how the Seventeenth Amendment affected observable indicators of legislative efforts. The switch to direct election might have provided new incentives for legislators to demonstrate to their constituents that they were active in legislative debates, were working to advance constituent interests and priorities, and to publicly announce their positions on salient

Table 3: The Seventeenth Amendment and Party Unity

	<i>Dependent variable:</i> Party unity scores					
	(1)	(2)	(3)	(4)	(5)	(6)
Direct election	-7.870*	-2.221*	-2.376*	-0.834	0.514	0.236
	(0.937)	(0.949)	(0.938)	(1.404)	(1.042)	(1.027)
Congress fixed effects		X	X		X	X
Constituency fixed effects	X	X	X			
Member fixed effects				X	X	X
House comparison		NMPV	All		NMPV	All
Observations	2,626	12,902	14,321	2,626	12,902	14,321

Note: Entries are linear regression coefficients with standard errors clustered on states in parentheses. The dependent variable is members' party unity scores in a given congressional term. Congress, constituency, and member fixed effects are included where indicated. Constituency fixed effects are based on state for Senators and at-large representatives and redistricting-specific House districts for all other representatives. "House comparison" indicates whether the comparison group included House members with non-missing values for district presidential vote share (NMPV) that were included in Table 1 for all House members. * $p < 0.05$ (two-tailed tests).

legislation.

We begin by studying the effects of the Seventeenth Amendment on bill introductions. We use our new data on Senate bill introductions to compare patterns of bill sponsorship among senators with bill sponsorship rates among House members using data from Finocchiaro and MacKenzie (2018). This strategy improves upon other research that examines bill sponsorship in the Senate in this period (Meinke 2008; Schiller 2006) and allows us to study the effects of the Seventeenth Amendment net of any other political developments during this period that would have had common effects on the behavior of both senators and representatives.

Table 4 shows the results. Following the models we estimated in the analyses above, Panel A shows the results when estimating models with constituency fixed effects while Panel B shows results when estimating models with legislator fixed effects. Each panel shows the results for four (not mutually exclusive) categories of bills—all bills, non-private legislation, all private legislation, and all private pension legislation. We estimate these models when comparing senator behavior

to House members with non-missing values of the presidential vote share measure (the House members included in Table 1) and when comparing senators to all House members. All models in both panels include congress fixed effects.

The results are generally consistent across model specifications and the specific nature of the House comparison group. Panel A shows that the Seventeenth Amendment significantly increased sponsorship rates in the Senate for non-private legislation. This category includes legislation that is more general in its orientation and, as a result, concerns more substantive policy matters. Interestingly, the coefficients for private legislation are both negatively signed, indicating that senators introduced fewer pieces of private legislation after the Seventeenth Amendment was ratified, though neither of the coefficients is statistically distinguishable from zero. The coefficients for the other categories are smaller in magnitude and also not statistically significant.

Panel B shows that individual senators sponsored legislation at greater rates after the Seventeenth Amendment, and that this primarily reflects an increase in the volume of non-private legislation. Thus, our evidence indicates that Senators increased their effort to introducing more general legislation after the Seventeenth Amendment, perhaps in an effort to develop reputations as policy entrepreneurs and to signal to their constituents their positions on salient issues. These patterns indicate that not only did voters select Senators who were more active in the legislative process upon direct election, but that individual Senators themselves modified their behavior in response to this reform. These results are generally consistent with those from Meinke (2008), and our ability to compare sponsorship rates of Senators against their House colleagues over the same time period increases our confidence in these conclusions.

Table 5 studies how the Seventeenth Amendment affected senators' participation in floor debates. Following Moskowitz and Rogowski (Forthcoming), our dependent variable is the number of speeches at least 200 words in length that were made on the floor of the chamber in each congress. However, our results do not depend on this particular characterization as we obtain similar results when using the total number of words spoken on the chamber floor in a given

Table 4: The Seventeenth Amendment and Bill Introductions

Panel A: Constituency fixed effects								
	All		Non-Private		Private		Private Pension	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Direct election	0.053 (0.101)	0.062 (0.010)	0.276* (0.092)	0.270* (0.089)	-0.133 (0.116)	-0.112 (0.114)	0.030 (0.106)	0.044 (0.105)
Panel B: Legislator fixed effects								
	All		Non-Private		Private		Private Pension	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Direct election	0.224* (0.110)	0.228* (0.105)	0.321* (0.106)	0.334* (0.099)	0.088 (0.120)	0.081 (0.117)	0.121 (0.122)	0.143 (0.121)
Unit fixed effects	X	X	X	X	X	X	X	X
Congress fixed effects	X	X	X	X	X	X	X	X
House comparison	NMPV	All	NMPV	All	NMPV	All	NMPV	All
Observations	10,275	11,135	10,275	11,135	10,275	11,135	10,275	11,135

Note: Entries are linear regression coefficients with standard errors clustered on states in parentheses. The dependent variable is the logged value of the number of legislative bills introduced (plus one) in the category identified at the top of each column. Congress, member, and constituency fixed effects are included where indicated. “House comparison” indicates whether the comparison group included House members with non-missing values for district presidential vote share (NMPV) that were included in Table 1 for all House members. *p<0.05 (two-tailed tests).

congress.¹¹ The first three models include constituency fixed effects, and the second and third of them include congress fixed effects as well as House members as the comparison group. The last three models follow similar structure but include legislator rather than constituency fixed effects.

We find consistent evidence that senators made public comments on the chamber floor more frequently after the Seventeenth Amendment was ratified, as the coefficient for *Direct election* is positive and statistically significant in each of the six models. Consistent with other research indicating that the Seventeenth Amendment created electoral incentives for senators to hold public hearings that sought to investigate alleged misconduct in the executive branch (Lowande and Peck 2017), our results suggest that senators perceived electoral incentives to speak more fre-

¹¹See Table A.6.

quently in floor proceedings. Speeches were often published in local newspapers, and thus these speaking opportunities may have enabled senators to demonstrate to their constituents that they were addressing issues of local concern. Moreover, we find these patterns both within constituencies and within legislators, indicating that not only did voters select senators who exhibited more public-oriented behavior in the Senate, but also that senators who served both before and after the Seventeenth Amendment was ratified increased their participation in floor debates. Interestingly, our results contrast with related work by Fu and Howell (Forthcoming), who study the effect of the filibuster on floor debates and find that the adoption of Rule 22 in 1917 had no effect on the volume of floor speech. Given our results, it may be that electoral concerns and the incentives to develop public reputations, rather than institutional rules, are paramount in structuring legislators' participation in floor debates.

Table 5: The Seventeenth Amendment and Legislative Speech

	<i>Dependent variable:</i> log(No. speeches \geq 200 words)					
	(1)	(2)	(3)	(4)	(5)	(6)
Direct election	0.477* (0.113)	0.533* (0.099)	0.537* (0.103)	0.669* (0.136)	0.495* (0.140)	0.492* (0.133)
Congress fixed effects		X	X		X	X
Constituency fixed effects	X	X	X			
Member fixed effects				X	X	X
House comparison		NMPV	All		NMPV	All
Observations	2,530	11,662	12,876	2,530	11,662	12,876

Note: Entries are linear regression coefficients with standard errors clustered on states in parentheses. The dependent variable is the logged value of the number of floor speeches with at least 200 words in a given congress. Congress, member, and constituency fixed effects are included where indicated. Constituency fixed effects are based on state for Senators and at-large representatives and redistricting-specific House districts for all other representatives. "House comparison" indicates whether the comparison group included House members with non-missing values for district presidential vote share (NMPV) that were included in Table 1 for all House members. * $p < 0.05$ (two-tailed tests).

Conclusion

Ratified in 1913, the Seventeenth Amendment has been described as “one of the cardinal achievements of the Progressive era” (Buenker 1969, 306). By shifting senators’ constituencies from state legislatures to voters, proponents argued that the Senate would become less corrupt, less elite, and more representative. Our analysis examines the latter part of this claim by studying how senator behavior changed in response to this reform. Investigating a host of measures related to the quality of representation provided by senators to their constituencies, we provide new evidence that the Seventeenth Amendment produced meaningful change in some—though not all—of the behaviors exhibited by members of the Senate.

Our findings have implications for considering the representational consequences of the Seventeenth Amendment. Our findings suggest that while the Seventeenth Amendment increased the incentives for legislators to engage more frequently in certain kinds of constituency-oriented behaviors, it did not meaningfully improve the quality of representation provided to states by their representatives in the Senate. One possibility, which is well-documented by Schiller and Stewart (2015), is that Senators who served prior to the Seventeenth Amendment were already responsive to the policy views of their constituents. It is possible, then, that the Seventeenth Amendment further reduced incentives for Senators to demonstrate party loyalty but increased their efforts to secure publicity for their efforts in the chamber. Another possibility, which is not mutually exclusive from the first, is that the potential consequences of the Seventeenth Amendment were pre-empted by the direct primary. In the decade prior to the ratification of the Seventeenth Amendment, most of the country had initiated Senate primaries, whereby voters expressed their views about whom should serve and state legislatures (sometimes) agreed to abide the public’s preferences. To the extent this development increased the relevance of public opinion for senatorial elections, it may have limited the potential impact of the Seventeenth Amendment.

Our results also have implications for evaluating the capacity of elections to facilitate popular control over officeholders. Across most of our analyses, we found that the Seventeenth

Amendment had the greatest effect on legislative behavior through replacement; that is, when newly-empowered state voters replaced a senator who had previously been chosen by the state legislature with a senator directly elected by the people. This finding suggests voters' ability to select officeholders who work to maintain their support, but it also suggests the limitations of electoral threat as a mechanism that leads incumbents to modify their behavior.

Finally, our analysis has several important limitations which provide opportunities for future scholarship. First, as we noted above, it is possible that the potential effects of the Seventeenth Amendment were pre-empted by state-level direct primaries. Researching this possibility will provide greater context for interpreting the effects (and noneffects) of the Seventeenth Amendment. Second, our analysis of responsiveness to constituency preferences relies upon highly aggregated and noisy measures of both constituency preferences and senator behavior. Hirano and Snyder (2019) find strong responsiveness among congressional candidates on the issue of Prohibition during this period, and could suggest that an analysis focusing on high-profile and salient issues may provide greater evidence of improved representation following the Seventeenth Amendment. Likewise, while our analysis studied responsiveness, we did not evaluate the degree to which senators cast ballots that were congruent with public opinion. If senators already were casting roll call votes that were aligned with the policy views of their constituents, then we may not expect an increase in responsiveness. Third, it is possible that the effects of the Seventeenth Amendment grew over time. Schiller and Stewart (2015) make this argument in the context of evaluating its effects on the partisan composition of the Senate. Our analysis focused on the immediate implications of the Amendment for legislative behavior; while a longer-run analysis may be more challenging to convincingly execute, it could also reveal that the Amendment had a more substantial effect than we have documented.

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ONLINE APPENDIX

Robustness Checks and Supplementary Analyses for “Direct Election and Senate Representation”

Contents

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A.1 Senate Bill Sponsorship Data

A.1.1 Data Source and Coverage

To compile data on senators' bill sponsorship activity, we use information from ProQuest Congressional's bill profiles. These profiles provide information on the sponsor of the bill, the summary of the bill, and the bill's progress through the legislative process. Based on these profiles, we construct measures of bill sponsorship activity for each U.S. Senator from the 46th through the 71st Congresses. These measures include the count of bills sponsored, the count of private bills sponsored, and the count of private pension bills sponsored in each Congress. ProQuest Congressional provides coverage of the vast majority of bills introduced in the Senate during this period.¹² To assess coverage, we take the number of bills available in ProQuest Congressional for which we are able to obtain information on the sponsor and a bill summary, and we compare this quantity to the total number of bills listed in the *History of Bills* from within the *Congressional Record Index*.¹³ As is indicated in Table A.1, while the coverage rate varies somewhat from Congress to Congress, overall, we have information on sponsor and bill summary for 91 percent

¹²ProQuest Congressional's coverage of other types of legislation (i.e., joint resolutions, concurrent resolutions, simple resolutions) is not nearly as comprehensive, especially in earlier Congresses. Overall during the period under examination, ProQuest Congressional has coverage of about 59 percent of joint resolutions, 21 percent of concurrent resolutions, and 63 percent of simple resolutions. Fortunately, bills are by far the most common type of legislation introduced during this period, as they constitute about 93 percent of all Senate legislation. In addition to concerns about data coverage, our focus is largely on private legislation, which is most commonly legislated through the vehicle of a bill rather than a joint, concurrent, or simple resolution.

¹³Because bills are numbered sequentially, we simply take the maximum value of the bill number from the *History of Bills* for each Congress to determine the total number of bills introduced.

of all bills introduced in the Senate during this period.¹⁴

Table A.1: Data Coverage of Bills by Congress

Congress	ProQuest	<i>History of Bills</i>	Coverage Rate
46	1,919	2,224	86.3%
47	2,286	2,509	91.1%
48	2,428	2,671	90.9%
49	2,874	3,358	85.6%
50	3,664	4,000	91.6%
51	4,738	5,130	92.4%
52	3,405	3,895	87.4%
53	2,515	2,809	89.5%
54	3,438	3,736	92.0%
55	4,727	5,594	84.5%
56	5,766	6,070	95.0%
57	6,956	7,447	93.4%
58	6,000	7,295	82.2%
59	8,228	8,627	95.4%
60	9,071	9,541	95.1%
61	10,316	10,906	94.6%
62	6,984	8,589	81.3%
63	6,795	7,751	87.7%
64	7,766	8,334	93.2%
65	5,596	5,680	98.5%
66	4,968	5,052	98.3%
67	4,043	4,658	86.8%
68	3,795	4,410	86.1%
69	5,500	5,835	94.3%
70	5,273	5,903	89.3%
71	5,655	6,279	90.1%
Total	134,706	148,303	90.8%

¹⁴To decompose the sources of non-coverage, 3 percent of all Senate bills during this period are not contained in ProQuest Congressional and 6 percent of bills are in ProQuest Congressional but are missing the sponsor and/or bill summary in their bill profiles.

A.1.2 Typology of Private Bills

In the paper, we examine whether the direct election of senators tightened connections between senators and their constituents. During this time period, legislators often responded to constituent concerns and demands by introducing private legislation. We consider a bill to be a *private bill* if it targets assistance or offers relief to an individual, a very small group of connected individuals, a single company or organization (e.g., church), an estate, or another similar entity. This definition of a private bill is somewhat narrower than other definitions, which sometimes include legislation that targets broader groups or classes of individuals or include funds directed to an individual or company for community projects like bridges that could alternatively be classified as pork. Examples of broader groups would, for instance, include classes of veterans, tribes, or geographic areas like cities, counties, or states.¹⁵ Of note, our measure is focused on relief and assistance and, thus, excludes bills that are commemorative in nature. Some examples of commemorative bills are legislation providing for the erection of a statue, an award to honor an individual, or permission for a person to accept an award from a foreign government.

We classify bills into three categories: private pension bills, private military bills, and private relief bills. The category of non-private bills is then formed from the complement of the three above categories. Private pension bills are bills that add an individual to the pension rolls or increase the amount of a person's pension already on the rolls. A couple of examples of private pension bills include:

53 S. 2087 – To pension Julia M. Fuller, widow of Bennett C. Fuller late of Company G., Fifty-ninth Regiment Indiana Volunteer Infantry.

¹⁵A couple of specific examples of these kinds of bills targeting broader groups include:

52 S. 906 – “To increase the pensions of those who have lost eyes, limbs, or the use of them, or have additional disabilities.”

66 S. 3667 – “To appropriate \$4,000,000 for loans to farmers in drought-stricken sections of the United States for the purchase of seed for 1920 spring planting.”

67 S. 3303 – Granting an increase of pension to Albert Young.

The first bill calls for the individual to be placed on the pension rolls and receive \$12 per month, while the second bill calls for increasing the individual’s pension to \$50 per month. Private pension bills are extremely common during this period. Based on our classification, there were nearly 60,000 private pension bills introduced in the U.S. Senate during this time period, which is about two in five of all Senate bills during the period.

The second category of private bills that we identify are private military bills. These bills typically would modify a person’s military record to indicate an honorable discharge. To be eligible for a pension, individuals needed to be honorably discharged at the end of their service. Thus, the intended purpose of this category of legislation was generally to allow an individual to gain eligibility for a pension. A few examples of private military bills include:

51 S. 4729 – Granting honorable discharge to Orrin M. Cheney.

54 S. 2853 – To remove the charge of desertion from the name of William Weigel.

65 S. 1927 – To place Maj. Deane Monahan on the retired list of the Army with the rank of brigadier general.

68 S. 325 – To correct the military record of William McCormick.

While considerably less common than private pension bills, based on our classification, they still constituted about 3 percent of all Senate bills introduced during this period.

The third category of private bills that we identify are those related to private relief. The private relief category is a somewhat broader category and captures various forms of relief or assistance. While these bills often begin with the phrase, “For the relief of...,” it is, to an important extent, a catchall category that includes a variety of kinds of legislation designed to assist an individual or narrow group that does not fall into the above two categories. A few examples of private relief bills from this period include:

54 S. 1031 – For the relief of Jane Latham Donnelly, only surviving daughter and heir of Amos Latham, a soldier of the Revolutionary War.

55 S. 1927 – For the benefit of Mrs. Nannie D. McGlintock, of Harrison County, Ky.

61 S. 3677 – For the relief of heirs or estate of Elizabeth McClure, deceased.

63 S. 6638 – To pay the claim of the American Towing & Lightering Co. for damages to its tug *Buccaneer*.

71 S. 10 – To extend the benefits of the employees' compensation act of September 7, 1916, to Leon H. Hawley.

Based on our classification, there were over 34,000 private relief bills introduced in the Senate from the 46th through 71st Congresses, which represents about one in four Senate bills introduced during this period.

A.1.3 Classification Method

We use a keyword-based search method to classify bills based on their summary. To initially construct the keyword dictionary, we identified common phrases included in the bill summaries for each category of private bill. We then implemented an iterative process in which we randomly sampled bills classified in each of the three categories as well as bills not classified in any of the three categories, refined the dictionary based on the errors we identified, and then repeated this process dozens of times until the remaining errors seemed mostly of an idiosyncratic nature.

For private pension bills, the keyword dictionary includes various common phrases (and dozens of slight variants of these phrases) that were used to indicate a private pension bill such as:¹⁶

¹⁶The full dictionary includes over 50 different phrases or variants of phrases and allows for fuzzy matching of some of the most common phrases.

“granting” AND “pension”	“restoring a pension to”
“to grant” AND “pension”	“correcting the pension granted to”
“placing the name of” AND “pension”	“providing a pension for”
“restoring the name of” AND “pension”	“to place on the pension rolls”

We also construct a dictionary that ensures that our classification method does not capture pension bills targeting broader classes of veterans. If a bill’s summary contains a phrase (or variant) that indicates the target of the bill is a broader class, the bill is *not* classified as a private bill. Some examples of the phrases in this dictionary to refine our classification include:

“soldiers and sailors”	“certain”	“limbs”
“keeper and members”	“all persons”	“wounds”
“prisoners of war”	“officers”	“arm or leg”

For private military bills, we again implement a keyword-based search method using various phrases (and slight variants) used to indicate a private military bill such as:

“the military record of”	“authorizing” AND (“retired list” OR “retired-list” OR “list of retire”)
“honorable discharge to”	“to appoint” AND (“navy” OR “army” OR “marine”)
“charge of desertion”	“grant” AND “honorable”
“to place” AND (“retired list” OR “retired-list” OR “list of retire”)	“to correct” AND “rank”

To avoid classifying bills targeting a broader class as private military bills, any bill summaries that contain one of several phrases indicating a broader target are not coded as private military bills. Some examples of such phrases include:

“enlisted men”	“promotion officers”	“disabled officers”
“to officers”	“certain”	“veterans of”
“discharged officers”	“surviving officers”	“persons”

Finally, for private relief bills, we construct a keyword dictionary with various phrases and variants indicating a private relief bill. Some examples from this dictionary include:

“for the relief of”	“to reimburse”
“for relief of”	“to pay”
“the claim of”	“for the benefit of”
“to carry out the finding” AND “court of claims”	“to compensate”

These phrases are also often indicative of many non-private forms of relief. As a result, we construct a lengthy dictionary to attempt to avoid incorrectly classifying these non-private bills as private relief bills. The examples listed below represent a small sampling of the entire list of phrases:

“for the relief of” AND (“the city” OR “the state” OR “settlers” OR “discharged soldiers” OR “sailors” OR “the citizens”)		
“certain officers”	“laborers”	“of claimaints”
“certain enlisted”	“and other persons”	“applicants”
“certain other”	“of settlers”	“to reimburse the gov”
“relief of all”	“of purchasers”	“to reimburse the state”
“workmen”	“of volunteer”	“to reimburse the county”

A.1.4 Validation of Classification Method

This keyword-based search method allowed us to identify bills within each of the three categories of private bills and then filter out bills that had been erroneously classified into these categories. To validate this process, we randomly sampled 500 bills classified as *private pension*, 500 bills classified as *private military*, 500 bills classified as *private relief*, and 500 bills classified into none of those three categories (which we refer to as *non-private*). We then manually inspected the bill summaries of each of the 2,000 randomly sampled bills. In cases in which the bill

summary was ambiguous, we retrieved the full bill text and made a manual classification decision based on a reading of the full text.

Table A.2: Error Rates Based on Validation from Manual Inspection

Dictionary Classification	Error Rate
Private Pension	< 0.1%
Private Military	0.4%
Private Relief	1.2%
Non-Private	3.0%

Table A.2 displays the error rates for each sample of bills inspected. Among bills classified by our dictionary method as private pension bills, < 0.1% of these bills were classified differently from manual inspection. Among bills classified by our dictionary method as private military, 0.4% of these bills were classified into a different category based on manual inspection (all of these bills were manually categorized as non-private). Among bills classified by our dictionary method as private relief, 1.2% of these bills were classified into a different category based on manual inspection (again, all of these bills were manually classified as non-private). Finally, among bills classified by our dictionary method as non-private (i.e., the dictionary did not classify them into any of the three private bill categories), 3.0% of these bills were classified into a different category based on manual inspection (2.4% of these bills were manually classified as private relief, 0.8% were manually classified as private military, 0.2% were manually classified as private). Overall, these error rates are remarkably low.

Table A.3: Confusion Matrix Based on Manual Validation Exercise

		Manual Inspection Classification			
		Priv. Pension	Priv. Military	Priv. Relief	Non-Private
Dictionary Method Classification	Priv. Pension	500	0	0	0
	Priv. Military	0	498	0	2
	Priv. Relief	0	0	494	6
	Non-Private	1	4	12	483

Table A.3 displays the full results of this validation exercise. Weighting our full sample of $n = 2,000$ using inverse probability weights to account for the stratification of the sampling procedure, the manual inspection validation exercise indicates an overall accuracy of 98.6%.¹⁷ We also calculate the precision, recall, and specificity for each of the classes, again, accounting for the stratified sample with weights (see Table A.4).¹⁸ Overall, these metrics provide for a strong evaluation of the dictionary classification method.

Table A.4: Precision, Recall, and Specificity
Based on Manual Validation Exercise

Class	Precision	Recall	Specificity
Private Pension	100.0%	99.8%	100.0%
Private Military	99.6%	92.1%	> 99.9%
Private Relief	98.8%	96.8%	99.6%
Non-Private	96.6%	99.0%	98.4%

A.2 Robustness to Measurement and Estimation

¹⁷Accuracy indicates the percentage of all bills classified correctly.

¹⁸Precision indicates the ratio of true positives to the sum of true positives and false positives; recall indicates the ratio of true positives to the sum of true positives and false negatives; and specificity indicates the ratio of true negatives to the sum of true negatives and false positives.

Table A.5: Responsiveness and the Seventeenth Amendment (substituting static NOMINATE scores)

	<i>Dependent variable:</i> NOMINATE roll call scores			
	(1)	(2)	(3)	(4)
Constituency Conservatism	0.641*	0.517*	0.513*	0.324*
	(0.118)	(0.090)	(0.087)	(0.055)
Constituency Conservatism x Direct Election	-0.240*	-0.324*	-0.296*	-0.234*
	(0.106)	(0.104)	(0.089)	(0.074)
Direct Election	0.133*		0.163*	0.052
	(0.059)		(0.049)	(0.054)
Congress fixed effects		X	X	X
Constituency fixed effects	X	X	X	X
House comparison	None	None	At-large	All
Observations	2,598	2,598	3,006	12,906

Note: Entries are linear regression coefficients with standard errors clustered on states in parentheses. The dependent variable is members' NOMINATE roll call score. Congress and constituency fixed effects are included where indicated. Constituency fixed effects are based on state for Senators and at-large representatives and redistricting-specific House districts for all other representatives. "House comparison" indicates whether the comparison group included any House members, House members from at-large districts only, or all House members. *p<0.05 (two-tailed tests).

Table A.6: The Seventeenth Amendment and Legislative Speech

	<i>Dependent variable:</i> log(1 + Total no. words)					
	(1)	(2)	(3)	(4)	(5)	(6)
Direct election	0.363* (0.111)	0.539* (0.114)	0.547* (0.116)	0.520* (0.167)	0.641* (0.162)	0.651* (0.159)
Congress fixed effects		X	X		X	X
Constituency fixed effects	X	X	X			
Member fixed effects				X	X	X
House comparison		NMPV	All		NMPV	All
Observations	2,530	11,662	12,876	2,530	11,662	12,876

Note: Entries are linear regression coefficients with standard errors clustered on states in parentheses. The dependent variable is logged value of the total number of words (plus one) spoken in floor speeches in a given congress. Congress, member, and constituency fixed effects are included where indicated. Constituency fixed effects are based on state for Senators and at-large representatives and redistricting-specific House districts for all other representatives. “House comparison” indicates whether the comparison group included House members with non-missing values for district presidential vote share (NMPV) that were included in Table 1 for all House members. *p<0.05 (two-tailed tests).