Agency Problems, the 17th Amendment, and Representation in the Senate

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A prominent change in American electoral institutions occurred when the 17th Amendment to the Constitution established direct election of U.S. Senators as of 1914. How did this change the political agency relationship between the mass electorate and U.S. Senators? We develop theoretical expectations about the representational effects of direct election by a relatively inexpert mass electorate and indirect election by a relatively expert political intermediary, based on principal-agent theory. The chief predictions are that the representative will be more responsive to the mass electorate under direct election, but will also have more discretion to pursue his or her own ends. We use the 17th Amendment as a quasi-experiment to test the predictions of the theory. Statistical models show strong support for both predictions. Moreover, the 17th Amendment is not associated with similar changes in the U.S. House of Representatives—as expected, since the amendment did not change House electoral institutions.

A cornerstone of theoretical justifications for representative government is that representatives faithfully translate popular will into policy decisions, and constituents are able to hold them accountable through elections if they do not. Even if voters are fully informed and rational, the blunt instrument of elections may not be up to this task, and if they are not, then any decoupling of citizens from policy decisions is inherently problematic (although perhaps necessary in a large democracy). Furthermore, placing extra intermediaries between citizens and ultimate policy choices linked by a chain of electoral connections would seem to make matters worse for citizens because every link in the chain is another possibility for a disconnect between voters and policy choices. However, this logic is complicated if electoral intermediaries hold better information than voters about selecting or monitoring elected officials. The intermediaries' information is useful for voters, although they may not leverage it exactly as voters would if they themselves had it.

The 17th Amendment to the U.S. Constitution provides a unique opportunity to study such institutional effects and trade-offs in representation. Enacted in 1913, the 17th Amendment changed the basis of election of U.S. Senators from the state legislature to a statewide popular vote. A simple principal-agent argument posits that the amendment made senators direct agents of their ultimate principals, state voters, rather than indirect agents directly accountable to political intermediaries in the state legislature. Put differently, the amendment replaced indirect control with direct accountability, of which the chief implication is that senators should be more responsive to the state electorate's preferences directly.

We believe this simple agency logic is partly right, but that it misses an important factor. That is, in terms of democratic accountability of senators to their state electorates, direct election involves a trade-off. While the 17th Amendment did create a direct agency relationship, it also eliminated both the informed selection and monitoring of U.S. Senators by relative political experts, state legislators. Therefore, U.S. Senators may have been held to a *better* postamendment standard in democratic terms, but not as *tightly* as they were held to their preamendment standard. Which arrangement is better normatively

depends on whether one wants a reliable shot that misses the bull's-eye, or an erratic shot that sometimes hits. Before this issue can even be broached, however, it is necessary to establish that our version of the agency theoretic account is empirically useful.

Our theoretical discussion has several implications that we test using scaled roll-call voting decisions of U.S. Senators in presidential election years from 1880 to 1940 as a measure of senator behavior, state-level Republican two-party presidential vote share as a proxy for state electoral "ideology" or policy preferences, and the Republican share of the joint convention in the state legislatures as a proxy for state legislative policy preferences. In particular, our argument implies that U.S. Senators should be responsive to state legislative preferences before the 17th Amendment, but less so afterward. It implies that—conditional on state legislative preferences—U.S. Senators should not have been especially responsive to state mass electorate preferences before the amendment, but should have been responsive afterward. Finally, our theory implies that the difference in roll-call records within a given state's Senate delegation should be greater, again conditional on state ideology, after the 17th Amendment than before it—because senators were no longer selected by and accountable to political experts. We find strong support for these hypotheses, with the effects being significant both statistically and substantively.

Our research design uses the 17th Amendment as a treatment in a quasi-experimental sense and compares observations without treatment (before the amendment) to those with it (after the amendment). However, other political developments contemporaneous with this treatment could in principle account for the results. Therefore, we also explore contemporaneous changes in the U.S. House of Representatives, a close comparison group not subject to the treatment we explore, but that is buffeted by the same confounding factors that could threaten the validity of our causal inferences. We find that the 17th Amendment is not associated with changes in the House similar to those we find in the Senate.

The rest of the article is organized as follows. We first begin by briefly developing the issue and reviewing related literature. We then lay out the theory behind our argument, followed by a discussion of the empirical measures and methods we use in a statistical test of several implications of our theory. Our empirical analysis of the 17th Amendment comes next, and then we further address the institutional mechanisms behind the effects we observe and apply our models to the House as an untreated comparison group. Finally, we conclude.

Electoral Institutions, Direct Election, and the 17th Amendment

A crucial plank in the normative defense of representative democracy is that elections help to translate popular preferences into public policy decisions. Given such factors as "rational ignorance" and information asymmetries faced by the mass electorate (Arnold 1993; Downs 1957), potential incoherence of mass electorate ideology (Campbell et al. 1960), and the development and biases of special interests in the United States (Baumgartner and Leech 1998), whether and how institutions cause this translation is not obvious.[1] Therefore, a number of scholars have offered theoretical and empirical analyses of electoral institutions in light of these troubling issues.

The questions amount to whether elections in real-world settings of limited choice, hidden actions by representatives, and hidden information about their goals and values offer a potent enough device for voters to (*i*) monitor and electorally sanction representatives who take undesired actions, and/or (*ii*) select representatives who want to take desired actions even when they are not induced to do so by monitoring and sanctions. If the answer to these questions is yes, then from a principal-agent standpoint voters can be said to "control" their elected representatives.[2] These questions gave rise to the principal-agent theoretical perspective on elections, and their consequences for representation and accountability. The pioneering contributions of Barro (1973) and Ferejohn (1986) focused on monitoring and electoral defeat as a sanction. Banks and Sundaram (1993, 1998) and Fearon (1999) have substantially enriched the theory by including the issue of selection as well as sanctioning. The crucial agent incentives in the theory of indirect and direct election that we develop can be generated by either the selection or sanctioning aspects of elections, and we will return to this dual role of electoral institutions below.

The American context offers several types of variation in electoral institutions that scholars have leveraged to

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[1]Of course, several important analyses have shown a link between constituency preferences or characteristics and the behavior of elected officials (e.g., Bartels 1991; Miller and Stokes 1963). However, these findings do not imply that the institution itself causes the link. To take an extreme and dim view of the incentives generated for elected officials, the same sort of link would appear even if elections had no incentive effects at all and representatives were selected at random from their constituencies. Thus, our purpose is not to establish a link between constituents and representatives, but to understand the institution's role in making it.

[2]We do not, therefore, use the word "control" to imply more than this, or any degree of domination of elected representatives by voters.

analyze their role in mediating political agency relationships. A common empirical strategy is to compare the policy decisions made by public officials, such as judges or regulators, in states or political units in which they are elected to units in which they are appointed. For example, Besley and Coate (2003) found that elected electricity regulators pursue policies one would suspect are more favorable to the public than appointed regulators do. Similarly, election of judges is associated with responsiveness of judges (as well as agendas in judicial elections), whereas appointment is associated with judicial independence (Hall 2001; Hanssen 1999).[3]

This literature neglects two features of our analysis. First, it considers only the responsiveness of elected agents to the populace or mass electorate, not the discretion of the agents with respect to their set of principals. But leaving selection and sanctioning to the mass electorate implies a different level of information and expertise in evaluation of elected agents (and potential replacements), thereby affecting their discretion as well as responsiveness. We think this is a crucial aspect of the normative evaluation of electoral institutions, and an important consideration for analysis. Second, this research typically assumes that no relevant factors affecting policy decisions in question, but excluded from the model, are correlated with the electoral institutions. Therefore, the potential for confounding events or changes to appear as a treatment effect of the electoral institution is great in these research designs. In contrast, we explore the potential for contemporaneous confounding events by comparing the Senate and the House of Representatives.

One prominent change in American electoral institutions is of course the 17th Amendment. However, it has elicited surprisingly few scholarly treatments.[4] This may be in large part due to the influence of Rogers (1926) and Riker (1955), who argued that the 17th Amendment as such was not a significant institutional change at all. These scholars contended that it was instead largely anticlimactic, as a majority of states[5] by 1912 had already passed direct-primary laws that served as de facto direct-election instruments.[6]

Even more rarely has the 17th Amendment been used to analyze representation or the effects of electoral insti-

tutions. Crook and Hibbing's (1997) work is a notable exception; they argued that direct election and the 17th Amendment had a politically significant effect on representation in the Senate. In a set of aggregate analyses, they found that, after the amendment, senators were more likely to have government experience, and Senate elections more closely mirrored both House and presidential elections (on the latter point, see also Engstrom and Kernell 2007). More recently, a small literature has emerged that examines the effects of the 17th Amendment on the behavior of senators. This research investigates whether the change in individual senators' underlying constituency—from the state legislature to the state-level populace—altered their behavior in office. And, in fact, a majority of such studies conclude that a significant behavioral change consistent with a broadening "electoral connection" occurred in the Senate (see Poole and Rosenthal 1997 on roll-call abstention rates; Lapinski 2003 on retention of committee assignments; Meinke 2008 on position taking; Bernhard and Sala 2006 on ideological moderation over the electoral cycle; and Patty 2008 on predictability of senator voting records; but Wawro and Schickler 2006 find no effect).

To our knowledge, ours is the first 17th Amendment study to explicitly connect senators with the geographic units they represent, a level of analysis that similar research on the House of Representatives has shown to be important for the study of representation (see, e.g., Bartels 1991; Canes-Wrone, Brady, and Cogan 2002; Miller and Stokes 1963). This "dyadic" approach offers important insights that are not captured by studies that analyze either the aggregate behavior of the Senate in relation to national public opinion or the behavior of individual senators facing common electoral pressures (but not specifically tied to their states' preferences). The geographic unit is the level at which electoral incentives operate, and where the greatest potential exists for elections to translate policy preferences in the electorate into policy decisions in government. These are the key concerns in our theoretical approach.

Agency Relationships in Representation: Theoretical Expectations

The relationship between an electorate and its representative is, at least in part, a principal-agent relationship. By this we mean that the electorate in representative government delegates decision-making responsibility to its

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[3]See Besley and Case (2003, 52–54) for a review of the related literature.

[4]Early treatments (Haynes 1906, 1938; Rogers 1926) tended to focus on the causes of the amendment rather than its consequences.

[5]Rogers (1926, 114) documented 29 such states. More recently, Lapinski (2003) identified 30.

[6]Lapinski (2003) and Schiller and Stewart (2007) recently developed the most direct evidence in contradiction to this argument.

agent, the representative. Since the electorate cannot observe all the beliefs and dispositions of every candidate for office, and cannot observe every decision (or its context or alternatives) made by every elected official, it faces the potential problems of (*i*) electing an agent who has ideological beliefs far from its own ("adverse selection"), and (*ii*) inducing its agent to make decisions it likes ("moral hazard").[7] The chief formal lever the electorate has to influence its agent's preferences and behavior is, of course, an election. Elections are useful both for selecting agents whose preferences are compatible with the electorate's (Banks and Sundaram 1993, 1998; Fearon 1999) and for inducing an agent with any given preferences to act in accordance with the preferences of the electorate (Banks and Sundaram 1998; Barro 1973; Ferejohn 1986). But they are also blunt instruments of accountability.

The 17th Amendment changed the agency relationship between a state electorate and its U.S. Senators. Prior to the amendment, under indirect election, the relationship was one of indirect or hierarchical agency—the principals selected an agent, who in turn selected another agent—with both delegated selection and monitoring of the second agent by the first. That is, state voters chose their agents, the state legislators, who in turn chose another set of agents, U.S. Senators, on behalf of themselves and the state electorate.

It must be noted that nineteenth-century state legislatures were not always thriving and active bodies. Turnover was often quite high. Assemblies met part-time. Nevertheless there are several reasons to expect them to be better positioned than the state mass electorate at one or both of the tasks of selection and monitoring of a political agent—and our conjecture is not that they were *good*, only that they were *better*. First, state legislators would generally have been more politically sophisticated and connected than the typical voter. This relative sophistication could arise in part because of a selection effect; it is conceivably part of the reason one enters politics in the first place. More concretely it could arise due to information sharing in social networks. Campbell (1980) notes that ethnic networks commonly played a role in election to state assemblies and that legislators tended to live in common quarters while the assembly was in session. Each of these factors facilitates rapid transfer of relevant information and would help even relatively short-term legislators assimilate the modest information needed to make informed judgments (Lupia and McCubbins 1998;

Popkin 1991). Campbell also notes (chap. 3) that state legislators, while often "short timers," had some local notability that would suggest both access to the requisite information to evaluate contenders for U.S. Senate, and the skills necessary to do so. Finally, while nineteenth-century state legislators did not typically spend many terms in the statehouse, the phenomenon of "political careerism" was much more common at this time than "legislative careerism" (Carson and Jenkins 2007; Kernell 2005). The state legislature was one (usually short) stop on a longer political career path from local to state to federal office and back again. Thus short-term service in the statehouse does not imply that state legislators were political novices or irregularly involved in politics.

In combination these factors likely gave state legislators an advantage over the mass electorate for (*i*) selection of new or replacement senators, on the basis of better knowledge of the ideology and values of hopefuls and candidates in their state's political scene, and (*ii*) monitoring sitting U.S. Senators, on the basis of a better grasp of what they had done, how they had voted, and positions they had espoused while in office.[8] These abilities—which we stress need only be relative for our argument, rather than absolute or finely honed—would arise naturally from immersion in political information networks and attention to politics simply as part of their (possibly part-time) jobs and daily business. As experts on politics, at least relative to the typical voter, they could keep a close eye on behavior in the Senate and select the "right" senators when they so desired. Note that this argument does not depend only on monitoring capabilities of preamendment state legislatures; both selection and monitoring support the same basic logic for our present purposes.

Second, and especially in case of a relatively inexpert state legislature, state legislators themselves may have been controlled or strongly influenced by state party or chamber leaders (Haynes 1938; but see Schiller and Stewart 2008). We contend that this essentially follows the same line of our argument. In each case one actor selects an agent to act on its behalf, then transfers that selection right to another actor—who is unlikely to be as informed about the pool of possible agents or their actions while in office. The observable implication in either case is that after the transfer of selection rights, the selected agent's preferences should become less strongly related to the initial selector's preferences, and more strongly related to

[7]The agency problem in representation has been part of American discussions on the issue since the founding of the republic. Representation is obviously a complex, multifaceted relationship; our theory only requires a focus on this facet. Fortunately, if our theory is wrong and this is not a crucial facet, it will fail empirically.

[8]The selection aspect of our argument, in particular, dovetails with Schiller and Stewart's (2007) finding that state legislators faced a nontrivial set of candidates to choose from, and their choice of U.S. senator was definitely not pledged before the balloting. This helped give state legislators scope to use their information about what candidates stood for in making their selection of U.S. Senators.

the subsequent selector's preferences. Moreover, the initial selector's informational advantage should allow for less variation of the agent around the selector's preferences before the transfer of selection rights.[9] In sum, these relative advantages in either selection or monitoring (or both) would allow the state legislature, or the subset of elites dominating the state legislature, to select and monitor its U.S. Senators relatively tightly in terms of its own standard of behavior.

Thus the state electorate was essentially forced, before the 17th Amendment, to delegate the selection and monitoring of U.S. Senators to the relative political experts in the state legislature. In terms of representation, the major problem with this arrangement is that the state legislature's (or controlling faction's) preferred standard of behavior need not be the mass electorate's preferred standard of behavior. Because elections are blunt instruments of selection and control, and mass elections typically have a small number of candidates, the state electorate must incur some "agency losses" relative to first-best, perfect control of the decisions of the state legislature. This is because opportunistic state legislators or party bosses can be expected to have substituted, to some extent, their own preferences for those of the state electorate in decision making. The existence of agency losses in state-level representation simply means that the electorate would not have made exactly the same decisions as the electorate's agents in the state legislature, had the electorate possessed the same resources and information as the state legislature.[10] This is perforce true about the selection of U.S. Senators by the state legislature.[11]

Putting these arguments together, viewing pre-17th Amendment U.S. Senators in terms of hierarchical agency with delegated selection and monitoring implies that they would hew relatively closely to a standard determined by the state legislature—either because of good selection from a field of candidates or effective monitoring—but

that this standard may *not* be the one chosen by the median in the mass electorate.

The 17th Amendment made the terms of the agency relationship quite different. Instead of indirect agency, the principal-agent relationship between voters and U.S. Senators was obviously more direct. Voters no longer had to rely on an imperfectly controlled intermediary to hold a further downstream agent to account for them. Instead, they themselves could select new U.S. Senators and try to induce desired behaviors from sitting senators with any given ideology, based on their own preferences. To the extent that voters were informed about the preferences of new Senate candidates or the behavior of sitting senators, they could hold them accountable just as well as state legislatures could—and hold them to a better standard (from their own point of view, and from a normative democratic point of view).

Of course, the information needed for that level of accountability was probably not as easily accessible by voters as by state legislators. Voters can use cues, opinion leaders, and heuristics to get a reasonable general idea of the position and actions of politicians (both prospective and sitting ones), but because of both "rational ignorance" and lack of practice they are probably not as precise in their estimations as politicians are about each other. That lack of information, or lack of context for the information that is available, attenuates control and reintroduces scope for agency losses through a different route. Whereas senators before the 17th Amendment might have been well selected according to the "wrong" standard, after the amendment they have been more loosely constrained to the "right" standard, due to weaker selection and monitoring.

To put it differently and somewhat crudely, consider a thought experiment with p as the percentage of variance in a senator's behavior explained by variation in state legislature preferences, before the 17th Amendment. After the 17th Amendment, part of p shifts to voters—some percentage of variance $q < p$ is explained, postamendment, by variation in mass electorate preferences. The restriction $q < p$ comes from the assumption that selection and monitoring are more effective when done by experts than by novices. But the other part, $p - q$, shifts to the individual senator herself, and is explained by variation in her own preferences, variation in the preferences of the reelection constituency she assembles (which may be different after the amendment for different senators from the same state), variation in the preferences of the political network she assembled "on the way up," her party leadership, etc. With agency losses between voters and state legislatures, U.S. Senator behavior can be more closely connected to the mass electorate's preferences in general,

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[9]Where this argument does fall apart is when the titular "agent" is in fact selecting the titular "principal" before the transfer of selection rights to the electorate, as has been argued about U.S. Senators and state legislators prior to the 17th Amendment (e.g., Riker 1955). If this were in fact the true version of events, our empirical analysis would find no significant support for our theory.

[10]A more familiar example helps make the point: compare to the case of party convention delegates, who are both more ideological and more informed than voters in general or even party voters. This creates a scope for agency loss in framing party platforms and selecting candidates whom voters have been unable to completely eliminate.

[11]Note, therefore, that we do not assume that the only or major difference between the mass electorate and state politicians is their level of information about U.S. Senators, though that is one important difference for our theory.

and yet more variable in general, after the amendment than before it. On the one hand, a state's U.S. Senate delegation should be on average more representative of the state's preferences, but on the other hand, its members should exhibit greater differences relative to each other.

This agency theoretic view of the state-senator relationship has two testable implications regarding the effects of the 17th Amendment on representation in the Senate.

H1 Responsiveness Effect: (*i*) A state's senators should be more responsive to the policy preferences of its mass electorate after the 17th Amendment than before it. (*ii*) A state's senators should be less responsive to the preferences of the state legislature after the 17th Amendment than before it. This hypothesis follows only from the change in political principals following direct election, not their sophistication in selection or monitoring.

H2 Increased Discretion Effect: A state's senators should exhibit greater differences in voting behavior from each other after the 17th Amendment than before it. If the electorate cannot select or monitor its agents in the U.S Senate as effectively as political experts can, the agents should be better able to pursue an agenda other than that of their immediate principal (weaker monitoring), and more likely to want to do so (weaker selection). This hypothesis depends on the relative sophistication arguments we made above.

Data and Empirical Methods

The hypotheses above imply that the 17th Amendment caused a specific, measurable change in both the behavior of individual senators, and the differences in behavior within a state's Senate delegation, as a function of policy preferences of state-level political principals. Exploring this relationship empirically requires measures of these variables. The unit of analysis for this measurement is a senator-state-year for individual senator behavior, and a state-year for differences within a state's Senate delegation.

We use DW-NOMINATE scores (Poole and Rosenthal 1997, 2001) for individual senators in presidential election years from 1880 to 1940 as a measure of individual senator behavior. This is our dependent variable for Hypothesis 1, the responsiveness effect. These scores are the output of a technique that scales the roll-call records of senators into a basic multidimensional policy space and

are explicitly designed to allow for dynamic comparisons of the ideology exhibited in roll-call behavior. We use only the scores from the first dimension of the recovered policy space, which captures a more enduring, left-right conflict present in every era of American politics (Poole and Rosenthal 1997). The scale of the resulting scores ranges from -1 to 1 (mean = 0.01, standard deviation = 0.41 in our sample period), with larger numbers implying a more conservative roll-call record (Democrats average about -0.33 over our sample period, Republicans about 0.34).[12]

The "within-delegation distance" for a state in a given year (in presidential election years from 1880 to 1940), tapping into discretion of senators, is the maximum difference[13] between the first-dimension DW-NOMINATE scores for any pair of members[14] in the state's Senate delegation for that year (mean = 0.25, standard deviation = 0.26). Its scale is 0 to 2. This is our dependent variable for Hypothesis 2, the discretion effect.[15]

To measure state-level ideology or policy preferences in the mass electorate, we use state-level Republican two-party presidential vote shares, a common technique in the literature (e.g., Carson 2005).[16] Specifically, we use the votes cast for the Republican presidential candidate in a state as a percentage of votes cast for the Republican and Democratic candidates in that state.[17] This measure

[12] All elected senators (or senators appointed but then elected) from a state in a year are included in each state-year pair.

[13] The mean pairwise difference in DW-NOMINATE scores in a delegation is very highly correlated (about 0.985) with the maximum difference in scores in a delegation, so results are not sensitive to using the maximum specifically.

[14] Typically there were only two members per delegation per year, but some states had three or even four senators in a session (due to replacement of senators who depart mid-session) who cast enough votes over their careers to have DW-NOMINATE scores. We omit observations that include appointed senators who are not subsequently elected, as these senators are ostensibly not subject to the accountability mechanisms we analyze.

[15] Poole and Rosenthal (1984) first identified the empirical leverage offered by within-delegation differences (in interest group ratings of senator roll-call records), in their study of increasing polarization in the Senate over time. Schiller (2000) developed the most comprehensive analysis of within-delegation differences in senator behavior, in her demonstration that senators typically represent different and often diverse interests from their common state.

[16] A similar technique is used to generate a measure of district-level ideology in studies of House members' behavior. See Erikson and Wright (2000) and Canes-Wrone, Brady, and Cogan (2002). Also see Brady (2006) for a persuasive argument for using presidential vote data in this way.

[17] Election returns from a given year are matched with roll calls of the senators serving in the Congress immediately subsequent to that election, i.e., seated in the year following the election.

has several important benefits for our approach.[18] Most importantly, Republican presidential vote share is both available and readily interpretable over the whole time period under consideration. The measure is also parsimonious without doing too much violence to the complexities of ideology that are not central to our approach. The Republican Party represented the "right" side of politics over the entire range of years, and with the exception of the South (which we address with dummy variables), both major parties were competitive in all states over this time period. Scaled from 0 to 1 (mean = 0.48, standard deviation = 0.18 over our sample period), larger values of state-level Republican two-party presidential vote share indicate a more right-leaning predisposition than smaller values.[19]

We take the partisan composition of a state legislature in each year as a measure of state legislative policy preferences or ideology in that year (Burnham 1984). Specifically, we use the Republican share of the "joint convention" in the state legislature in a given year (range 0–1, mean = 0.49, standard deviation = 0.31 over our sample period). This is the sum of Republican seats across (almost always) both chambers, divided by the total number of seats in both chambers.

We focus on the period from 1880 to 1940 because it encompasses the passage of the 17th Amendment and should be long enough to detect regularities but not so long that obvious "regime shifts" in American politics are reflected.[20] Before 1880, Southern Reconstruction dominated the national agenda and produced a deviation from normal politics. After 1940, political competition changes radically with the consolidation of the New Deal consensus, the politics of civil rights, and the rise of the Republican Party in the South. To avoid suspicion that observations from these periods taint the results, we leave them out of the dataset.

Following Canes-Wrone, Brady, and Cogan (2002), we pool observations from different years over time. This creates a time series of observations for a collection of cross-sectional units. A natural modeling approach for data with this structure is (feasible) Generalized Least Squares, assuming that errors may be correlated within a single panel over time and differ in conditional variances across panels at a given point in time.[21] This GLS estimator is consistent even if observations/error terms depart from independence and identical distribution in this way.[22] Additionally, as reported below, we replicate all our analyses using OLS with state fixed effects (unbiased for any such failures of i.i.d. observations) and Prais-Winsten regression with state fixed effects (or, where justified, a more efficient GLS estimator with state random effects).[23] We defer further comment on the assumption of pooling and other important robustness issues until after the presentation of results.

Our research design is to take the 17th Amendment as a treatment and use a before-and-after comparison—untreated observations compared to treated observations—to assess the treatment effects of direct

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[18]We also examine state Republican vote shares as deviations from national average Republican vote shares in each year. This helps to filter out the effects of unusually weak or strong candidates from the major parties: even when the Democrat (for example) is very weak, the relatively liberal states will have relatively liberal vote shares. This has little effect on the empirical results.

[19]Usage of state presidential votes is the reason why our data encompass presidential election years only. While other variables vary in nonelection years, statistical models would necessarily be misspecified (assuming our theory is correct) by excluding state presidential vote in nonelection years. It could be a useful robustness check to use multiple imputation methods to fill in this missing data and explore whether the results hold up. However, this is beyond the scope of what we can accomplish in this article given space limits, and we leave it for future research.

[20]We match year t state legislatures with year t U.S. Senate roll calls. If monitoring of U.S. Senators played a key role in their behavior, in addition to selection, then the year t state legislature is indeed a relevant principal and should be included in analysis of year t U.S. Senator roll calls. Nevertheless, the year t-1 state legislature may in fact have selected the year t U.S. Senator, so that this alternative matching is theoretically preferable. Thus we executed our empirical analysis with the lag of a state's joint convention share of Republicans as the explanatory variable. While the estimated coefficients change, the substantive findings do not. In particular, our findings in Tables 1 and 2 below continue to support the discretion and responsiveness effects. Neither of these effects is supported for the House of Representatives.

[21]A state's Senate delegation's behavior is probably not independent over time since its membership is durable and individual roll-call behavior is persistent. Moreover, in the congress that encompasses year t, the behavior of delegations from moderate states is clearly more variable than the behavior of delegations from extreme states, so heteroskedasticity is a potential issue as well.

[22]Our dependent variables have limited range, making censoring (a different failure of i.i.d.) a possible concern in principle. However, for individual DW-NOMINATE scores, only about 10% of the observations are within one standard deviation of the scale boundaries. For within-delegation differences, we use natural logs in our statistical models, which do have unlimited range (no observations have a within-delegation difference of exactly 0, at which the natural log is undefined). We also check our results (as mentioned in a subsequent footnote) in a GLM assuming a Γ distribution for within-delegation differences, which builds left-censoring into the model. All of these factors considered, censoring is a relatively minor issue and we neglect it for brevity.

[23]We also replicated our results using OLS with panel-corrected standard errors (Beck and Katz 1995). However, the contemporaneous correlation across units that helps this approach to outperform FGLS in comparative data is probably less of a factor in our study, so we do not present these results below. Regardless, in no case does OLS with PCSEs change our qualitative or principal statistical findings.

TABLE 1   **Regression Results: Senator DW-NOMINATE (First Dimension) Scores**

Explanatory Variable	FGLS	OLS, State FEs	Prais-Winsten, State FEs
State GOP pres. vote share	0.024	0.162	0.116
	(0.078)	(0.115)	(0.121)
Interact., state GOP pres. vote share and 17th Amendment	0.289***	0.596***	0.501***
	(0.108)	(0.162)	(0.169)
GOP joint convention share	0.564***	0.583***	0.384***
	(0.056)	(0.068)	(0.074)
Interact., GOP joint convention share and 17th Amendment	−0.481***	−0.634***	−0.551***
	(0.065)	(0.089)	(0.098)
17th Amendment indicator	−0.048	−0.143**	−0.017
	(.041)	(0.064)	(0.060)
South indicator	−0.349***	–	–
	(0.030)		
Time trend	0.017***	0.024***	–
	(0.003)	(0.004)	
Constant	0.290***	0.232***	−0.187***
	(0.049)	(0.053)	(0.037)
$N = 1221$	$\chi^2 = 1554.5$***	$F = 37.9$***	$F = 15.54$***
		Within $R^2 = 0.17$	Within $R^2 = 0.07$

Note: Unit of observation is a senator-state-year, 1880–1940. FGLS errors are assumed to be AR-1 within a state's time path and heteroskedastic across states in a given year. Standard errors are in parentheses.
*denotes significance at $\alpha = 0.10$, ** at 0.05, *** at 0.01.

election on an electoral agency relationship. We also take the House of Representatives as an untreated comparison group, which should not (according to our theory) be affected by the treatment. This helps to strengthen our assessment of the causal effects of the change to direct electoral institutions.

Responsiveness and Discretion of Elected Agents: Empirical Analysis

We analyze in turn the support for Hypotheses 1 and 2 in our evidence. This analysis will allow us to determine whether the theoretical expectations set out above provide a useful understanding of electoral institutions.

To test Hypothesis 1, we model senators' DW-NOMINATE scores as a function of Republican presidential vote share, Republican seat share in the joint convention, and their interactions with the 17th Amendment. We include a South indicator variable and time trend as controls,[24] and an indicator for the 17th Amendment

to ensure that the interaction terms do not erroneously pick up an intercept shift due to direct election. Table 1 presents results from FGLS estimation, as well as OLS regression and Prais-Winsten regression (assuming AR-1 errors within a state's time path) with state fixed effects.[25]

The unconditional effect of the 17th Amendment on senators' voting records is negative in each model, but uneven in magnitude and significant only in the OLS-FE model.[26] However, the crucial terms for Hypothesis 1 are the Republican two-party presidential vote share, the Republican seat share in the state legislature, and their interactions with the 17th Amendment. The un-interacted terms capture the effects of these covariates before the 17th Amendment. As the responsiveness effect implies, policy preferences in the state legislature had a large and significant effect on senator roll-call records under indirect election. A 30 percentage-point increase in

[24]The time trend is important because it separates autonomous temporal changes in senator voting records from the treatment effect of the 17th Amendment.

[25]Hausman tests reject the hypothesis that the more efficient random effects estimator is consistent at $\alpha = 0$ to four decimal places, both with and without AR-1 errors.

[26]Therefore our results offer only slight evidence for Republican bias before the 17th Amendment and are largely inconclusive for this debate (cf. King and Ellis 1996; Wirls 1999), which is not surprising given that our design is not aimed at this issue.

Republican joint convention seat share (about one standard deviation) increased first-dimension DW-NOMINATE scores by about 45% of a standard deviation. Yet policy preferences in the mass electorate, as expressed in Republican presidential vote share, had a small and insignificant effect on senators' voting behavior.

Senators' responsiveness to state-level political principals changed markedly after the move to direct election, in keeping with the responsiveness effect. This change is reflected in the interaction terms. First, senators' roll-call records became much more strongly related to Republican presidential vote shares in the state. This is reflected in the positive and highly significant effect of the interaction of Republican presidential vote share and the 17th Amendment. In total after the amendment, a 15 percentage-point increase in the state Republican presidential vote share (about one standard deviation) increased first-dimension DW-NOMINATE scores by about one-eighth of a standard deviation.

On the other hand, controlling for mass electorate preference, senators became substantially less responsive to the policy interests of the state legislature after the 17th Amendment. This is reflected in the negative and highly significant coefficient on the interaction of the amendment and Republican seat share in the state legislature.

The overall effect of Republican seat share in the state legislature on senator DW-NOMINATE scores was significantly closer to zero, when the state legislature was no longer explicitly a principal in the agency relationship. Indeed, the hypothesis that the total effect of state legislature preference was zero after the amendment—that the sum of the interacted and un-interacted coefficients for Republican seat share in the state legislature equals 0—cannot be rejected at nearly conventional significance levels in a likelihood ratio (χ^2) test (two-tailed p-value = 0.31).

This evidence is just what one would expect based on the responsiveness hypothesis in our principal-agent perspective. But responsiveness engendered by electoral institutions is only part of the accountability story. Also relevant is the discretion of agents to pursue agendas that depart systematically from those of either the ultimate electoral principals or their (former) intermediaries. This is the issue captured in Hypothesis 2, the discretion effect.

A graphical first cut at this hypothesis is presented in Figure 1. The figure displays the absolute difference between the first-dimension DW-NOMINATE scores for members of a state's Senate delegation in a given year, as a function of its GOP presidential vote share in that year. Aggregating over all years, the relationship is roughly

FIGURE 1   State Ideology and Within-Delegation Distance, Before and After the 17th Amendment

Graphs by amendment
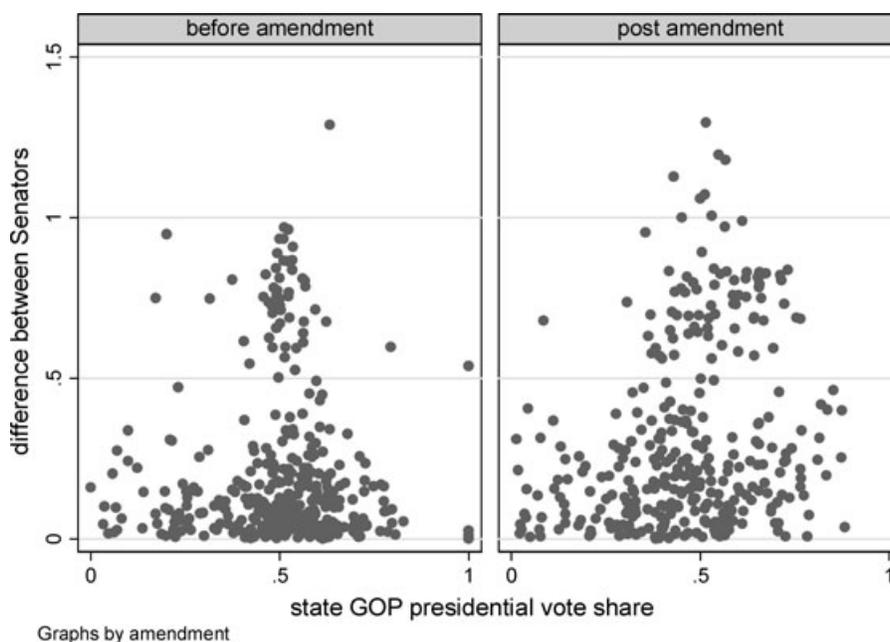
In both panels within-delegation distance (DW-NOMINATE first dimension) is roughly concave with peaks near 50% GOP vote share. Postamendment distances are less clustered around low values and more scattered on the vertical axis.

TABLE 2 **Regression Results: Senate Delegation DW-NOMINATE (First Dimension) Differences (log)**

Explanatory Variable	FGLS	OLS, State FEs	P-W, State FEs
State GOP pres. vote share	−1.259	−0.158	−0.166
	(1.247)	(1.646)	(1.732)
State GOP pres. vote share, squared	1.497	0.596	0.916
	(1.140)	(1.425)	(1.561)
GOP joint convention share	0.947	1.344	0.225
	(0.812)	(1.073)	(1.114)
GOP joint convention share, squared	−1.786**	−2.046**	−1.267
	(0.702)	(0.915)	(0.968)
17th Amendment indicator	0.549***	0.554***	0.583***
	(0.178)	(0.194)	(0.135)
South indicator	−0.496***	–	–
	(0.187)		
Time trend	0.012	0.014	–
	(0.020)	(0.022)	
Constant	−1.780***	−2.340***	−2.026***
	(0.390)	(0.493)	(0.321)
$N = 585$	$\chi^2 = 75.3$***	$F = 8.90$***	$F = 5.81$***
		Within $R^2 = 0.09$	Within $R^2 = 0.06$

Note: Unit of observation is a state-year, 1880–1940. FGLS errors are assumed to be AR-1 within a state's time path and heteroskedastic across states in a given year. Standard errors are in parentheses.
*denotes significance at $\alpha = 0.10$, ** at 0.05, *** at 0.01.

concave. Within-delegation distance is greater in moderate states than in extreme states. The key feature for Hypothesis 2 is that conditional on state ideology, the within-delegation distances appear larger on average after the 17th Amendment—the points are more dispersed in the vertical dimension and less tightly packed close to the horizontal axis. It would appear that senators post-amendment no longer behave so similarly as others facing much the same political environment.

The figure is suggestive but obscures many possible confounds. To determine if this difference in distances before and after the amendment is indeed significant, we model within-delegation distance statistically. Distances cannot be negative, and the distribution of distances shows strong right-skew. We use the natural log of within-delegation distance as the dependent variable.

Our baseline FGLS model specifies within-delegation distance as a function of mass electorate preference, state legislative preference, a 17th Amendment indicator variable, a South indicator, and a time trend.[27] For both state-level political principals, we include both the basic mea-

sures of their preferences and their squares. This is to allow for (but does not impose) a concave relationship between these preferences and within-delegation distance, as depicted in Figure 1. Results of the model are presented in Table 2.[28]

the moribund Republican Party in the South over much of this period likely also had a causal effect on party splitting. Therefore, including split-party status as a covariate in the regression models would cause systematic downward bias on the estimated effects of other covariates that cause party splitting: some of their effect would work through the party-splitting variable, therefore reducing their magnitude.

By far the simplest (this footnote notwithstanding) way to avoid this bias is to exclude party splitting as a covariate (i.e., to avoid controlling for an intermediate outcome). This exclusion allows consistent or unbiased (depending on the estimator) estimation of the treatment effects of interest. Another approach is to use a path model, a type of recursive system of equations estimable by a two-stage procedure, which in theory should produce the same estimated treatment effect as the basic model with exclusion. We estimated a path model by FGLS, with a first-stage Linear Probability Model to estimate the probability of a split-party delegation as a function of other covariates in the FGLS model for within-delegation distance. The results, which we omit for brevity, yield a treatment effect of the 17th Amendment similar in quantitative terms to those in Table 2 (changing the within-delegation distance by about a fourth of a standard deviation).

[27]Whether a Senate delegation is a unified-party or split-party delegation also affects the within-delegation distance (cf. Poole and Rosenthal 1984). However, as Brunell and Grofman (1998) argued, the 17th Amendment exerted a causal effect on the prevalence of split-party delegations, making them more common. Moreover,

[28]The table also presents results from OLS and Prais-Winsten (AR-1 errors within a state's time path) models with state fixed effects.

Before discussing the key results for our theory we briefly discuss the other estimates. For Republican joint convention share, but not Republican presidential vote share, the results reflect a generally concave relationship with within-delegation differences (and only the state-house composition variables are significant).[29] Specifically, the linear term is positive and the quadratic term is negative, which implies a concave shape for positive domain values. These results are fairly consistent across models in Table 2. Relatively extreme states tend to have relatively extreme U.S. Senators, but moderate states have U.S. Senators all over the ideological map. The negative coefficient on the South indicator reveals that southern delegations had significantly lower within-delegation differences overall (before and after the amendment) than all delegations. Ex ante it was not obvious that this result should occur; southern delegations leaned further left over this time period and tended to be of the same party, which could hold down differences, but effective one-party rule in the South could have fused quite distinct senators together in a unified Democratic delegation with a less than fully informative party label. Finally, the time trend, while slightly positive, is insignificant.

With respect to the increased discretion hypothesis, the 17th Amendment was associated with a strongly significant increase in within-delegation distance, all else constant. Direct election increased the natural log of within-delegation distance by about 40% of a standard deviation in the FGLS specification. For all error specifications the evidence is highly inconsistent with what one would expect if direct election had no effect (or a negative effect) on senatorial discretion, as measured by within-delegation distance.

Three other pieces of evidence corroborate our conclusion about the increase in discretion following the elimination of delegated selection and monitoring of senators. First, in our model of senator DW-NOMINATE scores for Hypothesis 1, the increased responsiveness of senator voting behavior to the mass electorate following the 17th Amendment is outweighed (in standardized as well as absolute terms) by the decrease in responsiveness to the state legislature. Second and relatedly, the R^2 in an

OLS model with state fixed effects for individual senator DW-NOMINATE scores, estimated on the sample period before the 17th Amendment (1880–1912), is higher than that from the period after (1916–1940). Third, in an independent analysis, Patty (2008) has demonstrated that predictability of DW-NOMINATE scores (measured by the generalized mean probability of correct classification of a senator's yea or nay vote on any given issue) declined after the 17th Amendment. All of these results mean that a collection of political principals one may use to explain senator voting behavior had less explanatory power after the formal institution of direct election. This is in line with the discretion effect.

Of course we cannot say whether senators are using this increased discretion to pursue their own ideological agendas (i.e., "shirking"), develop relationships with interest groups, assemble different reelection constituencies, etc.[30] We can only say that senators appear to be less constrained by factors in their state political scene after the 17th Amendment than they were before it, and this is the implication of eliminating delegated monitoring by political experts.

In short, the empirical support for Hypothesis 2, the discretion effect, also appears fairly robust. Taken together, the empirical arguments in this section strongly support the agency theoretic view of the institutional change in the 17th Amendment, and the accountability effects of direct election.

Probing the Mechanisms of Change

The results above reveal effects coincident with the 17th Amendment and consistent with our theory. However, they leave open to question the precise institutional mechanisms behind these effects, and indeed whether these mechanisms had a causal effect at all. We explore these issues in greater depth in this section and the next.

The responsiveness and discretion effects of the 17th Amendment could occur through several possible channels. Specifically, the changes observed may have occurred because the amendment made it easier for a state to send a split-party delegation to the Senate (Brunell and Grofman 1998), because of changes in behavior within parties, or some combination. For example, if change by

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Hausman tests strongly reject the hypothesis that the corresponding random effects estimators, while more efficient, are consistent. We also check for robustness using a Generalized Linear Model, specifying a Γ distribution (which is right skewed) for within-delegation distances and a natural log link function. Significance tests for the theoretically critical variables have similar results. We omit these results for brevity.

[29]It is possible that correlation between statehouse composition and state Republican vote share makes precise estimates of each factor difficult to obtain.

[30]Levitt's (1996) analysis would suggest all of the above, though catering to interest groups (which arguably would be a limitation on the discretion of senators, as they are constrained by another factor they may find useful for winning elections) may be unlikely before the midtwentieth century rise of the interest group system—a development that occurs after the end of our sample period (Baumgartner and Leech 1998).

established partisans is not possible, delegation splitting could simply be a means for an electorate to make its delegation, on average, more representative of its interests (Alesina and Rosenthal 1995), though it would simultaneously allow senators more leeway to differ from each other and the principal selecting them. Each of these channels is consistent with the theory, but with the basic results established it is interesting to see if one or the other channel can be ruled out.

To address this we estimate the FGLS responsiveness model from Table 1 separately for Republican and Democratic U.S. Senators, and the FGLS discretion model from Table 2 separately for unified Republican, Unified Democrat, and split-party Senate delegations. If all the results from the fourth section were due to a shift from unified to split-party delegations, these models would reveal no evidence supporting our theory, as they all condition on delegation composition. The results are contained in Tables 3 and 4.

TABLE 3   Regression (FGLS) Results: Senator DW-NOMINATE (First Dimension) Scores

Explanatory Variable	Republican	Democrat
State GOP pres. vote share	0.014 (0.054)	0.001 (0.089)
Interact., state GOP pres. vote share and 17th Amendment	0.759*** (0.094)	0.225** (0.106)
GOP joint convention share	0.108*** (0.036)	0.077 (0.052)
Interact., GOP joint convention share and 17th Amendment	−0.298*** (0.054)	0.164*** (0.059)
17th Amendment indicator	−0.286*** (.051)	−0.071* (0.040)
South indicator	0.128*** (0.036)	−0.075*** (0.018)
Time trend	0.006** (0.003)	0.027*** (0.002)
Constant	0.263*** (0.043)	−0.625*** (0.041)
	$N = 629$	$N = 558$
	$\chi^2 = 138.14$***	$\chi^2 = 506.58$***

Note: Unit of observation is a senator-state-year, 1880–1940. FGLS errors are assumed to be AR-1 within a state's time path and heteroskedastic across states in a given year. Standard errors are in parentheses.
*denotes significance at $\alpha = 0.10$, ** at 0.05, *** at 0.01.

Both within-party responsiveness models are consistent with three key implications of the theory. First, for neither Republicans nor Democrats is a senator's scaled roll call significantly related to state Republican presidential vote share. The p-values of 0.80 for Republicans and 0.99 for Democrats are large enough to suggest that Type II errors are not a serious concern here. Second, roll calls for both Republican and Democratic senators did become significantly more responsive to partisan leanings in the state electorate after the 17th Amendment. Two more implications hold for Republicans but not Democrats: roll calls for Republican senators became less responsive to statehouse composition after the 17th Amendment; and Republican senator roll calls are significantly related to statehouse composition before passage of the amendment. However, Democratic senators were not significantly responsive to statehouse composition prior to the 17th Amendment and became significantly responsive after it. It is not clear why this should be so; one possibility is that effective one-party rule in solidly Democratic states muted responsiveness before 1914, while better sorting of politicians into parties from 1914 to 1940 affected both state legislatures and Senate delegations simultaneously. These exceptions notwithstanding, analysis within parties shows support for our theory only somewhat weaker than the overall analysis. This suggests that two distinct channels, modification of behavior within parties and increasing likelihood of split-party delegations, each make a contribution to the observed effect.[31]

The within-party discretion models also reveal effects similar to the overall results. For unified Republican and split-party delegations, the 17th Amendment is associated with a positive, significant increase in within-delegation differences in scaled roll calls. For Democratic delegations, the amendment has an insignificant effect. However, this is at least partly because so many unified Democratic delegations from 1880 to 1940 were from the South (about 65%), where near one-party rule meant that common partisanship could mask great differences. For Democratic delegations outside the South, the effect of the amendment jumps from −0.04 to 0.48, though the latter estimate slightly misses conventional significance levels.[32]

[31]Note that the 17th Amendment indicator is negative and significant within each party. This suggests, distinct from Republican bias before the 17th Amendment (cf. footnote 26), a sort of "right-leaning bias" within each party. While an interesting possibility, this is beyond the scope of our article, and we defer further exploration for future research.

[32]Note, therefore, that including southern states creates a "hard test" for the discretion models because the party label is less informative about positions. We choose to retain southern observations in the models in general because it makes the analysis somewhat more circumspect.

TABLE 4   Regression (FGLS) Results: Senate Delegation DW-NOMINATE
(First Dimension) Differences

Explanatory Variable	Republican Delegations	Democratic (Non-Southern) Delegations	Split-party Delegations
State GOP pres. vote share	1.607	2.068	4.978***
	(2.278)	(7.207)	(1.746)
State GOP pres. vote share, squared	−0.214	−3.574	−4.772***
	(1.786)	(7.414)	(1.383)
GOP joint convention share	1.126	3.584**	−0.382
	(1.429)	(1.457)	(0.552)
GOP joint convention share, squared	−1.464	−3.319**	0.111
	(1.149)	(1.749)	(0.449)
17th Amendment indicator	0.347*	0.477	0.440***
	(0.194)	(0.368)	(0.140)
South indicator	−	−	−0.426**
			(0.178)
Time trend	−0.009	0.091***	−0.061***
	(0.025)	(0.032)	(0.014)
Constant	−3.299***	−4118**	−1.094***
	(0.628)	(1.639)	(0.521)
	$N = 238$	$N = 75$	$N = 129$
	$\chi^2 = 18.87^{***}$	$\chi^2 = 120.87^{***}$	$\chi^2 = 44.03^{***}$

Note: Unit of observation is a state-year, 1880–1940. FGLS errors are assumed to be AR-1 within a state's
time path and heteroskedastic across states in a given year. Standard errors are in parentheses.
*denotes significance at $\alpha = 0.10$, ** at 0.05, *** at 0.01.

To be sure, part of the effect of the 17th Amendment on within-delegation difference in Table 2 is due to shifts from unified-party to split-party delegations. However, as with responsiveness, the discretion results conditional on delegation type suggest that at least part of the effects uncovered in Table 2 are due to changes within each delegation type, and not entirely because of shifts among the delegation types.

These results also help to compare our theoretical explanation to an alternative account based on partisan gerrymanders in state legislative districts. In particular, these gerrymanders made state legislatures more homogenous than the state electorate in terms of partisanship, and much more likely to choose unified-party U.S. Senate delegations. The 17th Amendment can be understood as a shift in the median voter of a senator's electorate from the median of the state legislature to the median of the electorate. While gerrymandered state legislatures typically chose unified-party delegations, the state electorate did not have to (and indeed did so significantly less often than state legislatures; see Brunell and Grofman 1998). With state voters choosing split-party Senate delegations more often, the Senate delegation would be more

responsive to voters on average and would exhibit larger within-delegation differences after the amendment. This is a compelling argument based on the historical circumstances and at a broad level matches up with the evidence. However, we contend that our account is preferable for three reasons. First, it has more explicit foundations in agency theory, which clarify its logic in the abstract and also its linkage to other findings on electoral institutions. Second, by tying our Hypotheses 1 and 2 together in a single theory, our account also has broader empirical implications than this alternative, and therefore more grounds to be falsified. It is therefore all the more powerful because it is not in fact falsified in a battery of empirical tests. Third, this alternative account is unable to fully account for our findings, because it depends entirely on a shift toward split-party U.S. Senate delegations after the 17th Amendment. As Tables 3 and 4 show, the results are consistent with our account even conditional on delegation type.

Another important question about the institutional mechanism behind the results above deals with the date of the "treatment" whose effect we seek. Our theoretical perspective is based on direct election, not the 17th

Amendment per se. For us as for other scholars who have recently identified effects related to the 17th Amendment, the amendment is simply a convenient vehicle for analysis of our theoretical concern. As noted, several scholars (e.g., Riker 1955; Rogers 1926) have argued that the amendment was an anticlimactic codification of changes that were actually effected by prior electoral reforms at the state level, i.e., binding direct primaries.[33] Lapinski (2003) has authoritatively established dates of these reforms for each state.[34] We have replicated all of our substantive and statistical conclusions using a state's date of adoption of any sort of (binding) direct primary of senators as the date of its treatment,[35] rather than the nationwide ratification of the 17th Amendment. None of our conclusions, substantive or statistical, is altered by defining the treatment in this way. The main reason for this is that, of the states that adopted binding links between the mass electorate and the selection of senators, almost all of them did so after 1900, and most in 1907 or later. Thus, however the treatment date is defined, most pre- and posttreatment observations are identical. We discuss our main results with the 17th Amendment itself as the treatment, rather than states' primary-election adoption dates, because the amendment is a more focal (and legally binding) change and has the virtue of being an exogenous treatment for at least some states.[36]

[33]Of course, to the extent that the 17th Amendment only ratified reforms that individual states had already made before 1914, finding evidence of these implications should be difficult. Stated differently, if the amendment really amounted to nothing more than a codification, we would find a null effect of the amendment empirically.

[34]Lapinski (2003) also makes a persuasive case *against* these state-level direct primary laws truly mimicking direct election. The laws themselves could not be legally binding, Lapinski argues, as the direct agency relationship between state legislatures and U.S. Senators was spelled out in the U.S. Constitution. Moreover, the vast majority of states that passed direct-primary laws also were on the forefront of convention calls, i.e., petitions to Congress calling for a convention to alter the Constitution and include a direct-election amendment.

[35]Some states before the 17th Amendment had voluntary agreements by political parties to honor results of statewide popular vote in selection of U.S. Senators. We do not consider these to be as constraining as formal institutional changes and do not use them in defining the alternative treatment dates. In any case, only a few states took this approach before the 17th Amendment, and most only shortly before it.

[36]Another possible alternative treatment is the adoption of the Australian Ballot, which helped to wrest control of nominations for national political office generally (not just the Senate) from hard-core partisans and ideologues. When we use Australian Ballot adoption dates within states as a treatment date, the treatment has essentially no effect on responsiveness or discretion. This makes sense because Australian Ballot adoption tended to occur somewhat far in advance of direct election or the 17th Amendment. Our data

Beyond simply dating the treatment or parsing it out among parties and delegation types is a deeper issue of what the treatment actually is. Given the quasi-experimental nature of the research design, it is conceivable in principle that other institutional changes in the U.S. Senate at this time are simply reflected in our results as "effects" of the 17th Amendment. Three conspicuous cases of major institutional change in the Senate are cloture reform, the rise of formal Senate leadership, and the further institutionalization of the committee system. Nevertheless it is not clear how these factors could individually or jointly produce the combination of results we observe. Cloture reform might have affected the blocking power of minority coalitions and conceivably therefore the agenda over which roll calls are taken. However, we are not aware of any compelling reason why it would affect responsiveness and discretion consistent with our empirical findings above, and recent theoretical treatments (Wawro and Schickler 2006) have not offered any. The rise of the Senate party leadership in the first decades of the twentieth century (Campbell, Cox, and McCubbins 2002; Gamm and Smith 2004) is another important change. This might have, if anything, constrained member behavior as leaders coordinated or even compelled votes (or restricted the agenda to issues on which the party was cohesive), thereby holding together the behavior of otherwise different senators from a given state. Such an effect would work against the discretion results we observe; again, it is not clear what effect if any this change should have had on responsiveness.[37] Finally, Senate committees achieved new institutionalization over the time period we study. With any degree of self-selection it seems likely that committees could expand observed differences in specialization of senators and possibly roll calls. At the same time, because of the same self-selection it is not clear that committees would cause such differences rather than reflect ones that already exist. Moreover, as with the other changes, we cannot identify any natural reason why responsiveness should have been affected by committee development. Overall, then, while other institutional developments occurred in the Senate contemporaneous with the 17th Amendment, they do not

essentially allow a test of the conjecture that the "real" treatment is the Australian Ballot; if this were the case null results for the Australian Ballot and nonnull results for the 17th Amendment are the opposite of the expected pattern. Moreover, the Australian Ballot is not a Senate-specific change, but presumably affected the House of Representatives too; it is at least in part addressed by the "placebo test" we execute for the House in the sixth section below.

[37]Of course even now it is far from clear that Senate party leaders have the means to constrain senator behavior; moreover, as Gamm and Smith (2004) note, Senate party leadership did not fully solidify until the 1930s.

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appear immediately credible as alternatives to the institutional mechanism of direct election in accounting for the empirical patterns we identify.

The institutionalization of party leadership and committees is actually not unique to the Senate in the time period we study. These institutional changes and many others—to name a few, the rise of presidential primaries, the resurgence of the Democratic Party in presidential politics, changing alignments of social and economic coalitions with the two major parties, the Progressive Era, the Australian Ballot, and the 19th Amendment—occurred in the larger political narrative of this era as a whole. These developments all occurred at or near the adoption of the 17th Amendment. If some such factor or combination of them were actually driving the empirical results above, we might inadvertently attribute its effects to the 17th Amendment and incorrectly consider the theory in this article to be a useful one for explaining the effects of the institutional change to direct election.

Importantly, these and many other contemporaneous, potentially confounding political developments are not Senate specific. They should also affect the U.S. House of Representatives, which is therefore a useful comparison case: it was not directly affected by the treatment we explore, but was affected by potentially confounding events.

Does the 17th Amendment "Explain" Changes in the House of Representatives?

If the move to direct election caused the changes in Senate representation we identified above, then the 17th Amendment should not be associated with similar changes in the House. Conversely, if the amendment "explains" changes in the House as well as the Senate, the empirical support for our theory would be weaker.

We applied the same statistical models employed above to the U.S. House over the same time period. We find that the 17th Amendment is not associated with changes in House roll-call records similar to those that we found for the Senate. This is as expected assuming that the amendment, rather than a contemporaneous confounding factor, caused the changes we identified above for the Senate. This strongly suggests that our results for the Senate are not merely picking up some other factor unrelated to the change in the political agency relationship.

First consider Hypothesis 1, the responsiveness effect in the House. While House members should be responsive to several state interests, they should not be more or less so directly because of the 17th Amendment. We take each

state-year as the unit of analysis, for presidential election years from 1880 to 1940. The dependent variable is the average first-dimension DW-NOMINATE score for all members of a state's U.S. House delegation (mean = 0.10, standard deviation = 0.35).[38]

The two crucial features of the responsiveness effect work out as follows. First, since House members have always been chosen by direct election, their average DW-NOMINATE score should be positively related to the state's Republican share of the two-party presidential vote. However, our theory predicts that that relationship should not be any different after the passage of the 17th Amendment than before it. Second, the state U.S. House delegation's average DW-NOMINATE score should also be positively related to the Republican share of seats in the state legislature. This is because of control over congressional district lines and the possibility of partisan gerrymanders (Engstrom 2006): as one party increased its dominance in the state legislature, it had more control over districts and better ability to see its ideals carried into Congress. However, this effect did not systematically change with the 17th Amendment (especially controlling for a time trend), so the relationship between state legislature composition and House average DW-NOMINATE score should not differ before and after the 17th Amendment.

The results in Table 5 from feasible GLS regression are consistent with these expectations. U.S. House delegations are responsive in their roll-call voting to the Democratic/Republican leanings of the state electorate. But the interaction of Republican presidential vote share and the 17th Amendment indicator shows that they are no more or less responsive after the amendment. Similarly, U.S. House delegations are responsive to the partisan composition of the state legislature—but again no more or less so after the 17th Amendment.[39]

Bearing on Hypothesis 2, the discretion effect, we also find that the 17th Amendment is not associated with systematic changes in the within-delegation distance in

[38]Paucity of reliable congressional district level on election returns, or a natural benchmark comparison in the state legislature, precludes the use of congressional districts as the units of observation.

[39]These results are very similar to those obtained from pooled OLS with state fixed effects and pooled Prais-Winsten regression with state fixed effects and AR-1 errors within panels. (In both of these cases Hausman tests reject consistency of the random effects estimator.) The fixed-effects results are also in Table 5. One anomaly is that the 17th Amendment indicator variable has a marginally significant positive effect on the House delegation average in the autoregressive fixed-effects regression. However, based on the other models it appears to be an isolated occurrence and not surprising given the great chance of a false positive occurring at least once in this article. In any case, the main effect of the 17th Amendment is not a key implication of the responsiveness hypothesis.

TABLE 5   Regression Results: House Delegation DW-NOMINATE (First Dimension) Average

Explanatory Variable	FGLS	OLS, State FEs	Prais-Winsten, State FEs
State GOP pres. vote share	0.146*	0.301***	0.190*
	(0.086)	(0.099)	(0.111)
Interact., state GOP pres. vote share and 17th Amendment	−0.060	0.039	0.061
	(0.118)	(0.144)	(0.154)
GOP joint convention share	0.723***	0.554***	0.508***
	(0.052)	(0.060)	(0.068)
Interact., GOP joint convention share and 17th Amendment	−0.091	−0.155	−0.075
	(0.066)	(0.181)	(0.088)
17th Amendment indicator	0.044	0.115	0.095*
	(0.045)	(0.158)	(0.054)
South indicator	−0.199***	–	–
	(0.025)		
Time trend	0.015***	0.014***	–
	(0.003)	(0.003)	
Constant	0.427***	0.470***	−0.268***
	(0.047)	(0.054)	(0.035)
$N = 613$	$\chi^2 = 2282.4^{***}$	$F = 49.1^{***}$	$F = 35.4^{***}$
		Within $R^2 = 0.34$	Within $R^2 = 0.25$

Note: Unit of observation is a state-year, 1880–1940. FGLS errors are assumed to be AR-1 within a state's time path and heteroskedastic across states in a given year. Standard errors are in parentheses.
*denotes significance at $\alpha = 0.10$, ** at 0.05, *** at 0.01.

the U.S. House. Here again we take state-years as the unit of analysis, in presidential election years from 1880 to 1940. As the dependent variable, we take the interquartile range of first-dimension DW-NOMINATE scores for the state's U.S. House delegation in a given year (mean = 0.19, standard deviation = 0.23).[40] Since the 17th Amendment should not have affected discretion of House members with respect to their principals in the state electorate, our theory predicts no effect of the 17th Amendment on within-delegation distance. The FGLS results in Table 6 below show that the evidence is consistent with this prediction. The p-value on the 17th Amendment indicator is too large (two-tailed p = 0.93) to make this assumption of no effect appear suspect.[41]

These findings show that the 17th Amendment is not associated with changes in responsiveness or discretion of House members. This is not the pattern one would expect if the observed changes in the Senate were actually the result of broader political forces rather than the institution of direct election.

Conclusion

Our theory of the agency relationship between the mass electorate and U.S. Senators before and after the 17th Amendment implies that direct election had both a benefit and a cost. The amendment clearly made senators responsive directly to state electorates, so their selection and accountability once in office were based on a democratically stronger standard. At the same time, the amendment made senators answerable to relative political novices, so they could not be held to that standard (whether through electoral sanctioning or selection) as tightly as they were

[40]Since most states have many more than two congressional districts, using the maximum distance as in the Senate case would create a measure badly skewed by outliers in many state-years. The interquartile range is less sensitive to such issues, as it is based on order statistics.

[41]These results are again identical in qualitative and statistical significance terms to those obtained from OLS regression with state fixed effects and Prais-Winsten regression with state random effects and AR-1 errors within panels (also in Table 6). In the latter case,

a Hausman test does not reject the hypothesis that the random effects estimator is consistent. Since the RE estimator is necessarily more efficient than the FE, we prefer it in this case.

TABLE 6   **Regression Results: House Delegation DW-NOMINATE (First Dimension) Interquartile Range**

Explanatory Variable	FGLS	OLS, State FEs	P-W, State REs
State GOP pres. vote share	0.078	0.146	0.182
	(0.102)	(0.226)	(0.222)
State GOP pres. vote share, squared	−0.123	−0.204	−0.238
	(0.107)	(0.196)	(0.194)
GOP joint convention share	0.564***	0.666***	0.694***
	(0.104)	(0.149)	(0.145)
GOP joint convention share, squared	−0.532***	−0.662***	−0.681***
	(0.093)	(0.127)	(0.125)
17th Amendment indicator	0.002	0.026	0.011
	(0.022)	(0.026)	(0.016)
South indicator	0.042	–	−0.003
	(0.029)		(0.062)
Time trend	−0.0001	−0.001	–
	(0.002)	(0.003)	
Constant	0.020	0.072	0.040
	(0.046)	(0.068)	(0.068)
$N = 612$	$\chi^2 = 47.7$***	$F = 6.4$***	$\chi^2 = 42.5$***
		Within $R^2 = 0.06$	Overall $R^2 = 0.12$

Note: Unit of observation is a state-year, 1880–1940. FGLS errors are assumed to be AR-1 within a state's time path and heteroskedastic across states in a given year. Standard errors are in parentheses.
*denotes significance at $\alpha = 0.10$, ** at 0.05, *** at 0.01.

held to their preamendment standard. The trade-off is analogous to comparing two estimators, one having lower bias but greater variance than the other. Our empirical results show that the implications of this view do appear in senator behavior and that it is helpful in understanding the agency relationship between the mass electorate, its expert political intermediaries, and its U.S. Senate delegation. The same patterns do not appear in the House of Representatives, ruling out more general contemporaneous factors unrelated to direct election as an explanation for our finding. In short, we found an effect of the treatment on treated units that is in line with our theory and did not find an effect of the treatment on untreated units.

Theoretically, the trade-off we identify between responsiveness and monitoring is an important consideration for the design of electoral institutions. Even the 17th Amendment itself appears in contemporary policy debates occasionally: for example, within the last several years, senator Zell Miller (D-GA) introduced a measure in the Senate calling for its repeal (Pierce 2004),[42] and Alan Keyes made its repeal part of his platform in the Illinois race for the U.S. Senate in 2004 (Pearson 2004).

[42]Less than a week before Miller's motion, Rep. Tom DeLay (R-TX) came out against the 17th Amendment and stated that he would be willing to discuss its repeal (Pierce 2004).

In the end, however one comes down on the trade-off created by direct agency, our theory and results show it does matter for representation and the interests that get reflected in public policy.

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