

**Agency Problems and Electoral Institutions:
The 17th Amendment and Representation in the Senate***

Sean Gailmard

Department of Political Science
Northwestern University
gailmard@northwestern.edu

Jeffery A. Jenkins

Department of Political Science and Institute for Policy Research
Northwestern University
j-jenkins3@northwestern.edu

Abstract

Delegation of decision making authority to agents with different preferences and better information than their principals is ubiquitous in politics, and political representation is no exception. A prominent change in a political agency relationship, at least in formal terms, occurred when the 17th Amendment to the Constitution established direct election of U.S. Senators as of 1914. What effect did this institutional change have on the representation of states by their senators? We argue that prior to the 17th Amendment, the state population relied on an intermediary, the state legislature, to control its U.S. Senators. This intermediary was a sophisticated monitor of the behavior of senators, but itself not a perfect agent of the populace. After the 17th Amendment, the state populace gained more direct control over its U.S. Senators, but sacrificed expert monitoring of their behavior. If this view is correct, senators after the 17th Amendment should better reflect the ideology of their home states' populace, but also exhibit greater variability relative to other members of their state's delegation. We show that these expectations are borne out empirically. First, U.S. Senators from moderate states exhibit less extreme roll call behavior after the 17th Amendment; second, differences in roll call records for senators from the same state are greater after the 17th Amendment. Finally, we argue that the moderating influence of the 17th Amendment has made the Senate less polarized as a body than it would have been otherwise.

* A previous version of this paper was presented at the 2005 annual meeting of the Midwest Political Science Association, and in seminars at the University of Iowa, Duke University, Texas A&M University, and the University of Chicago. Thanks to Scott Ainsworth, Fred Boehmke, Doug Dion, Erik Engstrom, David Epstein, Sam Kernell, Jeff Milyo, John Patty, Dave Primo, Wendy Schiller, Chuck Shipan, Craig Volden, and other seminar participants for helpful comments.

1. Introduction

How should electoral institutions be structured to allow for both deliberation and representation in elected assemblies? Answers to this question are implicit in the institutions designed by the framers of the Constitution, as well as in subsequent reforms to the original design. One of these reforms is the 17th Amendment (1913), which changed the basis of election of U.S. Senators from the state legislature to a statewide popular vote. Yet, despite the apparent significance of this change, scholars have only recently begun to examine its effects on representation in the Senate. Those effects are the issue we address in this paper.

In purely formal terms, the 17th Amendment established a clear change in the connection between voters and U.S. Senators. A simple agency theoretic view posits that the amendment made Senators direct agents of their ultimate principal, rather than indirect agents directly accountable to the state legislature. Put differently, the amendment replaced indirect control with direct accountability, which should have made senators responsive to state preferences directly. While we believe this simple agency logic is partly right, we hold that it misses an important factor. That is, in terms of democratic accountability of senators to their state electorates, the 17th Amendment involves a tradeoff. While it did create a direct agency relationship, it also eliminated the informed selection and monitoring of U.S. Senators by relative political experts (i.e., state legislators). In short, senators may have been held to a *better* post-amendment standard, but not as *tightly* as they were held to their pre-amendment 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.

Our conceptual discussion has several implications that we test using scaled roll call voting decisions of U.S. Senators in presidential election years from 1872 to 2004 as a measure of senator behavior, and state-level Democratic presidential vote share as a proxy for state “ideology” or policy preferences. In particular, our argument implies that senators should be less extreme in their roll call records, conditional on state ideology, after the 17th Amendment than before it – because senators were now directly responsible to the state electorate. In addition, 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 accountable to expert monitors. We find strong support for both hypotheses.

Our results also suggest that the 17th Amendment led to a conditional decline in polarization in the Senate. Before the amendment, senators with moderate voting records tended overwhelmingly to be from very moderate states, directly in the middle of the distribution of state ideologies. But slight changes in state ideology led to large changes in senator roll call behavior, so that a somewhat (but not very) moderate state had about as extreme a senator as a relatively extreme state. After the amendment, senators from somewhat (but not very) moderate states were more likely to have somewhat moderate voting records. This implies a decline in polarization conditional on the preferences of the states sending delegations to the Senate. We find that controlling for heterogeneity in state ideology and other factors, the Senate as a whole has been significantly less polarized since the 17th Amendment than before it.

The rest of the paper is organized as follows. We begin by briefly reviewing the related literature in Section 2. We then discuss the theory behind our argument in Section 3, and flesh out specific empirical hypotheses and data sources in section 4. Our main empirical results are presented in Section 5, after which we examine the implications of our argument for polarization in the Senate in Section 6. We conclude in section 7.

2. Related Literature

Despite constituting one of the most prominent electoral reforms in U.S. history, the 17th Amendment has elicited surprisingly few scholarly treatments. Of the studies that exist, most have examined the *causes* of the amendment (Haynes 1906, 1938; Rogers 1926; Ellis and King 1996; King and Ellis 1999; Wirls 1999a, 1999b).¹ Few studies, by contrast, have examined the *consequences* of the amendment. Until recently, Riker (1955) stood as the primary work on the subject; he argued that the 17th Amendment, rather than being a significant change, was in fact largely anti-climactic, as a majority of states by 1912 had already passed primary-election laws that served as de-facto direct-election instruments.² Stewart (1994) agreed with Riker's assessment, arguing that the 17th Amendment simply

¹ These studies investigate a range of factors. Most discuss public opinion shifts toward progressivism in the late-19th Century that helped lead not only the 17th Amendment but other reform measures, like direct primaries, the income tax, and the Australian Ballot. Factors specific to the 17th Amendment are also investigated in detail, such as inter- and intra-party conflicts over issues like race, malapportionment, machine politics, and corruption.

² Rogers (1926, p. 114) documents twenty-nine such states. Lapinski (2003) identifies thirty.

ratified a trend toward electoral popularization that had become ubiquitous in the states.³ Crook and Hibbing (1997) were the first to argue that the 17th Amendment produced a significant effect; 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 election (on the latter point, see also Engstrom and Kernell 2003).

In the last few years, a small literature has emerged that examines the *behavioral* effects of the 17th Amendment in the Senate. Specifically, this line of 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. Poole and Rosenthal (1997, pp. 218, 225-26) found that senators, post-amendment, had higher abstention rates on roll call votes, consistent with the notion that senators in the direct-election era spend more time in their states courting voters. Lapinski (2003) found that senators, post-amendment, were more likely to retain their committee assignments, so as to maximize their ability to claim credit for policy outputs. Meinke (2005) reported that senators, post-amendment, focused more heavily on position-taking activities and voted in a more ideologically-consistent manner. Bernhard and Sala (2006) found that senators, post-amendment, were more likely to seek reelection and moderate their voting behavior as reelection neared. The sole exception to this trend is Wawro and Schickler (2005), who uncovered little evidence that senators' voting behavior, post-amendment, converged to the average House member's voting behavior in their state.⁴

Our research contributes to this emerging literature on the behavioral effects of the 17th Amendment. To our knowledge, however, ours is the first study to *explicitly* connect senators with the geographic units they represent, a level of analysis that other research (on House members) has shown to be important for the study of representation (see, e.g., Canes-Wrone, Brady, and Cogan 2002). We believe this “dyadic” approach offers important insights that are not captured by studies that analyze either the aggregate behavior of the

³ Lapinski (2000, 2003) finds little evidence to corroborate the Riker/Stewart argument.

⁴ One potential problem with Wawro and Schickler's analysis is their use of common-space W-Nominate scores to assess behavioral change. Because the common-space scores create a single set of ideal points for each member of Congress, Wawro and Schickler were forced to drop senators who served in both the pre- and post-direct election periods.

Senate in relation to national public opinion or the behavior of individual senators facing common electoral pressures (but not ones specifically tied to their states' preferences). The geographic unit is the level at which electoral incentives operate, and that is the key concern in our theoretical approach.

Our study differs from others in the behavioral-effects literature in one additional regard: our unit of analysis is an individual state's Senate delegation, not an individual senator. Here, we follow other scholars who have focused on differences within a state's Senate delegation for purposes of theoretical/empirical leverage. The most comprehensive such treatment is Schiller (2000), who argues that Senators adopt separate (and typically different) "portfolios" in order to represent the often diverse interests in a given state. In addition, Poole and Rosenthal (1984) investigate ideological polarization in the Senate over time by examining within-state differences for same-party and split-party Senate delegations, while Goff and Grier (1993) investigate the relation between within-state ideological differences and state-level heterogeneity. Our purpose is not to modify these explanations, though we do find interesting patterns in within-state differences consistent with previous treatments. Within-state differences are important in our approach because of how they should vary with electoral institutions, as well as state characteristics, if the logic we identify is correct and relevant.

Lastly, our research touches on aspects of congressional representation more generally. In particular, the degree of disconnect between a member's geographic constituency and the particular interests that drive her behavior is a recurring theme in the representation literature. Fenno (1978), for example, has identified different ("concentric") constituencies to which legislators attend, depending on electoral circumstance or immediate need, while Schiller (2000) argues that senators assemble multifaceted networks, often different from those of their same-state senatorial colleagues, to insure continued electoral success. In a different vein, Levitt (1996) estimates weights in senators' utility functions, showing that geographic constituents, the stance of the national party, and the senator's own ideology all have nontrivial effects on roll-call voting.⁵ We build on these studies by showing that senators

⁵ Levitt assumes that senators maximize a weighted utility function when casting roll call votes, as is standard in spatial analysis of roll call behavior (e.g., Poole and Rosenthal 1997). Levitt's key innovation is to parcel out this utility to different interests and devise a method to estimate the weights on each interest using roll-call voting behavior. In principle, the method could be applied to the 17th Amendment, but it requires preferences of

face numerous concerns and pressures, both intrinsic and extrinsic, and argue that it is unrealistic to expect them always to follow the will of the geographic constituency (even accounting for their possibly better information about policy options).

3. Theoretical Background

Our starting assumptions are that “ideology” or some amalgamation of policy preferences can be usefully analyzed in terms of preferences in a one-dimensional policy space, and that the relationship between an electorate and its representative is, at least in part, a principal-agent relationship. The electorate in representative government delegates decision making responsibility to its 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 that has ideological beliefs far from its own (“adverse selection”), and (ii) inducing its agent to make decisions it likes (“moral hazard”).⁶ The chief formal lever the electorate has to influence its agent’s type and behavior is, of course, an election. Elections are useful both for selecting agents whose preferences are compatible with the electorate’s, and for inducing an agent with any given preferences to act in accordance with the preferences of the electorate. But they are also blunt instruments of accountability.

The 17th Amendment changed the agency relationship between a state electorate and its U.S. Senators. Before the amendment, the relationship was one of hierarchical agency – the principals selected an agent, who in turn selected another agent – with delegated monitoring of the second agent by the first. That is, voters chose their agents, the state legislators, who in turn (or by the direction of state political elites) chose another set of agents, U.S. Senators, on behalf of themselves and the state electorate. The state legislature would generally be composed of relative sophisticates with a better grasp of U.S. Senator behavior, and the ideology and preferences of possible future senators, than the voting population as a whole. Being immersed in political information networks and following

each interest be measured in the same space (so that utility weights can be assumed to sum to one, a crucial identification assumption). Replicating this design for the time period under consideration is difficult, especially with regard to deriving individual preferences of state legislators.

⁶ 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.

politics simply as part of their (possibly part-time) jobs, state legislators were more likely than the underlying citizenry to know or learn about U.S. Senator behavior and the true leanings of senate hopefuls in the course of their daily business. As relative experts on politics, they could keep a close eye on behavior in the Senate (alleviating “moral hazard” by sitting senators), and select the “right” senators when a vacancy arose or the state legislature changed party majorities (alleviating “adverse selection” of new senators). Both capabilities would allow the state legislature, or the subset of elites dominating the state legislature, to hold its senators relatively tightly to its preferred standard of behavior.⁷ The state electorate was essentially forced, before the 17th Amendment, to delegate the monitoring, selection, and control 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 preferred standard of behavior need not be the mass electorate’s preferred standard of behavior. Because elections are blunt instruments of selection and control in a “market” with two differentiated “products,” the state electorate must incur some “agency losses” relative to first-best, perfect control of the decisions of the state legislature. Opportunistic legislators can be expected to substitute, to some extent, their own preferences (or those of party bosses, etc.) 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. This is per force true about the selection of U.S. Senators and the standard to which they are held by the state legislature.⁸

Putting these arguments together, viewing pre-17th Amendment U.S. Senators in terms of hierarchical agency with delegated monitoring implies that they would hew relatively closely to a standard determined by the state legislature, but that this standard may *not* be the one chosen by the median in the mass electorate.

⁷ Naturally, state legislators themselves may have been controlled or strongly influenced by state party leaders or chamber leaders. This does not affect our argument: whether state legislators or elites controlling state legislators dominated the U.S. Senator selection process, they were still almost certainly more sophisticated political observers than the state mass electorate.

⁸ 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.

The 17th Amendment made the terms of the agency relationship quite different. Instead of hierarchical 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 agent to hold a further downstream agent to account for them. Instead, they could select new U.S. Senators, and induce behaviors from sitting senators with any given ideology, on the basis of 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 monitored and tightly constrained to the “wrong” standard, since the amendment they have had weaker monitoring and a looser constraint to the “right” standard.

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, post-amendment, 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, 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.

4. Hypotheses and Data

This view of the state-senator relationship has two testable implications regarding the effects of the 17th Amendment on representation in the Senate.⁹ First is an "average responsiveness" effect. If direct agency allows voters to hold U.S. Senators to a "better" standard than hierarchical agency, that should affect how legislators behave, on average, as a function of their state's ideology. At this level the average responsiveness hypothesis says that a state's Senate delegation should, on average, better reflect its specific ideology after the 17th Amendment than before it. This suggests using the Senate delegation from state i in year t as the unit of analysis.

Second, the implied effect of removing delegated monitoring of downstream agents by upstream experts is an "increased discretion" effect. If the electorate cannot 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 and more likely to want to do so. Holding everything in a senator's political environment constant, they should exhibit greater differences in behavior from each other after the 17th Amendment than before it. This again suggests using a state's delegation as the unit of analysis, and focusing on differences in observed behavior for members of the same delegation.

To test these hypotheses we use DW-Nominate scores (Poole and Rosenthal 1997) for individual senators in congresses that encompass presidential election years from 1872 to 2004 as a measure of senator "ideology." These scores are the output of a technique (the Nominate estimation procedure) that scales the roll call records of senators into a basic multidimensional policy space. The scores are explicitly designed to allow for dynamic comparisons of the ideology exhibited in roll call behavior (the "D" part), and are based on a weighted utility function expressing how each legislator trades off policy gains in different

⁹ To the extent that the 17th Amendment only ratified reforms that individual states had already made before 1914, as argued by Riker (1955) and Stewart (1994), 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.

dimensions of the policy space (the “W” part). To put the scores into a single dimension and examine differences between senators, we take the weighted average of a senator’s estimated position in each dimension (the second dimension is weighted by .375; cf. Poole and Rosenthal 1997).¹⁰ The scale of the resulting scores ranges from -1 to 1, with smaller numbers implying a more liberal roll call record. In addition, because the weight of the second dimension changes over time (Poole and Rosenthal 1997), we explore all our results using only the first dimension scores, which captures a more enduring left-right conflict present in every era of American politics. Also, because the time span is quite long and may strain the assumption that years can be pooled, we explore all our results over a more limited time span from 1880 to 1932. These and a number of other examinations of robustness overturn none of our substantive findings; we elaborate on this throughout.

To measure state-level ideology, we use state-level Democratic presidential vote share data as a proxy, a common technique in the literature (see, e.g., Carson 2005).¹¹ Specifically, we use the votes cast for the Democratic presidential candidate as a percentage of all presidential votes cast in a state.¹² This measure has several important benefits for our approach, which requires ideology-conditioned senator behavior over a long period of time. Most importantly, Democratic 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 Democratic party represented the “left” 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 100, larger values of state Democratic presidential vote share indicate a more liberal predisposition than smaller values. Whatever “liberalism” might have meant in a given year, states seeking it in a president would, in our theory, also seek it in U.S. Senators.

¹⁰ In a check on their main results obtained with ADA scores, Canes-Wrone, Brady, and Cogan (2002) also use DW-Nominate scores to study representation. They use the arithmetic mean of the two dimensions.

¹¹ A similar technique is used to generate a measure of district-level ideology in studies of House members’ behavior. See Ansolabehere, Snyder, and Stewart (2000, 2001) and Canes-Wrone, Brady, and Cogan (2002).

¹² We also examine state Democratic vote shares as deviations from national average Democratic 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.

Our approach suggests that we analyze changes in a measure of both the central tendency of behavior in a Senate delegation, and the variability of behavior in a Senate delegation. The “average location” of a state delegation is the arithmetic mean of the DW-Nominate scores of its members.¹³ The “within-delegation distance” is the maximum difference¹⁴ between the DW-Nominate scores for any pair of members¹⁵ in the delegation. Breaking the delegations down into a delegation-specific central tendency (for each given year) and a delegation-specific variability (for each given year) is useful for showing changes and conditional aspects of these two distinct features of a delegation separately. For example, with the average location of a state delegation, we can study the mean and variability of the delegations’ central tendencies conditional on state ideology, without confounding it with the variability of locations within that delegation. A delegation is counted as a “split-party” delegation if any two members in it with DW-Nominate scores are from different parties (including third party members).

Following Canes-Wrone, Brady, and Cogan (2002), we pool observations from different years over time. Ideally for this type of approach, measures of senator behavior and state ideology each should have a constant temporal meaning. DW-Nominate scores meet this requirement because of the overlapping membership in different Senates over time,¹⁶ but the assumption is more questionable for state-level Democratic presidential vote share. For example, if the rise of presidential primaries caused presidential politics to moderate ideologically after the adoption of the 17th Amendment, it would not mean the same thing for a state to vote 60% Democrat in each era. Rather, that state would be expressing support for a more liberal candidate pre-amendment than post-amendment, and would in that sense be more liberal. This could make representation that is in fact unchanged (or even gets worse) appear better, and credit it to the 17th Amendment.

¹³ We also computed the within-delegation average with each member weighted by the votes s/he cast in a Senate session. The correlation with the arithmetic mean measure is 0.994.

¹⁴ 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.

¹⁵ Typically there were only two members per delegation per year, but some states had three or even four senators in a session who cast enough votes over their careers to have DW-Nominate scores.

¹⁶ This conclusion is most secure within a stable period in American politics; otherwise the importance and content of the second dimension can change. This is one of the reasons why we also checked, as noted, the empirical results using only the first dimension score for each legislator, which has a more stable interpretation over time. The results were essentially unchanged in terms of qualitative findings and statistical significance (and in some cases strengthened the estimated quantitative impact of the 17th Amendment).

We can use DW-Nominate scores for presidents to provide a cursory check on this problem.¹⁷ Unfortunately, only two Democrats served as President between 1872 and 1914 (Cleveland and Wilson), and Nominate scores for presidents are generally not as reliable as scores for legislators (since presidents take positions on far fewer roll calls). Nevertheless, the presidential Nominate scores do not give any additional cause for concern about scale comparability and may alleviate it. If anything, they show that presidential politics is more polarized since the end of the Progressive era than during and before it. Republicans on average are further right and Democrats are somewhat to the left.¹⁸ Generalization is hazardous in this case,¹⁹ but this result suggests that a state with a given Democratic vote share after 1914 is, if anything, somewhat more liberal than a state with that vote share before 1914. The effect of the scale change may be small enough not to matter, but since we find evidence of better average responsiveness since the 17th Amendment, we believe that if there is an effect it strengthens our findings.

It will be useful to specify a concept of “better representation” of states by their senators. If a one unit change on the x -axis always meant the same thing as an m -unit change in the y -axis, we could define “perfect conditional representation” to exist when the relationship between the measured state ideology and measured senator behavior has a slope of $-m$, and the most moderate point on the domain maps into the most moderate point on the range.²⁰ Then a one unit increase in state liberalism would be associated with a one unit increase in the liberalism of its senator’s roll call behavior. Of course, we are not that fortunate. However, we can still (or at any rate, we will) define “better conditional representation.” Let $s > 0$ and $t < 0$ denote the maximum and minimum scaled roll call

¹⁷ Since Presidents often take positions on legislation that is voted on in Congress, they can also be placed in the same congressional choice space, which thus allows presidential comparisons over time. For example, McCarty and Poole (1995) use CQ Presidential Support Roll Calls to generate Nominate scores for Presidents back to Dwight D. Eisenhower. More recently, Nominate scores for Presidents have been generated back to Thomas Jefferson, based on roll calls corresponding to Presidential requests. These latter roll calls were compiled by a research team led by Elaine Swift, under the auspices of an NSF grant. The Nominate scores for these early Presidents can be found at http://pooleandrosenthal.com/junkord/HL01108A1_PRES.DAT.

¹⁸ The average first dimension DW-Nominate scores for pre- and post-amendment Democratic presidents are -0.40 and -0.46. The pre- and post-amendment Republican averages are 0.29 and 0.52.

¹⁹ In addition to imprecision, these scores are based only on election winners, so there is some selection effect (e.g., McGovern cannot pull the Democrats left and Goldwater cannot pull the Republicans right).

²⁰ The second caveat assures that representatives not only change in the same way that their constituents change, but that they would be “close” to their constituents if translated into their space. It is necessary to overcome Achen’s (1977, 1978) critique that a strong relationship between the two is possible even when representatives are distant from their electorates, so that looking only at the strength and direction of the relationship would consider representation “good” when it is actually “bad.”

scores. Conditional representation is “better” under institution A than institution B when, conditional on any specific level of state liberalism, measured senator behavior in institution A is on the same side of the line passing through the points $(0,s)$ and $(100,t)$ as, but less extreme than, senator behavior in institution B . One implication of this is that if the relationship between these measures of state ideology and senator behavior is (say) cubic or shaped like a “backwards S,” representation is better when the curves of the S are more gradual. In other words, controlling for proximity in the policy space, systems that associate a slight change in state ideology with a large change in senator behavior count as “less representative” than systems that associate the same change in state ideology with a less extreme change in senator behavior.

5. Empirical Analysis

The empirical analysis is broken down into two components. First, we analyze the relationship between state ideology in year t , as measured by state Democratic presidential vote share in year t , and the average behavior of its U.S. Senate delegation in the congress encompassing year t , as measured by DW-Nominate scores. This addresses the “average responsiveness” aspect of the theory. Second, we analyze the relationship between year t ideology in a state and the distance between the roll call records of its senators in the congress encompassing year t . This addresses the “increased discretion” aspect of the theory.

An intuitive feel for average responsiveness can be drawn from Figure 1, which shows the average DW-Nominate score for a state’s Senate delegation in a given year, as a function of the state’s Democratic presidential vote share for that year. The first order relationship between state Democratic presidential vote share and the average scaled location of its Senate delegation is clearly negative both before and after the 17th Amendment (1913).²¹ That is, more liberal states are represented by more liberal senators. But it is also apparent that the relationship is not linear. Both before and after the 17th Amendment, the relationship has a “backwards S” or cubic shape. Extreme states have extreme senators, but moderate states have all kinds of senators. This finding closely resembles the empirical pattern

²¹ We use the date of national adoption of direct election, rather than individual states’ adoption dates. The national adoption has the virtue of being exogenous for at least those states that had not adopted direct election autonomously before 1913. In any case, the first state to institute direct election (Oregon) did not do so until 1907, so assuming a single common date of adoption seems unlikely to greatly affect the results.

presented in Snyder and Ting (2002) for the House from the 1970s through the 1990s. Moderation on the x -axis (i.e., convergence toward 50%) is the natural version of electoral heterogeneity in a one-dimensional policy space, and in that sense is also reminiscent of Bailey and Brady's (1998) finding that senators from more heterogeneous states have less predictable voting records.

[Figure 1 about here]

The operative feature of the figure for the “average responsiveness” hypothesis is that the “S” appears to be tighter before the 17th Amendment. Specifically, its slope for moderate states is greater in absolute value before the amendment. Senator behavior in the 40 years before the amendment had a “bang-bang” character. The band of state ideologies that would lead to moderate behavior in the Senate was relatively thin. On a given side of that narrow band, states tended to get draws from the same distribution of relatively extreme senators. Since the amendment, the band of state ideologies associated with moderate senator behavior has been much wider. Extreme states have still tended to get extreme senators, but fairly moderate states get fairly moderate to extreme senators, and very moderate states (in the middle of the x -axis) still get senators with the widest range of behaviors. The “middle” of the S appears in about the same place before and after the amendment; the slope is just more gradual after it. In the sense defined above and consistent with the average responsiveness hypothesis, representation of state ideology appears to be better since the 17th Amendment than it was before it.

Of course, this figure is only suggestive. It is possible that an apparent change in the cubic relationship is not statistically significant, or is picking up some (unspecified) intervening effect of unified party Senate delegations, some effect of the South, etc. Thus we control for these factors statistically. Given the panel structure of the data, a natural approach is to use “feasible” Generalized Least Squares estimation²² – which allows for heteroskedastic errors and first order autocorrelation²³ – of the average location of a state's

²² We also explored OLS with panel corrected standard errors. However, the contemporaneous correlation across units that helps this approach to outperform feasible GLS in comparative data is probably less of a factor in our study. Regardless, as noted in footnotes below addressing robustness, PCSE's do not change our main findings.

²³ 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.

senate delegation, with linear, quadratic, and cubic terms for state Democratic presidential vote share, the 17th Amendment, the 17th Amendment interacted with the three presidential vote share indicators, an indicator for the South, and indicators for unified Republican and Democratic delegations as explanatory variables. The problem with this approach is that state ideology, the 17th Amendment, the 17th Amendment-ideology interactions, and being in the South all probably have a causal effect on the makeup of a state senate delegation. The 17th Amendment certainly made delegation splitting easier for a state (Brunell and Grofman 1998), since senators were no longer selected by an assembly with a specific partisan tilt and agenda. Moreover, in any time period, a more extreme state is more likely than a less extreme state to have a solidly partisan political establishment, and less likely to have a credible candidate from the opposition party. Similarly, over much of the time period in our data, Republican party organizations in the South were extremely weak, so Republican candidates in Southern states were unlikely to win Senate seats in the short and medium term regardless of citizen preferences.

Failing to account for this causal relationship would mean that the explanatory variables that cause unified party delegations would not be credited with their full effect on average-delegation location. In other words it introduces a consistency problem, understating the effect of state ideology, the 17th Amendment, the amendment-ideology interactions, and Southern location for any sample size. What we actually want as an explanatory variable in the average-delegation location regression is the portion of the unified party delegation variables that is *not* explained by the *other* independent variables. Put differently, the initial specification of interest is

$$y = \beta_0 + x\beta_1 + z\beta_2 + \varepsilon,$$

but a causal relationship between X and Z exists so that

$$z = \alpha_0 + x\alpha_1 + \delta,$$

where ε and δ are stochastic errors independent across draws of y and draws of z respectively. In short, we have a recursive system of equations with strictly nested sets of regressors.²⁴ Substituting the second equation into the first gives

$$\begin{aligned} y &= \beta_0 + x\beta_1 + (\alpha_0 + x\alpha_1 + \delta)\beta_2 + \varepsilon \\ &= (\beta_0 + \alpha_0\beta_2) + x(\beta_1 + \alpha_1\beta_2) + \delta\beta_2 + \varepsilon. \end{aligned}$$

So estimating the mean of y conditional not on x and z , but rather on x and the part of z that x does not explain (i.e., δ), credits x with its direct and indirect effect on EY (i.e., $\beta_1 + \alpha_1\beta_2$ rather than just β_1).²⁵ If the random component of Y is uncorrelated with the random component of Z , then least squares estimators of the parameters in the final equation are consistent.²⁶

To address this issue we use a two-stage approach. The first stage involves two separate linear probability models of the dichotomous unified Democratic and Republican delegation variables on the three state Democratic presidential vote share variables, the 17th Amendment, the 17th Amendment interacted with the three presidential vote share variables, and a dummy for the South.²⁷ Then we use the residual unified Democrat and Republican delegation – by definition, the portion of each unified party delegation category unexplained by state ideology, the 17th Amendment, the amendment-ideology interactions, and being in

²⁴ For this reason, a standard instrumental variables approach like Two Stage Least Squares is not suitable, because the exclusion restrictions are not satisfied.

²⁵ The “orthodox” method for estimating recursive systems and obtaining overall effects is to regress Z on X , then regress Y on X and Z , then compute the overall effect of X on Y as (i) the direct effect estimated in the second regression, plus (ii) the effect of X on Z from the first regression times the effect of Z on Y in the second. Clearly, the two-stage approach we use identifies this as well, but somewhat more intuitively, and presents the significance test for the overall effects of the exogenous variables, as well as the magnitude and significance of unexplained party splitting, in one set of estimates. Besides this approach, simply omitting party splitting *and* its first stage residual from the specification entirely would also consistently estimate the overall effects of the amendment and state ideology: the effect of the unexplained variation in party splitting would fold into the residual as extra white noise (and would raise the standard error of the regression) by assumption. But then we would require a separate set of results for the magnitude and significance test of the unexplained portion of party splitting. All in all, we think our two stage approach is easier to interpret and discuss, this footnote notwithstanding. In any case, we also used the orthodox approach and found comparable magnitudes and significance of the amendment.

²⁶ For purposes of our theory, the important identification condition is that the grand effect of the 17th Amendment be identified, not that the direct and indirect effects be identified.

²⁷ We estimate an OLS/linear probability model rather than (say) a logit model because (i) the LPM gives “true” mean zero residuals orthogonal to the regressors whereas nonlinear models do not, and (ii) we are not directly interested in efficiency or the prediction of the conditional mean in the first stage.

the South – along with the other controls and theoretical variables in a second stage Generalized Least Squares estimation of average senate delegation location.

Results from the GLS estimation are reported in Table 1.²⁸ Consider first the top three parameter estimates. Taken together, these three quantities simply estimate the observed cubic relationship between state ideology and the average behavior of a U.S. Senate delegation before the 17th Amendment. The signs and magnitudes together produce the “backwards S” relationship between Democratic presidential vote share and Senate delegation behavior apparent in Figure 1.

[Table 1 about here]

The indicator variables for the 17th Amendment, being from a Southern state, and having a unified party Senate delegation are all straightforward to interpret. The interaction terms show that the shape of the curve did indeed change. Thus, the finding (in Figure 1) that the backwards S after the 17th Amendment is flatter and more gradual than before is statistically robust to controls for other factors. The estimated pre-amendment curve as a function of state Democratic presidential vote share is determined by the un-interacted polynomial terms; the estimated post-amendment curve is determined by the sum of the interacted and un-interacted terms of each order. Since the coefficients for the interacted terms are significantly different from 0, the curves for the pre- and post-amendment periods are different. And since the coefficient for each interacted term is slightly smaller in absolute value as, but the opposite sign from, the un-interacted polynomial term of the same order, the post-amendment curve is flatter.²⁹ In sum, the estimated relationship both before and after the amendment has a cubic shape, but it is significantly flatter and more gradual after the amendment. This “flattening” after the 17th Amendment is illustrated in Figure 2, which

²⁸ Other assumptions about the specification and error structure produced substantially similar results. In particular, the findings about the difference in the curves estimated for the pre- and post-amendment periods, controlling for other factors, also appear in OLS estimation, OLS with year random effects (which relaxes the assumption of identical distribution for the dependent variable over time), OLS with state fixed effects and autocorrelated errors, and Prais-Winsten regression with panel corrected standard errors (allows contemporaneous correlation of panels, AR-1 errors within a state’s time path, and heteroskedasticity). While we emphatically agree that a theory-driven approach to the error structure is better than using results just because they look similar no matter what tool is thrown at them, we would also be suspicious if the results were wildly sensitive to assumptions about the action in the error term.

²⁹ Note that the hypothesis is that the curve has a different shape before and after the amendment, so the question is whether the polynomial terms interacted with the amendment are different from the un-interacted polynomial terms – not whether the sum of the interacted and un-interacted terms for a given polynomial are different from zero.

plots the predicted values from the Table 1 regression, assuming a non-Southern state with a unified-party delegation.

[Figure 2 about here]

These results are based on all presidential election years from 1872 to 2004. While good for precision, some of these observations may be from different “political regimes” than others. Realignments, changes in the meaning of the measurement scales, or other large scale changes all may have changed the relationship in some way that nevertheless shows up as a significant difference in average representativeness since the 17th Amendment. We checked the robustness of this result by limiting the time span used for estimation. Excluding the years 1912 (when the amendment may have already been anticipated) and 1916 (when its incentive effects may not have been fully understood) and looking only at the relatively more stable time period from 1880 (after Reconstruction) to 1932 (before the New Deal), we find much the same pattern: the estimated relationship between state ideology and Senate delegation behavior has a cubic structure. But it is significantly flatter and more gradual as a function of state ideology after the 17th Amendment than before it, even controlling for other potential confounds.^{30, 31}

In general this evidence supports the “average responsiveness” hypothesis. By the notion of representativeness specified above, senators have been more representative of their states – particularly states that are neither right in the middle nor extreme ideologically – since the 17th Amendment. The direct agency relationship removed one source of agency loss in electoral politics, namely that the downstream agent would be held to the standard of the upstream agent – and not necessarily the ultimate principal.

We now turn to the “increased discretion” hypothesis. A graphical first cut at this is presented in Figure 3, which displays the difference between the scaled roll call scores for

³⁰ Using only first dimension DW-Nominate scores over the entire range of years produced the same significant “flattening” of the cubic relationship between state ideology and delegation average following the amendment. The effect also occurred when we used only first dimension scores and the years from the end of Reconstruction to the start of the New Deal.

³¹ To ensure we are not incorrectly attributing the effects of larger trends in state or national politics to the 17th Amendment, we also ran the model in table 1 with the average DW-Nominate score in the state’s delegation in the U.S. House as an additional explanatory variable. More general political factors affecting state delegation positions, not caused by the 17th Amendment and not captured in other variables, should be reflected in this variable. It proved to be highly significant ($p < 0.001$) suggesting that it may tap into state-level ideological factors excluded from other specifications, but including it did not change the qualitative effect of the amendment or statistical significance of the findings in the table.

members of a state's Senate delegation in the congress encompassing year t , as a function of its Democratic presidential vote share in that year. Aggregating over all the years, the relationship is concave. Within-delegation distance is greater in moderate states than in extreme states. As a state moderates ideologically (i.e., converges toward 50% vote share), the distance between its senators' roll call scores increases. A smaller majority in presidential vote share is a type of heterogeneity in a one dimensional policy space, meaning that within-delegation distance is greater for more heterogeneous states. While her approach emphasizes multiple policy dimensions and cleavages, this finding is similar in spirit to Schiller's (2000) demonstration that same-state senators exhibit greater differences when their state is heterogeneous than when it is homogeneous. Goff and Grier (1993) also use a multidimensional approach and show greater within-delegation differences in ADA scores for more heterogeneous states in the early 1980s.

[Figure 3 about here]

The key feature for the "increased discretion" hypothesis is that, conditional on state ideology, the within-delegation distances appear larger on average after the 17th Amendment than before it. This is particularly true for relatively moderate states. It would appear that senators since the amendment no longer behave so similarly as others facing much the same political environment. They may be pursuing their own ideological agendas, responding to their own reelection constituencies (which were forced to be more or less similar before the 17th Amendment), pursuing interest group endorsements or contributions, or something else. But they are doing it their own way more often.

Again, 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.³² We use the natural log of within-delegation distance as the dependent variable; this has a mean of -1.78 and a standard deviation of 1.25.

In terms of estimation, a natural approach is to use feasible GLS, with linear and quadratic terms for state Democratic presidential vote share and indicators for the South, split-party delegations (which can certainly affect within-delegation distance; cf. Poole and

³² We also check for robustness using a Generalized Linear Model, specifying a gamma 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.

Rosenthal 1984), and the 17th Amendment as explanatory variables. The problem – as with modeling average-delegation behavior – is that state ideology, the 17th Amendment, and South all probably have a causal effect on the likelihood of split-party delegations.

Thus, we again use a two-stage approach. The first stage is a linear probability model of the dichotomous split-party delegation variable on the 17th Amendment indicator, an indicator for the South, a measure of state ideology, and the square of state ideology. We then use the residual split-party delegation – by definition, the portion of party splitting unexplained by state ideology, being in the South, and the 17th Amendment – along with the other controls and theoretical variables in a second stage GLS estimation of within-delegation distance.

Results of the second stage GLS³³ estimation, with corrections for heteroskedasticity across states and autocorrelated errors over a state’s time path, are presented in Table 2. The parameter estimates for the state ideology variables reflect the quadratic relationship apparent in the figure (i.e., the second derivative of the estimated function with respect to state deviation from the average ideology is negative). The unexplained variation in split-party delegations also has a strong and significant effect on within-delegation distance.

[Table 2 about here]

With respect to the “increased discretion” hypothesis, the 17th Amendment has a highly significant effect in the expected direction.³⁴ It increased the natural log of within-delegation distance by about a fifth of a standard deviation. As before, the statistical and substantive significance of the key explanatory variables are robust to restrictions on the time period used

³³ In the stage one LPM, the 17th Amendment indicator has a large, significant, positive effect on predicted probability of a split Senate delegation, while South has a strong, significant, negative effect. Deviation of a state’s Democratic presidential vote share from the average does not have a significant effect, but its square has a significant, negative effect. All of these effects are as expected per the justification in the text.

³⁴ We do view a one-stage, direct estimation approach that ignores indirect effects as deficient, but even in that case, OLS estimation with robust standard errors gives a p-value of 0.054 for the 17th Amendment indicator, controlling for the same explanatory variables. As expected the estimated effect goes down: it is about one half as large as the one presented in the text. The split-party variable has a comparable magnitude in OLS estimation (as it should) and is highly significant. But the estimated relationship between within-delegation distance and state ideology is not significantly concave. A Generalized Linear Model assuming within-state distances are gamma distributed also confirms that the 17th Amendment has a significantly positive effect on within-state distances, even when its causal effect on party splitting is not reckoned. Estimating that model with the same independent variables and robust standard errors produces a p-value of 0.01 for the amendment.

in estimation,³⁵ using only the first dimension of DW-Nominate scores,³⁶ and changes in the assumed error structure.^{37, 38} Of course we cannot say whether senators are using this discretion to pursue their own ideological agendas (in line with a “shirking” story), develop relationships with interest groups, assemble different reelection constituencies, etc. We can only say that senators appear to be less constrained by factors in their state political scene since the 17th Amendment than they were before it, and that this is the implication of eliminating delegated monitoring by political experts.³⁹

In short, the empirical support for the “increased discretion” hypothesis also appears fairly robust. Taken together, the empirical arguments in this section corroborate the agency theoretic view of the institutional change in the 17th Amendment.

6. Polarization in the Senate

The conceptual argument and empirical results in this paper imply that conditional on their state’s ideology, more senate delegations are more moderate in their roll call behavior after the 17th Amendment than before it. This in turn implies that, conditional on the ideological extremity of the states themselves, the 17th Amendment caused a decline in polarization in the Senate as a whole.⁴⁰ However, because this implication is conditional on

³⁵ Considering years 1880-1932, except 1912 and 1916, and allowing for heteroskedasticity and autocorrelation, the amendment’s estimated effect on the natural log of distance goes to about a third of a standard deviation (p-value < 0.001). Other qualitative findings from the table are also unchanged.

³⁶ Using the first dimension and all years in estimation, the amendment’s parameter estimate goes up to about 0.34 (p-value < 0.001); other findings from the table are comparable in magnitude and significance.

³⁷ OLS with year random effects, OLS with robust standard errors, panel-corrected Prais-Winsten regression, and Prais-Winsten regression with robust standard errors all produce very similar magnitudes and p-values as the GLS results in the table. In all cases the estimated curve is concave in Democratic presidential vote share, and the 17th Amendment indicator is significant. It raises log within-delegation distance by about a fifth to a quarter of a standard deviation.

³⁸ We also checked a specification with a measure of within-delegation distance in each state’s U.S. House delegation in a year as an additional explanatory variable in stage 2. This helps to ensure that effects of larger trends (not caused by the amendment) are not attributed to it, because broader political trends that change within-delegation distances unrelated to the amendment should be reflected in it. We used the interquartile range for House delegations as the measure of within-delegation distance in House delegations, rather than maximum distance within the delegation, which would artificially inflate the measure in large states. The statistical and substantive significance of the findings was unchanged.

³⁹ We also included a linear time trend in one specification to ensure that the 17th Amendment indicator is not just picking up some unspecified affect of the passage of time, and found that the magnitude of the dummy variable actually increased.

⁴⁰ We mean “moderation” as proximity to the midpoint of scaled ideology and “polarization” as average ideological distance. All else constant, if a group of senators shifts to more moderate positions, so that more senators have more moderate records, polarization in this sense will decline. We consider polarization and “heterogeneity – say, the variance of ideological positions – to be conceptually distinct, though of course they

state ideology, it does not imply that the Senate as a whole was in fact either more moderate or less polarized after the amendment. It only implies that the post-amendment Senate has been less polarized than it would have been had the amendment never passed. If states themselves became more polarized after the amendment than they were before it – say with one group of states moving left and another group moving right – more senators would exhibit more ideologically extreme roll call behavior after the amendment, causing an increase in overall Senate polarization.

Indeed the Senate has been less polarized since the adoption of the 17th Amendment. One piece of evidence for this appears in Figure 4, which shows the mean and median of the pairwise distances between state delegations⁴¹ for the congress encompassing each presidential-election year.⁴² This measure must lie in the [0,2] interval by construction; in our sample the mean pairwise distance is .42, with a standard deviation of .06 and a range of [.32,.52]. Except for a sharp drop in 1900, these measures of senate polarization began to decline around 1912, reaching a trough from the late 1930s through the end of the Eisenhower administration (roughly the consolidation of the New Deal consensus). While polarization has risen since the early 1960s by this measure, it is still well below its late 19th and early 20th century high points. These findings regarding polarization in the Senate are consistent with other research that addresses polarization more directly (see Poole and Rosenthal 1984, 1997, 2001; McCarty, Poole, and Rosenthal 1997).

[Figure 4 about here]

Not only has the Senate been less polarized over most of the 20th century than in the 40 years before the 17th Amendment, but states are less ideologically heterogeneous as well. One measure of this is the variance of state Democratic presidential vote shares. The mean vote share is about the same before and after the amendment (p-value in unequal variance t

may coincide for specific distributions such as a normal or uniform. Indeed, for state preferences as we measure them it appears that they do coincide.

⁴¹ Specifically, in each year, the average DW-Nominate score of a state delegation was computed, then the distances between the averages for pairs of states were computed, then for those yearly pairwise distances the mean and median were computed. These results are based on the weighted average of the two DW-Nominate dimensions as the score for each senator.

⁴² Average pairwise distance between groups is exactly the measure of polarization scholars are using when, e.g., they discuss the distance between party medians as a measure party polarization. Average pairwise distance is the generalization of this measure to the case of more than one pair of groups in the assembly. We explicitly prefer this measure to, say, the standard deviation of DW-Nominate scores or average delegation locations in a given year, because polarization is not necessarily the same as heterogeneity, though it may be in special cases (cf. next footnote).

test for difference in means is 0.28), and the assumption of a normal distribution for vote shares cannot be rejected before or after, so an increase in variance is essentially an increase in the scale parameter of the distribution.⁴³ The variance of state Democratic presidential vote share before the 17th Amendment is 254; the variance after the 17th Amendment is 179. This difference is highly significant (p-value 0.000003) in an F test for equality of variances.⁴⁴

Based on the theory and evidence above, both the general moderation of states and the 17th Amendment may have contributed to declining polarization in the Senate since the end of the Progressive era. Our agency argument implies a *conditional* decline in polarization, holding state preferences constant, since the passage of the amendment. In addition, the decreased average spread of state vote shares around a fixed mean may have caused moderation of Senator roll call behavior regardless of the 17th Amendment. To put this differently, senators can be viewed as functions. They map state-level ideology into roll call behavior in the Senate. The 17th Amendment flattened out the cubic function from the domain into the range. Moreover, states became somewhat less heterogeneous in their ideologies after the 17th Amendment, so the function operated on less extreme values in the domain. Both the change in the function and the change in domain values would tend to reduce polarization in the Senate.

We use a regression approach to assess the respective contributions of the institutional change and the general ideological moderation to declining Senate polarization. But there are other important factors to control for as well. As noted, other scholars have found important changes in legislative polarization over the time period in our data (Poole and Rosenthal 1984; McCarty, Poole, and Rosenthal 1997). With polarization measured as average distance between party members, both the House and Senate saw a decline in party polarization in the early 20th century, and polarization levels in both chambers – while on the rise recently – have stayed below their peak levels for most of the 20th century. Polarization levels in the chambers track each other very well (their correlation is about 0.89), and certainly in the House these changes have nothing to do with the 17th Amendment.

⁴³ Since the distribution and location parameter are about the same before and after the amendment, an increase in variance can be identified with an increase in polarization in this case.

⁴⁴ Since vote shares are approximately normal before and after the amendment, an F test for equality of variances is suitable. On the other hand, the observations in each subsample are certainly not independent, because of correlation of a state's ideology at different points in time. This probably inflates the significance of the difference in variances, but the p-value is so small that this seems unlikely to affect a conclusion about significance.

To avoid attributing larger, contemporaneous, but autonomous changes in polarization to the 17th Amendment, we control for party polarization in the House. This variable is a useful proxy for the broader political developments in polarization common to both the House and the Senate, but is not computed from the same observations as our measure of Senate delegation polarization (so should not be correlated with the error in a regression with our measure as the dependent variable). Our approach, then, is to regress the average pairwise distance between state delegations (our measure of state delegation polarization) on the variance of the state-level Democratic presidential vote share for that year, a 17th Amendment indicator, and party polarization in the House.

Results are presented in Table 3.⁴⁵ Clearly, party polarization in the House is tapping into broader underlying political changes that also affect the polarization of Senate delegations. The effect is positive and highly significant. The standard deviation of House party polarization is about 0.12, so a one standard deviation change in that measure increases Senate delegation polarization by about 0.55 standard deviations.⁴⁶ The effect of our measure of ideological heterogeneity across states has the expected sign but is not statistically significant.⁴⁷ Most importantly for our argument, however, is the finding that the institutional change embodied in the 17th Amendment had an important effect on top of the broader political developments proxied by the House polarization measure. The 17th Amendment indicator is significant at the 0.033 level, and lowers our measure of delegation polarization by about three fourths of a standard deviation. From this we conclude that the 17th Amendment has had a moderating influence in the Senate, in that the 17th Amendment

⁴⁵ The Durbin-Watson statistic for this regression is 1.38. The lower hurdle in a 0.05 level test for autocorrelation is 1.14, so autocorrelation is not especially severe. In a Prais-Winsten regression (allows for first order autocorrelation in residuals) with the same explanatory variables the estimated effect of the amendment was -0.044 (p-value = 0.049). Other findings were qualitatively similar to those in the table.

⁴⁶ We also used a linear time trend to account for broader (unmodeled) political developments in polarization or other important factors. In that specification the effect of the amendment goes up and still rejects the null hypothesis of no effect in a 0.10 level test. We prefer the specification in the text, as it has a more useful interpretation and the broader changes in polarization it captures are not linearly related to time.

⁴⁷ One possible reason why the variance in state Democratic presidential vote share does not have a significant effect on polarization is that, even with the changes in it over time, most states lie in the band of neither very moderate nor very extreme vote shares most of the time. These are the state ideologies that saw the biggest moderating change in average senator behavior after the 17th Amendment (Figure 1). These states tend to be more numerous at every point in time, regardless of changes in the variance of Democratic presidential vote share. That variance could decline significantly over time, but still not push enough states out of this band of ideologies to have a robustly significant affect on polarization. In short, variance in state ideologies can change without changing the “peakedness.”

(all else equal) has made the Senate less polarized as a body than it would have been otherwise.

[Table 3 about here]

7. 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 responsible to relative novices, so they could not be held to that standard as tightly as they were held to their pre-amendment standard. The tradeoff 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 changing polarization of the Senate as a whole.

Both the conceptual and empirical approaches in this paper have more general applications beyond this (important) institutional change in U.S. Senate elections. Empirically, this approach could be used to study any number of electoral reforms over time, such as the Australian Ballot, the Voting Rights Act, landmark “one person-one vote” court cases, or various campaign finance reforms, and their effect on representation.⁴⁸ Closer to the application in this paper, it would be interesting to study how the effect of the 17th Amendment propagated – was it by replacement of pre-amendment senators, by changes in their behavior, or both?

Theoretically, the tradeoff 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 year and a half, Senator Zell Miller (D-GA) introduced a measure in the Senate calling for its

⁴⁸ However, the very useful feature of multimember districts, which implicitly allows for control of many unobservable factors in a state political scene, is more or less unique to the Senate in the American context, except in special cases.

repeal (Pierce 2004)⁴⁹ and Alan Keyes made its repeal part of his platform in the Illinois race for the U.S. Senate in 2004 (Pearson 2004). In the end, however one comes down on the tradeoff created by direct agency, our theory and results show that it does matter for representation and the interests that get reflected in public policy.

⁴⁹ 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).

References

- Achen, Christopher. 1977. "Measuring Representation: Perils of the Correlation Coefficient." *American Journal of Political Science* 21: 805-15.
- Achen, Christopher. 1978. "Measuring Representation." *American Journal of Political Science* 22: 475-510.
- Ansolabehere, Stephen, James M. Snyder, Jr., and Charles Stewart III. 2000. "Old Voters, New Voters, and the Personal Vote: Using Redistricting to Measure the Incumbency Advantage." *American Journal of Political Science* 44: 17-34.
- Ansolabehere, Stephen, James M. Snyder, Jr., and Charles Stewart III. 2000. "Candidate Positioning in U.S. House Elections." *American Journal of Political Science* 45: 136-59.
- Bailey, Michael, and David W. Brady. 1998. "Heterogeneity and Representation: The Senate and Free Trade." *American Journal of Political Science* 42: 524-44.
- Bernhard, William, and Brian R. Sala. 2006. "The Remaking of an American Senate: The 17th Amendment and Ideological Responsiveness." *Journal of Politics*, forthcoming.
- Brunell, Thomas and Bernard Grofman. 1998. "Explaining Divided U.S. Senate Delegations, 1788-1996: A Realignment Approach." *American Political Science Review* 92: 391-99.
- Canes-Wrone, Brandice, David W. Brady, and John Cogan. 2002. "Out of Step, Out of Office: Electoral Accountability and House Members' Voting." *American Political Science Review* 96: 127-40.
- Carson, Jamie L. 2005. "Strategy, Selection, and Candidate Competition in U.S. House and Senate Elections." *Journal of Politics* 67: 1-28.
- Crook, Sara Brandes, and John Hibbing. 1997. "A Not So Distant Mirror: The Senate and Congressional Change." *American Political Science Review* 91: 845-53.
- Ellis, Susan, and Ronald F. King. 1999. "Inter-Party Advantage and Intra-Party Diversity: A Response to Wirls." *Studies in American Political Development* 13: 31-45.
- Engstom, Erik and Samuel Kernell. 2003. "The Effect of Presidential Elections on Party Control of the Senate under Direct and Indirect Elections." Presented at the 2003 History of Congress Conference, University of California, San Diego.
- Fenno, Richard. 1978. *Home Style: House Members in their Districts*. Little, Brown.

- Goff, Brian, and Kevin Grier. 1993. "On the (Mis)Measurement of Ideology and Legislator Shirking." *Public Choice* 76: 5-20.
- Haynes, George H. 1906. *The Election of Senators*. New York: Henry Holt.
- Haynes, George H. 1938. *The Senate of the United States*. New York: Russell & Russell.
- King, Ronald F., and Susan Ellis. 1996. "Partisan Advantage and Constitutional Change: The Case of the Seventeenth Amendment." *Studies in American Political Development* 10: 69-102.
- Lapinski, John S. 2000. *Representation and Reform: A Congress Centered Approach to American Political Development*. Ph.D. Dissertation, Columbia University.
- Lapinski, John S. 2003. "The Behavioral and Institutional Effects of Direct Election." Paper presented at the Annual Meeting of the American Political Science Association, Philadelphia, PA.
- Levitt, Steven D. 1996. "How Do Senators Vote? Disentangling the Role of Voter Preferences, Party Affiliation, and Senator Ideology." *American Economic Review* 86: 425-41.
- McCarty, Nolan M., and Keith T. Poole. 1995. "Veto Power and Legislation: An Empirical Analysis of Executive and Legislative Bargaining from 1961-1986." *Journal of Law, Economics, and Organization* 11: 282-312.
- McCarty, Nolan M., Keith T. Poole, and Howard Rosenthal. 1997. *Income Inequality and the Realignment of American Politics*. Washington: AEI Press.
- Meinke, Scott R. 2005. "Institutional Change and Position Taking in the Senate: The Impact of the Seventeenth Amendment." Paper presented at the Annual Meeting of the Midwest Political Science Association, Chicago, IL.
- Pearson, Rick. 2004. "Keyes Says Game Plan is Controversy." *Chicago Tribune*, September 14, 2004.
- Pierce, Greg. 2004. "Inside Politics." *Washington Times*, April 30, 2004.
- Poole, Keith T., and Howard Rosenthal. 1984. "The Polarization of American Politics." *Journal of Politics* 46: 1061-79.
- Poole, Keith T., and Howard Rosenthal. 1997. *Congress: A Political Economic History of Roll Call Voting*. New York: Oxford University Press.

- Poole, Keith T., and Howard Rosenthal. 2001. "D-NOMINATE After 10 Years: An Update to *Congress: A Political-Economic History of Roll Call Voting*." *Legislative Studies Quarterly* 26: 5-29.
- Riker, William. 1955. "The Senate and American Federalism." *American Political Science Review* 49: 452-69.
- Rogers, Lindsay. 1926. *The American Senate*. New York: Knopf.
- Schiller, Wendy. 2000. *Partners and Rivals: Representation in U.S. Senate Delegations*. Princeton: Princeton University Press.
- Snyder, James, and Michael Ting. 2002. "Roll Calls, Party Labels, and Elections." *Political Analysis* 11: 419-44.
- Stewart, Charles, III. 1992. "Responsiveness in the Upper Chamber: The Constitution and the Institutional Development of the Senate." In Peter F. Nardulli, ed., *The Constitution and American Political Development: An Institutional Perspective*. Urbana, IL: University of Illinois Press.
- Wawro, Gregory, and Eric Schickler. 2005. "Obstruction and Institutional Change." Paper presented at the Annual Meeting of the Midwest Political Science Association, Chicago, IL.
- Wirls, Daniel. 1999a. "Regionalism, Rotten Boroughs, Race, and Realignment: The Seventeenth Amendment and the Politics of Representation." *Studies in American Political Development* 13: 1-30.
- Wirls, Daniel. 1999b. "Beyond Bias: A Rejoinder to Ellis and King." *Studies in American Political Development* 13: 46-49.

Figure 1. State ideology and average delegation behavior, 1872-2004.

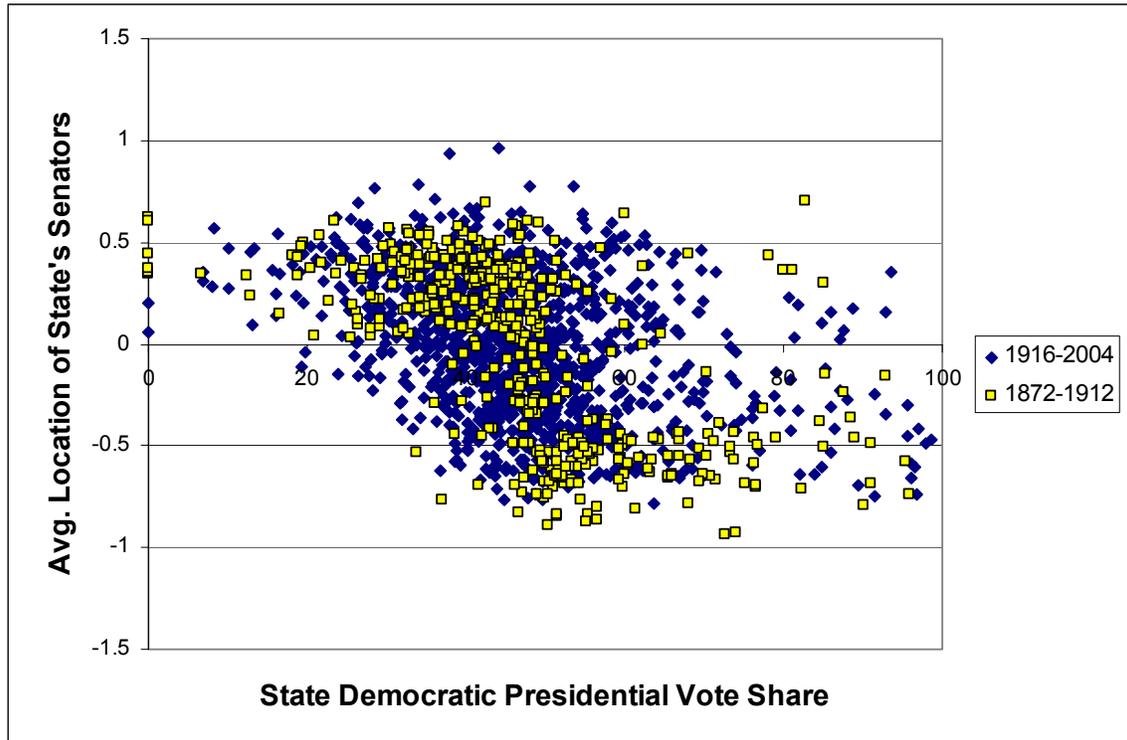


Figure 2. Predicted average delegation behavior and state ideology, before (steeper curve) and after (flatter curve) 17th Amendment.

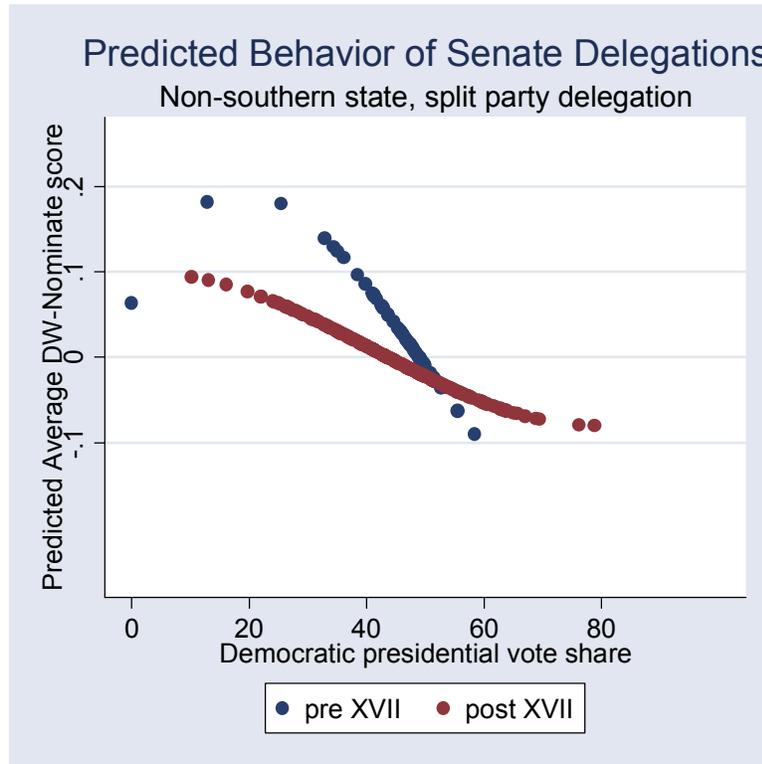


Figure 3. State ideology and within-delegation distance, 1872-2004.

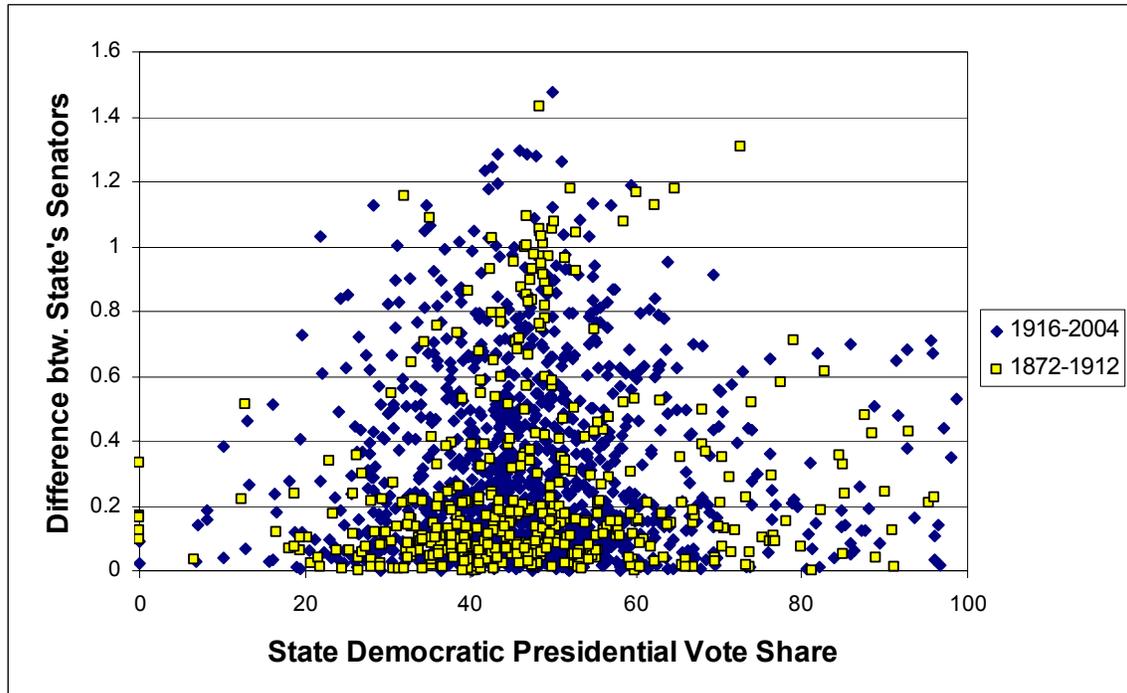


Figure 4. Polarization of U.S. Senate Delegations, 1872-2004.

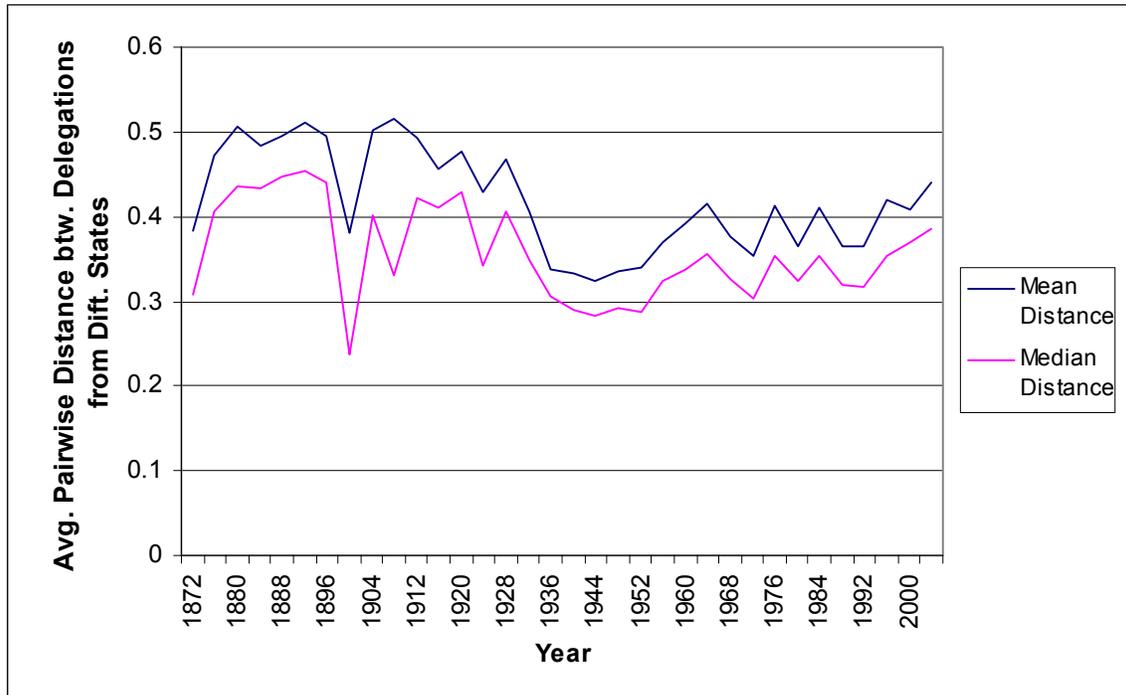


Table 1. Second stage GLS results: average delegation behavior.

Dependent variable: average location of state's Senate delegation, by state-year		
Explanatory Variable	Coefficient Estimate	p-value
State Dem. pres. vote share	0.021	< 0.001
State Dem. pres. vote share squared	-0.00079	< 0.001
State Dem. pres. vote share cubed	0.00000572	< 0.001
17th Amendment indicator	-0.137	0.034
Interact., 17th Amendment and state Dem. vote share	-0.009	0.040
Interact., 17th Amendment and state Dem. vote share squared	0.00037	0.001
Interact., 17th Amendment and state Dem. vote share cubed	-0.00000281	0.001
South indicator	-0.018	0.576
LPM residual, unified Republican delegation	0.295	< 0.001
LPM residual, unified Democratic delegation	-0.294	< 0.001
Constant	0.173	< 0.001

N = 1574; years 1872 – 2004; log likelihood = 865.29; $\chi^2 = 2202.81$, p-value < 0.0001

Table 2. Second stage GLS results: within-delegation distances.

Dependent variable: natural log of within-delegation distance, by state-year		
Explanatory Variable	Coefficient Estimate	p-value
State Dem. pres. vote share	0.0155	0.043
State Dem. pres. vote share squared	-0.000124	0.066
17th Amendment indicator	0.249	0.001
South indicator	-0.079	0.360
LPM residual, split-party Senate delegation	1.377	< 0.001
Constant	-2.28	< 0.001

N = 1574; years 1872 – 2004; log likelihood = -2189.81; $\chi^2 = 646.09$, p-value < 0.0001

Table 3. OLS results: Senate polarization.

Dependent variable: average pairwise distance between state delegations, by year

Explanatory Variable	Coefficient Estimate	p-value
17th Amendment Indicator	-0.045	0.033
Variance in state Dem. vote share	0.0000052	0.517
Party polarization, House	0.264	0.001
Constant	0.005	0.996

N = 32; adj. R² = 0.65; F = 17.00, p-value = 0.002
