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The Responsiveness of Direct and Indirect Elections

Previous research argues the Seventeenth Amendment made Senate elections more responsive. To make this claim, existing work compares the vote-seat relationships of direct and indirect elections before and after the Seventeenth Amendment. I argue this approach is problematic because it does not account for regional variation and compares elections from different time periods using presidential instead of Senate vote. I overcome these problems by simulating indirect elections using state legislatures' partisan compositions to evaluate the responsiveness of direct and indirect elections after the Seventeenth Amendment. With this counterfactual approach, my findings suggest direct elections are not necessary for electoral responsiveness.

Almost a century after the ratification of the Seventeenth Amendment, the direct elections of senators has again become a subject of American political conversation. Presidential candidates, a Supreme Court justice, and even directly elected senators have recently called for the repeal of the Seventeenth Amendment. Former Governor Mike Huckabee said its ratification was "one of the dumbest things we ever did" (Huckabee 2009). The Seventeenth Amendment even became a campaign issue in the 2010 elections. Many members of the Tea Party movement sought the Seventeenth Amendment's repeal, leading the Democratic Senatorial Campaign Committee to air what is likely the first television ad defending the direct elections of senators.

Despite contemporary commentary, the Seventeenth Amendment was a landmark accomplishment for direct democracy advocates of the Progressive Era. Progressives hoped direct elections would correct the unresponsive Senate through the redemptive powers of democracy (Hoebeke 1995; Riker 1955; Rossum 2001). Recently, there has been a surge in interest amongst political scientists to identify the effects of the Seventeenth Amendment on legislative behavior (Bernhard and Sala 2006; Gailmard and Jenkins 2009; Meinke 2008; Romero 2007; Schiller 2007; Wawro and Schickler 2006). There is comparatively less research investigating how direct elections affected the electoral responsiveness of

Senate elections. This is surprising. The Seventeenth Amendment fundamentally changed an electoral institution and established a direct connection between votes and seats. Studies of electoral institutions often consider the sensitivity of the votes-seats relationship to reflect an institution's responsiveness, but few explore this relationship when comparing direct and indirect elections.

The electoral research focused on the Seventeenth Amendment generally suggests that direct elections are more responsive to the electorate than indirect elections. Crook and Hibbing argue that direct elections made the Senate "[react] to the popular mood with more sensitivity and more rapidity" (1997, 852). Engstrom and Kernell likewise show that indirect elections were "less responsive" than direct elections (2007, 40). These works rely on similar research designs to compare the responsiveness of direct and indirect elections. Following the practice of House election analyses, both estimate vote-seat relationships or swing ratios to capture responsiveness.[1] They compare the relationships between presidential vote and Senate election outcomes before and after the ratification of the Seventeenth Amendment. With this approach, previous research has found direct elections to be more responsive, but the vote measure and comparisons of different time periods may limit what these analyses can tell us about the impact of the Seventeenth Amendment on electoral outcomes.

To better investigate the electoral responsiveness of direct and indirect elections, I utilize a different research design. Instead of using observed indirect elections before the Seventeenth Amendment, I simulate counterfactual indirect elections since 1914 as if state legislatures still selected U.S. senators. By assuming the majority party in the state legislature would have indirectly elected a senator of its own party, I can compare the vote-seat relationships of these simulated indirect election outcomes to their factual counterparts over the same time period.

Using this counterfactual approach, I find evidence suggesting previous research overstates the impact direct elections had on the electoral responsiveness of Senate elections. While I confirm direct elections produce outcomes more sensitive to voters' partisan Senate preferences nationwide, this increase in responsiveness is concentrated in southern Senate elections. There is little evidence that direct elections are more electorally responsive than indirect elections outside this region. In the non-South, the vote-seat relationships of the two electoral institutions are surprisingly indistinguishable, suggesting the Seventeenth Amendment had little impact. This finding challenges both the conventional wisdom, as well as Progressives' intentions in advocating direct Senate elections.

### Existing Research and Significance of Regional Variation

To assess the responsiveness of electoral institutions, political scientists often estimate vote-seat relationships or swing ratios (Cox and Katz 2002; King and Gelman 1991; Tufte 1973). Swing ratios capture how sensitive electoral outcomes are to votes. In elections with higher swing ratios, fewer votes are needed to change the same number of seats. These elections are more responsive to the electorate's partisan preferences. Previous analyses of direct and indirect elections often compare vote-seat relationships before and after the Seventeenth Amendment. The consensus is that direct elections are more electorally responsive than indirect elections, but the research is limited in several respects. Most notably, prior work relied on presidential vote and examined limited time periods, such as those including a solidly Democratic South.

A key limitation of using vote-seat relationships to study the impact of the Seventeenth Amendment is that Senate election returns before 1914 generally do not exist. As a substitute for partisan preferences, presidential vote has been popular because it is available across both the pre- and postratification periods (Crook and Hibbing 1997; Engstrom and Kernell 2007; King and Ellis 1996).[2] Despite its availability, presidential vote is not an ideal measure for congressional analysis. Its usage assumes that voters are consistent in their partisan preferences for presidents and senators. Employing presidential vote is also restrictive in studies of congressional elections because it is only available once every four years, meaning it fails to capture interelection swings in state partisan leanings.[3]

This limitation is apparent in Crook and Hibbing's work. They use a historical swing ratio to measure the relationship between the changes in presidential vote and Senate seats. These comparisons were across the four-year intervals between presidential elections despite senators being elected to six-year terms in classes representing different states. Therefore, Crook and Hibbing's measure of responsiveness never compares a Senate class to itself. Presidential vote is also unavailable for midterm elections, so when examining the time period from 1916 until 1964, their analysis included 12 congressional elections instead of 24.

Prior research also primarily focuses on elections during the first half of the twentieth century. Crook and Hibbing's analysis is the most extensive but still ignored elections after 1964. Engstrom and Kernell measure the responsiveness of indirect elections over 74 years outside the South but only examine direct elections from 1916 to 1940. A shift in the distribution of vote-seat relationships during this short time period may have influenced their estimates. Democrats never received more than

FIGURE 1
Southern Distribution of Votes, Seats, and Legislative Chambers
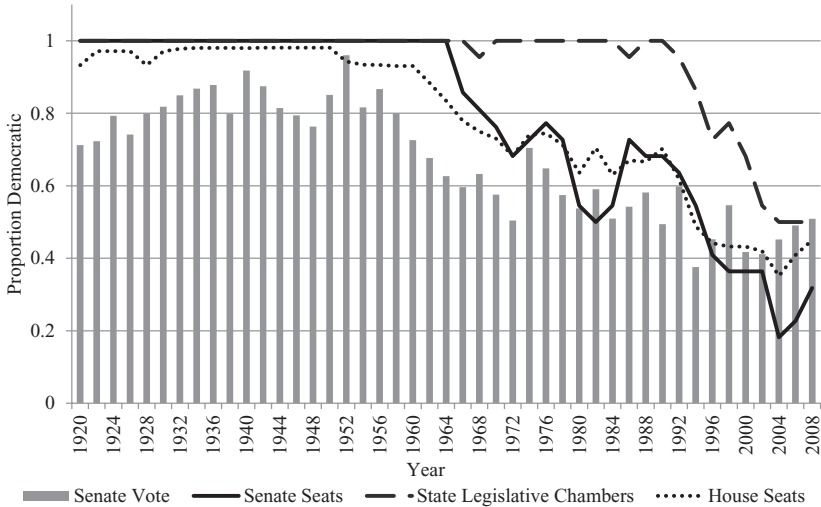
Note: Lines indicate proportion of seats or chambers controlled by southern Democrats during general elections from 1920 to 2008. Bars show the share of southern Senate vote earned by Democrats in each election year.

40.1% of the nonsouthern presidential vote in the 1920s, but in the 1930s, they never received less than 56.7%. Despite this dramatic shift in vote, Democratic Senate seat shares did not proportionally follow. Nonlinear swing-ratio estimations help alleviate this problem, but the small number of observations over this time period may still be cause for concern.

By considering only presidential election years, Engstrom and Kernell's analysis includes only seven elections after the Seventeenth Amendment. They compare this sample to 19 indirect election observations. The authors find direct and indirect elections to have swing ratios of 3.25 and 2.21 respectively. To balance the number of direct and indirect election observations, I extend Engstrom and Kernell's analysis to include direct elections until 1988.[4] When both sets of elections have 19 observations, the nonsouthern direct election swing ratio falls to 2.47, and the difference between direct and indirect election swing ratios drops from 1.04 to .26. This suggests the levels of responsiveness between the two types of electoral institutions are closer than previously measured.

Another implication of using a limited time period is that the South was solidly Democratic in the first half of the twentieth century. Figure 1 illustrates southern electoral trends following the Seventeenth

Amendment. In this region, Republicans did not make noticeable gains in the U.S. House until the 1950s. The pattern of electing Republicans to the Senate started later in the 1960s with the election of senators such as Strom Thurmond. Not until the early 1980s would a southern state's Senate delegation be solidly Republican. Republican takeover of southern state legislatures, however, did not begin until the 1990s. It is important to recognize Republicans did not control these institutions until recently given the legislature's role in indirect elections without the Seventeenth Amendment.[5]

Nonsouthern legislatures were considerably more likely to experience changes in majority party. Since the Seventeenth Amendment, there have been over 500 majority party changes in nonsouthern state legislative chambers. In the South, there have been less than 30, most of which occurred recently. Given this Democratic dominance and infrequent changes in majority, southern state legislatures may have indirectly elected Democrats to the Senate despite voters' federal preferences for Republicans. By directly tying voters to the allocation of seats, the Seventeenth Amendment reduced southern Democratic state legislators' influence, allowing Senate elections to more directly respond to the partisan preferences of the electorate.

By not accounting for regional differences and studying narrow time periods, previous research misassesses the effect of the Seventeenth Amendment. To overcome these shortcomings and investigate the Seventeenth Amendment's impact on electoral responsiveness, this work uses a counterfactual research design to test two hypotheses. The first addresses the relative responsiveness of direct and indirect elections. Following earlier findings, I expect direct senatorial elections to be more electorally responsive than indirect elections as indicated by swing ratios. The second hypothesis is built from the changing southern electoral trends and investigates the effect of direct elections in the South. I expect the establishment of direct elections to have a different effect on the swing ratios of nonsouthern and southern Senate elections due to their ability to respond to changes in the southern electorate.

A Counterfactual Senate

To test these hypotheses and explore variations in electoral responsiveness, I use a counterfactual research design. Counterfactuals are speculative, but they can uncover interesting historical insights. Stewart and Weingast (1992), for example, show how different nineteenth-century statehood-admission strategies could have altered party control of Congress and the White House. Some counterfactuals have been used

to study the Seventeenth Amendment (Ellis and King 1999; Stewart and Schiller 2011; Walling 2005). In this analysis, I simulate counterfactual indirect elections since 1914 as if the state legislatures still controlled Senate elections. With this approach, I identify the electoral effect of the Seventeenth Amendment by comparing factual direct election and counterfactual indirect-election outcomes.

An advantage of the counterfactual approach is that Senate election returns are available for the entire time period studied. Thus, my analysis is not constrained by the use of presidential votes. Also, by using the Senate vote in a particular year, my vote measure captures only one class of states preferences. Both Crook and Hibbing's and Engstrom and Kernell's measures of vote include states that did not cast ballots for senators in a given election.

To create the counterfactual, indirectly elected Senates, I assume that the majority party in the state legislature would have indirectly elected a senator of their own party. While not always the case before the Seventeenth Amendment, this "party loyalty" assumption has empirical and legal founding. The correlation between party-loyalty assumption outcomes and factual senatorial appointments from 1872 to 1912 is .885.[6] A federal law also simplifies how to account for inter- and intra-state legislative party disagreements regarding Senate nominees. To help address deadlocks, Congress passed *An Act to Regulate the Times and Manner of Holding Elections for Senators in Congress* requiring legislators to convene daily in a joint assembly until they directly elected a senator.[7] For the party-loyalty assumption, I therefore designate the majority party as the one who would have held the most seats in this joint assembly.

Even with empirical and legal foundations, assumptions limit my counterfactual approach. By focusing on the votes-seats relationship, my analysis sheds less light on how the Seventeenth Amendment influenced senators' legislative responsiveness or behavior than other recent research (Bernhard and Sala 2006; Gailmard and Jenkins 2009; Meinke 2008). The party-loyalty assumption only predicts the partisanship of an indirectly elected senator. It does not predict which Democrat or Republican would have been nominated. Senate nominees of the same party often had different policy positions leading to intraparty conflict over whom to indirectly elect (Schiller, Stewart, and Xiong 2011). These disagreements resulted in delays, deadlocks, and sometimes complete lack of representation. From 1901 to 1903, Republicans controlled both of Delaware's legislative chambers but failed to indirectly elect a senator during this time, denying the state any representation in the Senate (Haynes 1906). The party-loyalty assumption, however, does not account

for these disagreements within the legislative parties and assumes the immediate appointment of a senator of the majority party.

There are additional limitations to the party-loyalty assumption. First, not all state legislatures are partisan. Minnesota's legislature was nonpartisan from 1914 to 1974, and Nebraska's state legislature has been nonpartisan since 1936. Therefore I omit these states from the analysis during these time periods. Second, between 1914 and 2008 there were 18 ties within state legislatures when putting chambers into a simulated joint assembly. Since the party-loyalty assumption cannot be applied to these legislatures, I also omit these observations.[8] Third, over 30 states before the Seventeenth Amendment instituted some form of direct primaries or the "Oregon System" (Lapinski 2004). As the focus of this work is the comparison of direct and indirect elections, I assume that all states maintained indirect elections. Furthermore, due to circumstances that cannot be accounted for under simulated indirect elections such as party switches, deaths, or resignations, both directly and indirectly elected senators serve their full six-year term with the same party in this study.

My analysis additionally makes two key assumptions regarding voters. By using partisan Senate vote to capture senatorial preferences after the Seventeenth Amendment, I assume a strong party effect within the electorate's voting decisions. While party is generally most important, voters may base their decisions on nonpartisan reasons such as candidates' positions on issues, demographic variables, or perceived viability (Abramowitz and Segal 1992; Kahn and Kenney 1999). Therefore, votes cast under direct elections are not necessarily strictly partisan.

I also assume the adoption of the Seventeenth Amendment did not affect voter behavior in state legislative elections.[9] Senate elections often were focal points in these contests, and presumptive Senate nominees sometimes publicly canvassed on behalf of their party's state legislative candidates or contributed to their campaigns (Riker 1955; Rothman 1966). The most famous indirect Senate election featured direct appeals by Abraham Lincoln and Stephen Douglas to Illinois' state legislative voters. Under indirect elections, votes for state legislators therefore could be influenced by individuals' preferences over Senate candidates. Since state legislators no longer select senators, voters' senatorial preferences should be less influential in state legislative elections. The Seventeenth Amendment therefore weakened the connection between state legislative and U.S. Senate election outcomes, and my counterfactual indirect elections should be less responsive to voters' federal preferences.[10]

Following these assumptions, I generate directly and indirectly elected Senates as follows. For the directly elected Senate, state-level

Senate election returns determine the partisan control of a Senate seat. The party-loyalty assumption dictates seat control for the indirectly elected Senate. Both direct and indirect elections follow the factual, staggered Senate election schedule for general elections from 1914 to 2008. I primarily use three data sources to construct the factual and counterfactual Senates. Senate election returns for the two parties were mostly determined from the *CQ Voting and Elections Collection* (Congressional Quarterly Press 2010).[11] State legislatures' partisan make-ups were taken from *Party Affiliations in the State Legislatures* (Dubin 2007) or the National Conference of State Legislatures (2009).

Before measuring the electoral responsiveness of the two types of institutions, attention should be brought to differences between the directly and indirectly elected Senates. The institution of direct elections potentially changed almost 400 individual elections. The Seventeenth Amendment therefore had a considerable electoral impact. Table 1 identifies these differences in results. Montana had the most inconsistent direct and indirect election outcomes with 15 partisan changes. The impact of the Seventeenth Amendment, however, was not regionally uniform. Only one election outcome changed in Arkansas or Louisiana, and there were no differences in the South before the 1960s.
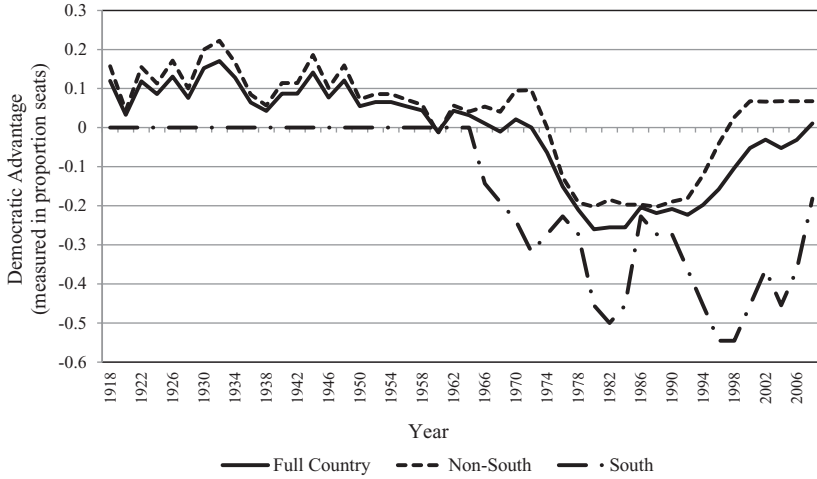
In terms of partisan control of the Senate, the majority party under indirect elections would have been different for 17 congresses.[12] As compared to the directly elected Senates, there are 13 additional instances where the indirectly elected majority party could have invoked cloture without minority party members. Figure 2 illustrates the advantage or disadvantage created for Democrats with the establishment of direct elections. Through the 92nd Congress, there was generally a Democratic bias in seat allocation in the directly elected Senate. Elections outside the South created this advantage. For the first half of the twentieth century, southern states would have likely elected Democrats with or without the Seventeenth Amendment. This Democratic bias is consistent with the findings of other research, which does not examine any elections after 1950 (Engstrom and Kernell 2007; King and Ellis 1996; Stewart and Schiller 2011).

Looking at the full time period, there generally has been a Republican bias under direct elections since the 1970s. The advantage enjoyed by Republicans reached greater levels during the 96th to 104th Congresses than those ever achieved by Democrats. Even when omitting the Democratic South, a Republican advantage appears in the directly elected Senates until the 1998 elections. In the South, direct elections continually favor Republicans through 2008.

TABLE 1

Elections with Different Direct and Indirect Election Outcomes

(D or R represents state-years where a Democrat or Republican was uniquely directly elected)

Note: The above indicates when states directly elected a senator of a different party than that predicted by the party loyalty assumption. 391 general election outcomes differed, but none in the South until 1966. The bottom three rows indicate the Democratic seat bias for an individual election year for the full country and by region.

FIGURE 2
Democratic Advantage in the Senate
Created by the Institution of Direct Elections

Note: Democratic advantage is the proportion of seats held by Democrats under counterfactual indirect elections subtracted from the proportion of seats held by Democrats under direct elections. The figure presents these proportional advantages for the full Senate along with the nonsouthern and southern delegations.

The Responsiveness of Direct and Indirect Elections

Similar to previous research, I assess the electoral responsiveness of direct and indirect elections by comparing vote-seat relationships or swing ratios. Political scientists commonly employ these measures to examine House elections, but considerably less attention has been devoted to developing swing ratios to study the Senate. The practice therefore in Senate analysis has been to borrow models from the House literature. Of those focusing on Senate electoral responsiveness, Crook and Hibbing (1997) and Pothler (1984) use historical swing ratios; Alford and Hibbing (2002) use hypothetical swing ratios; Engstrom and Kernell (2007) use a modified version of a swing ratio developed by Tufte (1973); and Stewart (1992) uses a model developed by Ansolabehere, Brady, and Fiorina (1988).[13]

Research focused on the Seventeenth Amendment only compares separately estimated swing ratios of direct and indirect elections. Crook and Hibbing find the swing ratios of direct and indirect elections respectively to be 1.09 and .75. For Engstrom and Kernell, they are 3.25 and

2.21. From these measures, the works conclude that direct elections are more responsive than indirect elections. I similarly estimate separate swing ratios for direct and indirect elections, but my analysis also compares the electoral responsiveness of direct and indirect elections within a single estimation. With equation (1), I test the first hypothesis by simultaneously estimating the swing ratios of direct and indirect elections using a measure of seats, votes, and a dummy variable for direct elections.

$$SEATS = \beta_0 + \beta_1 VOTES + \beta_2 DIRECT + \beta_3 [DIRECT \cdot VOTES] \qquad (1)$$

The dependent variable is the proportion of seats won by Democrats. For every election year, there is an observation for both a directly and indirectly elected Senate class. Therefore, in the analysis, there are two Senate classes for each of the 48 elections from 1914 to 2008, producing 96 observations. The measure of votes is the same across both observations, but a dummy variable indicates whether direct or indirect elections determined the Democratic seat share. The coefficient on this dummy variable reflects the Democratic bias of direct elections.

Through the interaction term in equation (1), β_3 captures the difference in responsiveness between the electoral institutions. Prior comparisons of direct and indirect elections do not discuss the uncertainty of the responsiveness differences. In my analysis, the standard error of the interaction term's coefficient reflects this uncertainty. To provide evidence for the consensus that direct elections are more responsive than indirect elections, β_3 should be positive and statistically significant.

I conduct a similar analysis to test the second hypothesis, which predicts that the effect of direct elections will be different in the South. Instead of measuring the vote-seat relationship for the full country, I disaggregate votes and seats regionally. For each election year, there are South and non-South observations for both directly and indirectly elected Senate classes. The coefficient on the interaction of "Measure of Votes" and "Direct" again indicates the change in responsiveness attributable to direct elections.

Following the presentation of earlier research, Table 2 compares separately estimated direct and indirect election swing ratios. Consistent with previous findings, direct elections have larger swing ratios than indirect elections. The magnitude of swing ratios for direct Senate elections is even greater than that for U.S. House elections.[14] Indirect senate elections meanwhile are considerably less responsive than either direct elections or House elections. Swing ratios for indirect elections are less

TABLE 2
Swing Ratios of Direct and Indirect Elections, 1914–2008
(robust standard errors in parentheses)

Level of Analysis	Directly Elected Senate Class	Indirectly Elected Senate Class
Measure of Votes	2.00*	.866*
	(.255)	(.426)
Constant	−.534*	.098*
	(.141)	(.238)
R-Squared	.517	.074
N	48	48

*$p \leq .05$.

TABLE 3
Estimates of Swing Ratios and Regional Differences
for Direct and Indirect Elections
(robust standard errors in parentheses)

Region	Full Country	South	Non-South	Difference between South and Non-South
Measure of Votes	.867*	.463*	3.97*	−3.51*
	(.426)	(.131)	(.384)	(.406)
Measure of Vote × Direct	1.13*	.983*	−.359	1.34*
	(.497)	(.171)	(.486)	(.516)
Direct	−.631*	−.857*	.211	−1.07*
	(.277)	(.144)	(.248)	(.287)
Constant	.098	.611*	−1.51*	2.12*
	(.238)	(.110)	(.198)	(.226)
R-Squared	.270	.725	.693	
N	96	96	96	

*$p \leq .05$.

than half the magnitude of those for their direct counterparts, which serves as evidence for the first hypothesis.

To further investigate the differences between direct and indirect elections, the first column of Table 3 presents estimates of equation (1) for the full country. Again evidence is found supporting the existing consensus. The coefficient on "Measure of Vote × Direct" is positive and statistically distinguishable from zero, indicating direct elections produce larger swing ratios than indirect elections. This estimate suggests direct elections are more responsive than indirect elections

when studying the full country. The estimations of bias, however, conflict with previous findings. Reflecting the Republican advantages displayed in Figure 2, the negative coefficient on the "Direct" dummy variable suggests a Republican bias attributable to direct elections. By excluding most of the second half of the twentieth century, previous research did not capture this change.

To consider regional differences, Table 3 presents southern and nonsouthern swing-ratio estimates for equation (1). While direct elections seemed to produce more responsive elections for the full country, these regional estimates suggest that this increased responsiveness is primarily attributable to the institution of direct elections in the South. The Seventeenth Amendment effectively tripled the magnitude of southern Senate swing ratios. Paralleling nationwide trends, southern direct elections are more electorally responsive than House elections, but indirect elections are not. Southern direct elections additionally seem responsible for the aforementioned Republican bias illustrated by Figure 2. In the beginning of the second half of the twentieth century, Democratic senators continued to be indirectly elected even though southerners preferred Republicans.

Despite these substantial effects in the South, the institution of direct elections had little if any impact on the electoral responsiveness of Senate elections outside this region. The difference in swing ratios for direct and indirect elections is in the unexpected direction and statistically indistinguishable from zero. Unlike trends observed in the South, both direct and indirect Senate elections produce larger nonsouthern swing ratios than House elections. This implies there likely would have been a great deal of electoral responsiveness in this region with or without the Seventeenth Amendment and serves as evidence against the first hypothesis.

Consistent with the second hypothesis, the effect of the Seventeenth Amendment and direct elections varied for the South and non-South. The final column of Table 3 presents the responsiveness differences between the regions. The estimated difference between southern and nonsouthern direct election swing ratios is 1.34 and statistically distinguishable from zero. This suggests the increased responsiveness found in the full country is concentrated in the South. Regional findings are not sensitive to using a Senate or gubernatorial vote measure, and the appendix presents similar results when using the Tufte and historical swing-ratio models.[15] Estimations therefore using both linear and nonlinear swing-ratio models suggest there seems to have been relatively little direct impact of the Amendment outside the South—at least in terms of electoral responsiveness.[16]

Previous research contributed to the consensus that direct elections are more responsive than indirect elections. When examining the full country, this and prior work provide evidence for the existing argument. My findings, however, demonstrate that regional considerations are critical to evaluating the electoral effects of the Seventeenth Amendment. When estimating swing ratios by region, the proportion of variation explained by the models increased from .270 to at least .693, and I discover little difference between direct and indirect elections in the non-South.

While the Seventeenth Amendment may not be necessary to produce more responsive Senate elections, direct elections can encourage responsiveness. My regional estimations suggest southern Senate elections were considerably more responsive under direct elections. By directly tying southern voters' Republican federal preferences to seat outcomes, the Amendment helped overcome the southern federal- and state-level voting disconnect. This disconnect did not exist in the non-South, so the Seventeenth Amendment was not necessary for responsive Senate elections in this region.

Conclusion

By expanding the time period analyzed and disaggregating Senate elections regionally, a more accurate account of the impact of the Seventeenth Amendment and the institution of direct elections emerges. Temporal trends influence interpretations of the impact of the Seventeenth Amendment. Isolating narrow time periods affected previous studies' estimation of the differential electoral impact of direct and indirect elections. Direct elections did seem to cause Senate elections to "[react] to the popular mood with more sensitivity and more rapidity" as characterized by Crook and Hibbing (1997, 852). Still, tests of my second hypothesis imply that this increased sensitivity was yet another unique aspect of the South.

Without the Seventeenth Amendment, changing voting patterns may not have translated into different election outcomes. For example, South Carolina voters wanted Strom Thurmond to represent them as a Republican, likely influencing his 1964 party switch, but this desire could have been thwarted by indirect elections and their Democratic state legislature. More recently Senators Richard Shelby (AL) and Ben Nighthorse (CO) left the Democratic Party following the Republican successes in the 1994 elections. With impending direct elections, these senators may have feared shifting voting patterns in their states. Both Alabama and Colorado supported Bob Dole in the 1996 presidential election, and

both states reelected their now Republican senators in 1998. During these times, control of their state legislature never changed partisan hands.

Tea Partiers and Governor Huckabee may consider the ratification of the Seventeenth Amendment to be "one of the dumbest things we ever did," but they probably appreciate Republican senators such as Tea Party leader Rand Paul serving in Washington today despite their Democratic state legislatures. By directly linking votes to seats, the Seventeenth Amendment allowed election outcomes to be more responsive to preferences. Even though the institution of direct elections increased the responsiveness of southern elections, there were no evident changes in the levels of electoral responsiveness for nonsouthern elections. These findings imply the Seventeenth Amendment is not necessary to create responsive Senate elections.

The southern findings presented here may be a result of the region's peculiar political history. This history appears to be changing course. The trend of southerners electing Democrats at high rates to their state legislatures while sending Republicans to Washington may be ending. In 2008 alone, more southern Republicans would have been indirectly elected than had been from 1914 to 1998. Voting for federal and state legislators is becoming more consistent in the South as it is in the non-South, where direct elections seemed to have little impact. If the South continues to mirror other regions, the efforts of direct democracy and Seventeenth Amendment advocates seeking increased responsiveness may ultimately have been for naught—at least along the electoral dimensions of representation studied here. Only future work including more election observations can adequately address whether this is the case and if the Seventeenth Amendment is required for responsive elections across the country.

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APPENDIX
TABLE A.1
Democratic Proportions of Directly Elected
and Indirectly Elected Senates

Year	Congress	Directly Elected Senate	Indirectly Elected Senate	Year	Congress	Directly Elected Senate	Indirectly Elected Senate
1919	**66**	**0.52**	**0.40**	1965	89	0.71	0.68
1921	67	0.39	0.36	1967	90	0.64	0.63
1923	68	0.45	0.33	1969	91	0.59	0.60
1925	69	0.43	0.34	1971	92	0.55	0.54
1927	70	0.50	0.37	1973	93	0.57	0.58
1929	71	0.41	0.34	1975	94	0.63	0.70
1931	72	0.49	0.34	1977	95	0.63	0.77
1933	**73**	**0.63**	**0.46**	1979	96	0.60	0.80
1935	*74*	*0.74*	*0.62*	***1981***	***97***	***0.48***	***0.73***
1937	75	0.80	0.74	***1983***	***98***	***0.45***	***0.70***
1939	76	0.69	0.70	***1985***	***99***	***0.47***	***0.72***
1941	*77*	*0.72*	*0.63*	*1987*	*100*	*0.55*	*0.76*
1943	78	0.63	0.53	*1989*	*101*	*0.55*	*0.77*
1945	**79**	**0.63**	**0.48**	*1991*	*102*	*0.56*	*0.77*
1947	**80**	**0.52**	**0.43**	*1993*	*103*	*0.56*	*0.79*
1949	**81**	**0.58**	**0.46**	*1995*	*104*	*0.50*	*0.70*
1951	**82**	**0.52**	**0.46**	***1997***	***105***	***0.46***	***0.62***
1953	**83**	**0.53**	**0.46**	1999	106	0.45	0.56
1955	**84**	**0.52**	**0.46**	2001	107	0.50	0.55
1957	**85**	**0.54**	**0.49**	2003	108	0.49	0.53
1959	*86*	*0.67*	*0.63*	2005	109	0.45	0.50
1961	*87*	*0.65*	*0.66*	2007	110	0.50	0.52
1963	88	0.70	0.66	2009	111	0.58	0.56

Note: Only Senates since 1919 are presented since this was the first year with a fully directly elected Senate. Changes in majority are **bolded**, and changes in cloture power are *italicized*.

Below are specifications for the measures used in swing-ratio models for this article and the appendix.

ABF Swing Ratios

$$\text{Measure of Votes} = \text{VOTES} = V = \frac{1}{m} \cdot \sum_{i=1}^{m} D_i$$

D_i = Proportion of Democrat vote in state i
m = Number of states with Senate elections
Measure of Seats = SEATS = S = Proportion of seats won by party

Tufte Swing Ratios

With above specifications of V and S:

Measure of Vote: $\log\left(\dfrac{V}{1-V}\right)$

Measure of Seats: $\log\left(\dfrac{S}{1-S}\right)$

Following Grofman and Brunell (1997), I estimate Tufte swing ratios using OLS equations similar to: $\log\left(\dfrac{S}{1-S}\right) = \log\alpha + \beta\log\left(\dfrac{V}{1-V}\right)$

Historical Swing Ratios

Measure of Vote:

$(\text{Proportion Democratic Senate Vote})_t - (\text{Proportion Democratic Senate Vote})_{t-6}$

Measure of Seats:

$(\text{Proportion Seats won by Democrats})_t - (\text{Proportion Seats won by Democrats})_{t-6}$

$$t = \text{Year}$$

To estimate regional differences in responsiveness, Equation 2 introduces regional interactions and controls.

$$
\begin{aligned}
SEATS = {} & \beta_0 + \beta_1 VOTES + \beta_2 DIRECT + \beta_3[VOTES \cdot DIRECT] \\
& + \beta_4 SOUTH + \beta_5[VOTES \cdot SOUTH] \\
& + \beta_6[DIRECT \cdot SOUTH] + \beta_7[VOTES \cdot DIRECT \cdot SOUTH]
\end{aligned}
\tag{2}
$$

Seats, votes, and direct are the same as equation (1). South is a dummy variable distinguishing between the nonsouthern and southern Senate delegations. In both equations (1) and (2), the coefficients on "Measure of Votes" and related interaction terms are swing-ratio estimates. β_1 indicates the baseline swing ratio for both direct and indirect elections. In equation (2), β_3 captures the effect of direct elections common both to the non-South and South, and β_7 isolates the effect the Seventeenth Amendment had on southern swing ratios. These swing ratios indicate levels of electoral responsiveness for direct and indirect elections in different regions. The other variables and interactions serve as controls or potential indicators of bias. Table A.2 presents estimates of these regional differences and biases using each swing-ratio model discussed.

For equation (1), there are two observations for each election year. There is an observation for each directly and indirectly elected Senate class each year. Therefore, in the analysis there are two Senate classes for each of the 48 elections from 1914 to 2008, producing 96 observations. Similarly for equation (2), there are four observations per

election year. For each year, there is an observation for the directly elected non-South, indirectly elected non-South, directly elected South, and indirectly elected South. Having four observations per election year produces 192 observations. Since Democrats sometimes regionally won or lost all elections in a given year, $\log\left(\dfrac{S}{1-S}\right)$ at times was undefined. Similar to the omission of single-district states in House election analyses, these observations were dropped from Tufte estimations. As an informal robustness check in separate analyses, I set S equal to .05 or .95 in situations where S equaled zero or one. These values are more extreme than any nonzero or non-one observed values of Democratic seat share and give defined estimates of $\log\left(\dfrac{S}{1-S}\right)$. Even with these changes, the findings resemble those in Table A.2.

Similarly affecting the historical swing ratio, Democratic dominance in southern state legislatures created little variation in the early twentieth century. Table A.2 therefore provides swing-ratio estimates for both the full time period and 1964–2008.

TABLE A.2
Estimations of Equations 1 and 2 for ABF, Tufte, and Historical Swing-Ratio Models
Using Senate Vote
(robust standard errors in parentheses)

Model:	ABF		Tufte		Historical			
Election Years:	1914–2008		1914–2008		1914–2008		1964–2008	
Equation Estimated:	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Measure of Votes	.867*	3.96*	.975*	4.80*	1.95*	2.95*	1.57*	3.27*
	(.426)	(.384)	(.471)	(.481)	(.354)	(.528)	(.499)	(.983)
Direct Election × Measure of Votes	1.13*	−.359	1.18*	−.925	.551*	.613	.741*	−1.24
	(.497)	(.486)	(.551)	(.634)	(.096)	(.628)	(.120)	(1.24)
South × Measure of Votes	—	−3.51*	—	−3.72*	—	−3.02*	—	−3.55*
		(.406)		(1.01)		(.502)		(1.02)
South × Direct Election × Measure of Votes	—	1.34*	—	2.30*	—	.430	—	3.44*
		(.516)		(1.14)		(.715)		(1.36)
Direct Election Dummy	−.631*	.211	−.298	.168	−.009	−.009	.010	.013
	(.277)	(.248)	(.170)	(.105)	(.021)	(.032)	(.028)	(.045)
South Dummy	—	2.12*	—	1.15*	—	−.039	—	−.056
		(.226)		(.368)		(.030)		(.049)
South × Direct Election	—	−1.07*	—	−1.55*	—	−.019	—	.038
		(.287)		(.391)		(.043)		(.071)
Constant	.098	−1.51*	.146	−.137	.005	.004	−.012	−.019
	(.238)	(.198)	(.149)	(.088)	(.019)	(.026)	(.026)	(.037)
R–Squared	.266	.842	.263	.674	.724	.468	.612	.355
N	96	192	96	122	90	180	46	92

*$p \leq .05$.

Table A.3 presents estimates of equations (1) and (2) using gubernatorial instead of Senate vote. Main findings are similar, but differing Senate and gubernatorial election calendars force the omission of some regional observations. For example, no southern state elected both a governor and senator in 1976, 1988, and 2000. With the historical swing-ratio vote measure, varying election calendars additionally force comparisons of different states when accounting for the six-year term of a senator.

TABLE A.3
Estimations of Equations 1 and 2 for ABF, Tufte, and Historical Swing-Ratio Models
Using Gubernatorial Vote
(robust standard errors in parentheses)

Model	ABF		Tufte		Historical			
Election Years	1914–2008		1914–2008		1914–2008		1964–2008	
Equation Estimated	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Measure of Votes	1.76*	3.27*	1.92*	4.02*	1.17*	2.41*	.875	1.76*
	(.422)	(.564)	(.467)	(.819)	(.398)	(.579)	(.793)	(.838)
Direct Election ×	.339	−.607	.081	−1.18	.831*	.295	.930*	−.020
Measure of Votes	(.573)	(.650)	(.634)	(1.01)	(.065)	(.762)	(.069)	(1.20)
South ×	—	−3.05*	—	−10.2*	—	−2.27*	—	−1.21
Measure of Votes		(.578)		(2.94)		(.586)		(.904)
South ×	—	1.76*	—	7.34*	—	−.635	—	−1.59
Direct Election ×		(.708)		(3.12)		(.803)		(1.71)
Measure of Votes								
Direct Election	−.160	.366	.053	.397*	−.016	−.002	−.003	.013
Dummy	(.309)	(.315)	(.167)	(.178)	(.032)	(.050)	(.054)	(.085)
South Dummy	—	2.01*	—	.892*	—	−.006	—	.032
		(.291)		(.409)		(.036)		(.068)
South ×	—	−1.36*	—	−.998	—	−.039	—	−.174
Direct Election		(.393)		(.569)		(.075)		(.189)
Constant	−.420	−1.20*	−.171	−.392*	.015	.009	−.003	−.004
	(.230)	(.272)	(.140)	(.142)	(.031)	(.033)	(.053)	(.057)
R-Squared	.281	.716	.234	.468	.598	.224	.564	.117
N	96	186	95	107	90	180	46	80

*$p \leq .05$.

NOTES

1. Crook and Hibbing do not refer to their measure as a swing ratio, but it is calculated the same as a historical swing ratio. Swing ratios better assess electoral responsiveness than legislative responsiveness. Other Senate swing-ratio literature has

come to relatively consistent conclusions regarding the electoral responsiveness of Senate elections. Stewart argues that following the Seventeenth Amendment the Senate had actually "become at least as 'democratic' as the House" (1992, 79). Also using comparisons to House elections, Alford and Hibbing find that "the House is now no more sensitive to the public mood than the Senate" (2002, 104). Erikson, Mackuen, and Stimson find "far greater responsiveness to national forces in Senate elections than the House" (2002, 278) using nonvote-seat measures of responsiveness.

2. Stewart and Schiller (2011) use Ansolabehere and Snyder's (2002, 2010) measure of mass partisanship to study the impact of direct elections on partisan bias rather than responsiveness.

3. An alternative to this presidential measure is gubernatorial vote. Election returns for Governor contests had a stronger correlation with state legislative seat shares than presidential vote before the Seventeenth Amendment, .81 versus .73. Gubernatorial election returns therefore may better capture state-level preferences under indirect elections. With this measure, the votes-seats relationships under indirect and direct elections from 1880 to 1944 resemble those found by Crook and Hibbing or Engstrom and Kernell. These estimates are available upon request. Gubernatorial vote, however, presents limitations similar to those of presidential vote, such as differing election calendars.

4. I thank Erik Engstrom for explaining his method and providing the necessary software to replicate it.

5. One explanation for the federal- and state-level voting disconnect is the "Republican top-down advancement" theory where the policies and platforms presented by the national Republican party differed with those from the state level starting in the 1960s (Aistrup 1996). With the national parties' change in strategy and the rise of candidate centered campaigns, Republican Senate candidates in the South possibly established themselves more independently of the state party. Southern voters may have been attracted to Republican candidates in federal elections but remained loyal to the state-level Democratic party, which would be consistent with the "dual partisan identification" hypothesis (Hadley 1985). However, there is disagreement concerning this disconnect, with some suggesting that Republican gains come from the "bottom-up" (Aldrich 2000). Resolving this debate is beyond the scope of this project, but regardless of the reasons behind the divergence, there was a clear disconnect between southern federal- and state-level voting.

6. This result is similar to Walling (2005, 45). Most states never violated the party-loyalty assumption, and approximately a fourth of violations are attributable to vacancies or Populist party appointments during the Progressive Era. Looking at a larger sample from 1840 to 1912, Engstrom and Kernell find "[w]hen Democrats controlled both branches they elected a fellow Democrat 93 percent of the time, and Republican controlled legislatures elected a Republican Senator in 97 percent of the contests" (2003, 13). Although these relationships are imperfect, the likelihood that the majority party of a state legislature would appoint a senator of the same party is high, suggesting the "party loyalty" assumption is reasonable.

To account for potential violations of the assumption, I additionally simulate counterfactual indirect elections using two alternative approaches. In the first, I assume Democratic and Republican controlled legislatures were respectively "party loyal" 94.5% and 93.9% of the time, as they were from 1872 to 1912. In the second, I

estimate the factual indirect election outcomes from 1880 to 1912 as a function of the state legislature majority party, the majority party's margin, and averaged statewide presidential, gubernatorial, and U.S. House vote share. I use these estimates to generate a state-year specific predicted probability of indirectly electing a Democratic senator for each Senate election from 1914 to 2008. Substantive results presented in Tables 2 and 3 do not change when using the probabilities generated by either approach to indirectly elect senators.

7. After this law's passage in 1866, there was an even greater likelihood of the party-loyalty assumption being accurate (Engstrom and Kernell 2007).

8. If I omit a state-year observation for either the directly or indirectly elected Senates, I omit it for both. Including ties and forecasting half Democratic and Republican senators in these scenarios does not change the substantive results.

9. To check this assumption, I examine the relationship between a state's presidential vote and state legislative seat shares before and after the Seventeenth Amendment, comparing the 1900–12 and 1916–28 time periods. If the relationship between votes and lower chamber outcomes changes, it suggests the institution of direct elections affected state legislative elections. I, however, discover no significant difference before and after the Seventeenth Amendment, even when controlling for ballot types, region, and non-November elections or using statewide U.S. House vote instead of presidential vote (Dubin 2007; Ludington 1911).

10. I generate simulated state legislatures that account for voters' senatorial preferences to assess how sensitive my results are to this assumption. These simulations weight voters' preferences over U.S. senators and the actual state legislative election outcomes using the formula α[Senate Vote] + $(1 - \alpha)$[Factual State Legislature Partisan Composition] = [Simulated Partisan Composition] for each state-year observation. This weighted average accounts for both voters' federal and state government preferences, reflecting decision making in state legislative elections before the Seventeenth Amendment. For higher values of α, voters' preferences over senators more strongly determine the partisan control of the simulated state legislature and the ultimate indirect election outcome. With these simulated legislatures, I estimate swing ratios for direct and counterfactual indirect elections where α ranges from 0 to 1. Direct elections are more responsive than indirect elections when α is respectively less than or equal to .16, .58, or 0 for the full country, South, or non-South. If the assumption that the Seventeenth Amendment did not affect state legislative election outcomes is unreasonable, my southern findings overstate the increases in electoral responsiveness attributable to direct elections.

11. In the CQ collection, the 1960 and 1966 Arkansas Senate election returns were unavailable, so I omit these.

12. Democratic seat shares of the directly and indirectly elected Senates are available in the appendix.

13. The estimates presented in the main text here use Ansolabehere, Brady, and Fiorina's (ABF) model. Details and estimates for this as well as the historical and Tufte swing ratios are in the appendix.

14. This is consistent with the class level findings of Stewart (1992, 78). House election swing ratios for the full country, non-South, and South during this time period respectively are 1.50, 2.27, and 1.18.

15. To estimate differences and their uncertainty, equation (1) was expanded to include regional controls and interactions. See equation (2) and accompanying discussion in the appendix for details.

16. In bivariate regressions similar to those in Table 2, one could interpret the R^2 as another measure of responsiveness. It captures the proportion of variation in seats explained only by votes. With this interpretation, the Seventeenth Amendment was most influential in the South. In bivariate regressions only using southern states, the R^2 for direct elections was .782 and .336 for indirect elections. The difference was much smaller in the non-South where the proportions of explained variation for direct and indirect elections were .780 and .631.

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