

The Southern Seventeenth Amendment Swing

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Abstract: Crook & Hibbing (1997) and others argue that the Seventeenth Amendment and the institution of the direct election of Senators have made Senate elections more responsive. Previous research has compared direct and indirect elections by examining elections before and after the Seventeenth Amendment. I argue this approach is problematic because it examines inconsistent time periods and relies on presidential instead of Senate election vote in its swing ratio measures. Instead this work simulates counterfactual, indirectly elections since 1914 and uses Senate election returns to compare the responsiveness of direct and indirect elections after the Seventeenth Amendment. Similar to previous research, direct elections are found to be more responsive, but a central finding is that this increased responsiveness is largely attributable to elections in the south.

The two chambers of Congress were intended to be elected differently. The House of Representatives has been directly elected for its entire history, but it was not until the 1914 that all Senators were elected by the people. At the time of the United States' founding, individuals argued that indirect elections would "refin[e] the popular appointments by successive filtrations" through the state legislatures.¹ By removing these filters, the ratification of the Seventeenth Amendment caused a significant institutional change. Recently there has been a surge in interest to investigate how the institution of direct elections affected Senators' legislative behavior (Gailmard & Jenkins 2009; Meinke 2007; Romero 2007; Schiller 2007; Bernhard & Sala 2006; Wawro & Schickler 2006). However less attention has been given to addressing how and if the Seventeenth Amendment affected the electoral responsiveness of Senate elections.

Within the research focused on the Seventeenth Amendment, consensus has formed around the finding that direct elections are more responsive than indirect elections. For example, Crook and Hibbing (1997) argued that direct elections have made the Senate react "to the popular mood with more sensitivity and more rapidity." Engstrom and Kernell (2007) likewise asserted that indirect elections were "less responsive" than direct elections. These works relied on similar research designs. Both compared Senate elections before and after the ratification of the Seventeenth Amendment and used swing ratios as measures of responsiveness, following the practice of House election analysis (Tufté 1973; King & Gelman 1991; Ansolabehere, Brady, Fiorina 1992; Cox & Katz 2002).² However this approach may have limited these authors' findings by forcing their analyses to consider inconsistent time periods and rely on a proxy

¹ The Debates in the Federal Convention of 1787 as reported by James Madison: May 31.

² Crook and Hibbing do not refer to their measure as a swing ratio, but it is calculated the same as a historical swing ratio. Other Senate swing ratio literature has produced relatively consistent findings regarding the responsiveness of Senate elections. Stewart (1992) argued that following the Seventeenth Amendment the Senate had actually "become at least as 'democratic' as the House." Also using comparisons to House elections, Alford and Hibbing (2002) found that "the House is now no more sensitive to the public mood than the Senate."

measure of vote. To address whether direct elections are more responsive than indirect elections, I aim to avoid the complications of prior research by utilizing a different research design.

Previous research measured the responsiveness of indirect elections by examining elections before the Seventeenth Amendment. Instead of observing indirect elections from this time period, I simulate counterfactual indirect elections since 1914 as if Senate elections were controlled by the state legislatures. These election outcomes can then be compared to their factual counterparts over the same time period. Examining a consistent time period after the Seventeenth Amendment allows me to overcome some of the problems encountered by previous research when testing the hypothesis that direct elections are more responsive than indirect elections. Using this counterfactual approach and the tests employed in prior work, direct elections are still found to be more responsive than indirect elections nationwide when separately comparing swing ratios. However when regional variables are included in stricter tests of the hypothesis, it is discovered that the increased levels of responsiveness are concentrated amongst southern Senate elections. Meanwhile in the nonsouth, little evidence is found to support the argument that direct elections are more electorally responsive than indirect elections.

To explore and assess the differences in responsiveness between direct and indirect elections, I will do the following. First, some of the problems of prior works' research designs are discussed. I then explain the methodology used for conducting simulated, indirect elections since 1914. This is followed by a presentation of the differences between the compositions of the factual and counterfactual Senates. Two hypotheses concerning the responsiveness of direct and indirect elections are offered and tested using modified models of swing ratios. The findings of these tests are presented and discussed before concluding remarks.

Problems in Previous Research

The most common research design previously used to study the responsiveness of direct and indirect elections has been to compare swing ratios for Senate elections before and after the ratification of the Seventeenth Amendment. One shortcoming of this approach is that a measure of voters' preferences is unavailable for Senate elections before 1914. Previous analyses have therefore been forced to rely on a proxy measure of preferences. Using presidential vote has been popular because it is available across both the pre and post ratification periods (King & Ellis 1996; Crook & Hibbing 1997; Engstrom & Kernell 2007). Despite presidential vote's availability, it is not an ideal measure of voter preferences in Senate elections. Its usage relies on the assumption that voters are consistent in their partisan preferences for President and Senatorial representation. However in congressional election analysis, using presidential vote is limiting because it is only available once every four years.

This limitation is exemplified in Crook and Hibbing's work. They used 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. However Senators are elected to six year terms in classes representing different states. Therefore Crook and Hibbing's historical swing ratio never compared a class to itself, which could introduce error into their responsiveness measure. Another implication of using presidential vote is that it is unavailable for midterm elections, so when examining the time period from 1916 until 1964, Crook and Hibbing only used 12 elections in their estimations instead of 24.

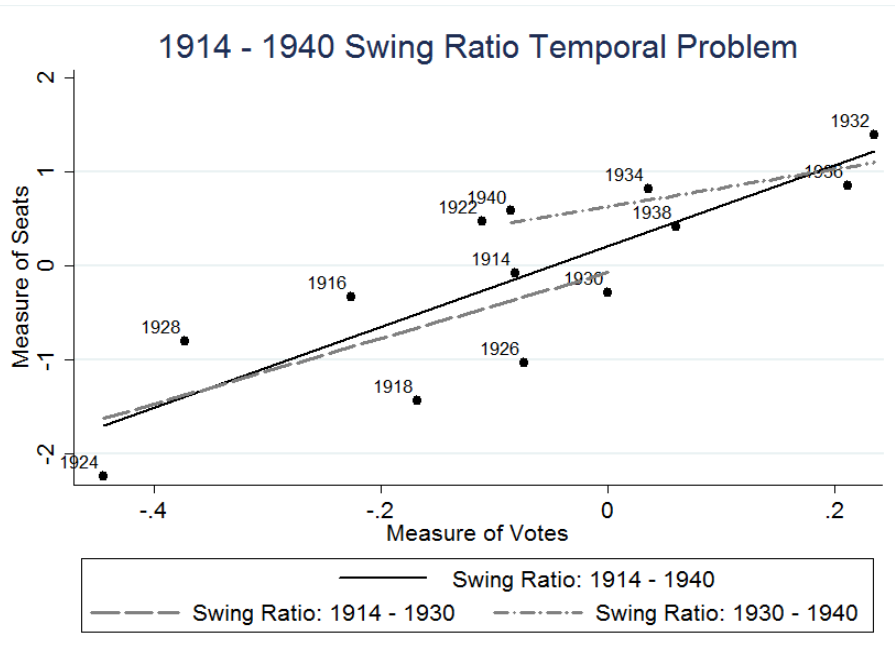
Earlier research has also excluded observations by predominantly focusing on elections during the first half of the twentieth century. Crook and Hibbing's analysis has been the most extensive but still ignored elections from 1966 - 1996. Engstrom and Kernell measured the

responsiveness of nonsouth indirect elections over 74 years but only looked at a 26 year period of direct elections. When considering midterm election omissions, this limited their analysis to seven direct election observations, but more importantly, these occurred during a unique time in American history, which included the Great Depression and the notable 1932 election.

Swing ratios during the short time period analyzed by Engstrom and Kernell may have been influenced by a temporal shift in the distribution of the relationships between votes and seats. Following the 1932 election, Democrats consistently received larger proportions of the vote, but their maintained gains did not continually translate into more seats. The slopes of the shorter lines in Figure 1 represent the vote-seat relationships or swing ratios for nonsouthern elections from 1914 – 1930 and 1932 – 1940 (Tuftte 1973). From 1932 - 1940, the swing ratio was 2.01, falling from 3.48 during 1914 - 1930. Also as shown in Figure 1, the elections in the later time period are distributed differently than the earlier time period. Due to the temporal shift, both of these swing ratios are smaller than that for the whole time period, which was 4.28.³ By observing so few elections and combining these two time periods, Engstrom and Kernell's direct election swing ratio was likely influenced by this temporal shift. When examining a 92 instead of 26 year period, the swing ratio fell from 4.28 to 3.29.

³ These swing ratios were calculated using senate vote. Engstrom and Kernell used presidential vote.

Figure 1

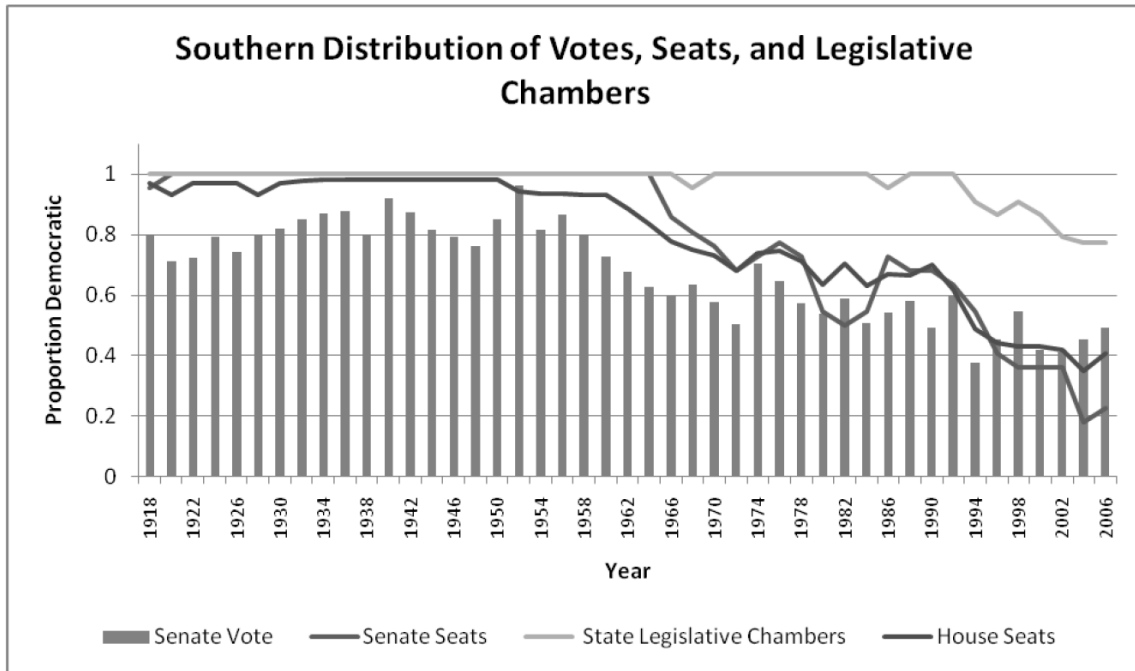


Another implication of using this time period is that the observed elections only occurred when the south was solidly Democratic. There was only one southern Republican Senator during this time frame, and he was elected in 1914. Additionally as shown by Figure 2, southern federal legislative seats began to trend Republican starting in the 1960s. However the state legislatures continued to be dominated by Democrats, suggesting a difference between federal and state level election voting starting in this decade.

Southern state legislatures also differ from their northern counterparts. Since the ratification of the Seventeenth Amendment, there have been less than twenty-five majority party changes within southern state legislative chambers, and most of these have occurred recently. Meanwhile in the nonsouth, there have been over five hundred. Despite the partisan divergence in federal and state level election outcomes and differences between the northern and southern state legislatures, no previous research on direct and indirect elections has specifically examined the effect of the Seventeenth Amendment on southern Senate elections. Before the Seventeenth Amendment, the state legislatures were Senators' electorate, and these differences between the

nonsouth and south could have significant implications if the Senate still was indirectly elected. Therefore these regional differences could be an interesting part of the Seventeenth Amendment story.

Figure 2



Research Design: Counterfactual Approach

I aim to avoid the oversights and complications of previous analyses through a counterfactual research design. Counterfactuals are speculative, but they can uncover interesting historical insights. An excellent example being Stewart and Weingast’s (1992) work showing how different nineteenth century, statehood admission strategies could have altered which party controlled the Presidency and Congress. Some counterfactuals have already been used in the Seventeenth Amendment literature (Ellis & King 1999; Walling 2005).⁴ In this project,

⁴ Briefly discussed in Ellis and King’s *Inter-Party Advantage and Intra-Party Diversity* was a counterfactual Senate created by the authors. They applied the coefficients on state legislature proportions from the pre amendment period onto the proportions from the post amendment period “to predict the counterfactual party composition of each class elected to the Senate, as if indirect election had remained in force.” They also executed the reverse procedure to the indirect election period. From 1887-1913, seven of fifteen Senates changed majorities. While there was only one change from 1915 to 1953, many Democratic majorities were weakened. The authors believed that this supported

counterfactual, indirect elections are simulated since 1914 as if Senate elections were controlled by the state legislatures. By using this approach, the electoral effect of the Seventeenth Amendment is identified by comparing the factual direct election and counterfactual indirect election outcomes. While counterfactual outcomes can be valuable in identifying the effects of institutions, it should be emphasized the assumptions used to generate counterfactuals limit how findings can be generalized.

An advantage of the counterfactual approach is that it allows for the estimation of swing ratios using the actual Senate election returns instead of a proxy measure of preferences. This helps overcome the aforementioned problems of using presidential vote such as assumed partisan consistency and ignored midterm elections. Also by using Senate vote in a particular year, only one class of states' preferences are captured. Some previous work has used the nationwide presidential vote despite only one third of the states casting ballots for Senators in a given election.

Again, the assumptions used in counterfactual research limit the approaches' generalizability. For example, this project assumes a strong party effect within the electorate's voting decisions. Voters may cast ballots for Senators for non-partisan reasons such as candidates' policy positions, demographic variables, or personality characteristics. Therefore votes cast under direct elections are not necessarily strictly partisan. It is also assumed that outcomes of state legislative elections were unaffected by the adoption of the Seventeenth Amendment. Individuals may have voted differently in state legislative elections if the legislature still appointed Senators.

their thesis that indirect elections created biased Republican results. See Ellis and King, p. 39. Unfortunately, no specific, counterfactual Senate compositions were provided by the authors upon request.

The primary assumption used to create the counterfactual, indirectly elected Senates supposes 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.

In addition to the face validity that a Democratic legislature would appoint a Democrat to the Senate, there is empirical evidence supportive of the party loyalty assumption. From 1840 to 1912, “[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” (Engstrom & Kernell 2003). Walling found the correlation between state legislature control and Senator appointments to be .88 from 1870 to 1914. While these relationships are imperfect, the probability that the majority party of a state legislature would appoint a Senator of the same party is high.

From a legal perspective, a federal law passed in 1866 simplifies how to account for state governments with divided legislatures. Hoping to prevent gridlocks, Congress passed *An Act to Regulate the Times and Manner of Holding Elections for Senators in Congress*. It required for state legislators to convene in a joint assembly in circumstances where the two legislative chambers could not agree on a Senatorial appointment. All legislators would then vote each day of session until a candidate received a majority.⁵ After the 1866 law there was an even greater likelihood of party loyalty assumption being true (Engstrom & Kernell 2007).

There are limitations to the party loyalty assumption. For example, not all state legislatures are partisan. Minnesota’s state legislature was non-partisan from 1914 to 1948, and Nebraska’s legislature has been non-partisan since 1936. Therefore these states are omitted from

⁵ *An Act to Regulate the Times and Manner of Holding Elections for Senators in Congress* in *Select Statutes and Other Documents Illustrative of the History of the United States, 1861-1898* By William MacDonald: 152.

the analysis during these time periods. Between 1914 and 2006, there were 18 ties within state legislatures when they were put into a simulated joint assembly. Since the party loyalty assumption cannot be applied to these legislatures, these observations were also omitted.⁶ Also some states before the Seventeenth Amendment instituted direct primaries or the “Oregon System.”⁷ As a focus of this work is the comparison of direct and indirect elections, it is assumed that all states maintained indirect elections.

It is also unknown how long it would have taken state legislatures to indirectly elect a Senator. In my counterfactual elections, I assume that parties would agree on their selections immediately and appoint a Senator of the majority party, but this was not always the case under indirect elections before the Seventeenth Amendment. For example Delaware went from 1901 to 1903 without any representation in the Senate. Therefore counterfactual, indirect elections under the party loyalty assumption operate much more smoothly than the actual indirect elections before 1914. Due to circumstances that cannot be accounted for under indirect elections such as party switches, deaths, or resignations, it is assumed that both directly or indirectly elected Senators would serve their full six year term with the same party.

Following these assumptions and omissions, directly and indirectly elected Senates were generated using the following criteria. The factual class election schedule was used for both, and only general elections from 1914 to 2006 were considered. For the directly elected Senate, partisan control of a seat was determined by the factual, state-level election returns. For the indirectly elected Senate, seat control was determined by the assumptions described above. Two

⁶ These eighteen ties are West Virginia (1916), Delaware (1916, 1988, 2006), Missouri (1926), Nevada (1926), Rhode Island (1946), Pennsylvania (1958), Alaska (1972), Arizona (1974, 2000), Indiana (1976), Utah (1976), Montana (1988), Iowa (1992), Ohio (1992), Michigan (1996), and Oregon (2004). Both the direct and indirect elections in these states in these years were omitted.

⁷ Crook (1992) p. 192. Riker (1955) notes that 28 states had primary elections laws that “provided in one way or another for the nomination of party candidates for the Senate at the party primary” (p. 466).

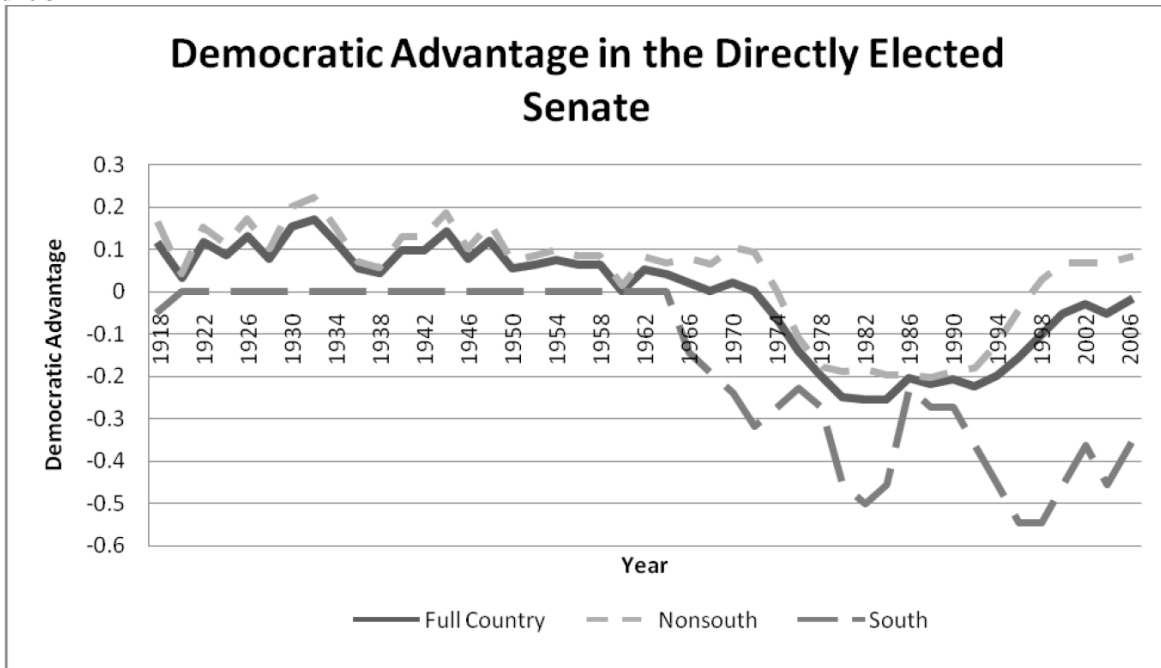
data sources were primarily used to construct the factual and counterfactual Senates. Senate election returns for the two parties were mostly determined from the *CQ Voting and Elections Collection*.⁸ State legislatures' partisan make ups were taken from *Party Affiliations in the State Legislatures* (Dubin 2007).

The Democratic proportions of the directly and indirectly elected Senates are presented in Table A.1. Before measuring the two types' of elections levels of responsiveness, attention should be brought to the differences between the Senates. The majority party is different for 17 congresses, and there were six occurrences where a party would have uniquely achieved three fifths control under indirect elections. Figure 3 shows that until the 93rd Congress, there was generally a Democratic advantage in seat allocation in the directly elected Senate, but since the 94th Congress, there consistently has been a Republican bias. The advantage enjoyed by Republicans reached greater levels during the 96th to 104th Congresses than those ever achieved by the Democrats before the 93rd Congress.

Even when omitting the traditionally Democratic south, a similar Republican advantage appears in the directly elected Senate until the 1998 elections. Meanwhile in the south, direct elections seem to favor Republicans starting in the 1960s through 2006. This Republican advantage is inconsistent with the findings of previous work. King, Ellis, Engstrom, and Kernell's arguments regarding bias asserted that the institution of direct elections helped Democrats, but their samples ended in the 1940s. This is prior to the declines in Democratic advantage displayed in Figure 3, which started in the 1960s. Interestingly in this decade, the south directly elected its first Republican Senator in over fifty years.

⁸ Some data was missing from this collection. CQ lists the 1938 Alabama Special U.S. Senate election result but not the general election. The election result was found in Van der Veer Hamilton (1987). Additionally the 1960 and 1966 Arkansas Senate election returns were unavailable, so these were omitted.

Figure 3



The Responsiveness of Direct and Indirect Elections

Swing ratios can indicate how sensitive electoral outcomes are to votes. The translation of votes to seats is often considered to be a measure of responsiveness and has been frequently used in the analysis of House elections. Higher swing ratios signify increased levels of responsiveness. For example, a swing ratio of one generally indicates that a one percentage difference in vote results in a one percent change in seats won by a party. A swing ratio of two implies that a one percent difference in vote will result in a two percent change in seats. In elections with higher swing ratios, fewer votes are needed to change the same number of seats.

Considerably less attention has been devoted to developing swing ratios for studying Senate elections. Therefore the common practice has been to borrow models from the House literature. Of those focusing on the Senate, Crook, Hibbing (1997) and Pothler (1984) used historical swing ratios; Alford and Hibbing (2002) used hypothetical swing ratios; Engstrom and Kernell (2007) used a modified version of a swing ratio developed by Tufte; and Stewart used a model developed by Ansolabehere, Brady, and Fiorina (1989).

It should be noted that the lack of variation in southern Senate elections could contribute to many dropped observations or statistical significance problems when estimating some models. I sometimes encountered these difficulties. Similarly, Crook and Hibbing's historical swing ratios measuring indirect elections' responsiveness encounter statistical significance problems, and Engstrom and Kernell omitted southern elections, stating that "[i]ncluding this electorally isolated region...understates the responsiveness of the Senate." Also all House swing ratio models cannot be estimated for this project. For example, hypothetical swing ratios cannot be calculated for the counterfactual Senates as a one percent shift in actual vote will not affect the change in seats. Although versions of the other swing ratios used in the aforementioned Senate election analyses were estimated for this project.

The Ansolabahere, Brady, and Fiorina (ABF) model used in Stewart's work has been less popular in the House literature, but it may prove appropriate for Senate election analysis. Ansolabehere, Brady, and Fiorina were critical of swing ratios that assumed equality in district sizes. For their measure of votes, they proposed regressing "the average party proportion across constituencies" on the proportion of seats won by a party instead of using the aggregated proportion of national vote.⁹ By using the average of proportions, the model is not influenced by varying constituency sizes or voter turnout. This approach may be more appropriate for examining Senate contests to control for the state-level constituencies with varying voter populations. For example, the voters of California do not wash away the voters of Delaware when the states' proportions are averaged. Instead the states' voters are weighted equally, following the design of representation in the Senate.

⁹ Ansolabehere, p. 12. See Equation A.1 in appendix for their model. For reference, more detail regarding the measures within the historical and Tufte swing ratios used in this paper are provided in the appendix.

I use swing ratios to test two hypotheses. The first addresses prior findings regarding the relative responsiveness of direct and indirect elections. The second is built from the divergence of the Democratic advantage discussed above and seeks to investigate the effect of direct elections in the south. Specifically it looks to see if there were regional distinctions in the changes in responsiveness with the institution of direct elections.

Hypothesis 1

Direct senatorial elections are more responsive than indirect elections as indicated by swing ratios.

Hypothesis 2

The institution of direct elections will have a different effect on the swing ratios of nonsouthern and southern Senate elections.

Previous literature only compared separately estimated swing ratios of direct and indirect elections. Crook and Hibbing found the swing ratios of direct and indirect elections respectively to be 1.09 and .75. For Engstrom and Kernell, they were 3.25 and 2.21. From these measures, it was concluded that direct elections are more responsive than indirect elections. This work similarly presents separately estimated swing ratios for direct and indirect elections.

Additionally it collapses the data into a single model.

Equation 1

$$Y = \beta_0 + \beta_1 x_1 + \beta_2 x_1 x_2$$

Y = Measure of Seats

X₁ = Measure of Votes

X₂ = Dummy Variable for Direct Elections

Through interaction terms in Equation 1, the difference between the two types' of elections responsiveness can be identified. Estimating Equation 1 is a stricter test for the first hypothesis than a surface comparison of two swing ratios. To support the consensus of previous findings that direct elections are more responsive than indirect elections, β_2 should be positive and statistically significant.

The second hypothesis supposes that the effect of direct elections will be different regionally. This is tested using another regression model:

Equation 2

$$Y = \beta_0 + \beta_1 x_1 + \partial_1 x_2 + \partial_2 x_3 + \partial_3 x_2 x_3 + \beta_2 x_1 x_2 + \beta_3 x_1 x_3 + \beta_4 x_1 x_2 x_3$$

Y = Measure of Seats

X₁ = Measure of Votes

X₂ = Dummy Variable for Direct Elections

X₃ = Dummy Variable for Southern Elections

In both Equations 1 and 2, the coefficients on “Measures of Votes” and interaction terms including it are estimates of swing ratios. Therefore in both equations β_1 will indicate the baseline swing ratio for both direct and indirect elections. In Equation 2, β_2 will capture the effect of direct elections common both to the nonsouth and south, and β_4 will show the specific effect the Seventeenth Amendment had on southern swing ratios. The other variables and interactions serve as controls or potential indicators of bias.¹⁰

Swing ratios estimated using robust standard errors for direct and indirect elections across the United States are presented in Table 1. As comparisons of separate swing ratios of direct and indirect elections, these findings are generally consistent with those of previous research with direct election swing ratios being higher than those for indirect elections. However when performing stricter tests using Equation 1, statistically significant differences were only found for the ABF model (Table 2). The results of this particular test were in the expected direction indicating that direct elections produce higher swing ratios than indirect elections, but when using the other models, no statistically significant evidence was found to support the first hypothesis.

¹⁰ The general interest of this work is the comparison of direct and indirect elections. Therefore for clarity it will focus on the class level of analysis. All tests conducted at the class level are also conducted at the Senate level, and results are presented in the Appendix.

Table 1

MODEL	ABF		TUFTE		HISTORICAL	
Level of Analysis	Directly Elected Class	Indirectly Elected Class	Directly Elected Class	Indirectly Elected Class	Directly Elected Class	Indirectly Elected Class
Swing Ratio	2.12*** (.257)	.901** (.443)	3.10*** (.260)	2.96*** (.370)	3.20*** (.249)	2.70*** (.394)
Constant	-.596*** (.140)	.077 (.248)	.075 (.056)	.135 (.084)	-.007 (.015)	.002 (.018)
R-Squared	.564	.079	.675	.561	.740	.573
N	47	47	47	47	44	44

*** p < .001 ** p < .05 * p < .10

The second hypothesis expects for there to be a regional difference in the effect of direct elections. This is tested by estimating Equation 2, and the class level results for each swing ratio model are presented in Table 2. The key variable of interest is the interaction of “South, Direct Election, & Measure of Vote,” whose coefficient isolates the effect of direct elections in the south. The model also allows for the estimation of a common direct election effect across both regions through the “Direct Election & Measure of Vote” measure. Due to controls, this coefficient should indicate whether direct election affected responsiveness in the nonsouth.

The findings from the ABF and Tufte models support the second hypothesis. Following the estimations of Equation 2, a substantial portion of the swing ratio increases displayed in Table 1 can be attributed to the institution of direct elections in south. Under the Tufte model, direct elections increase southern swing ratios by 1.97. Under the ABF model and comparing Equation 1 and 2, the coefficient on the “Direct Election & Measure of Votes” interaction loses statistical significance once I control for the south. Therefore the Seventeenth Amendment may have had little, if any, effect on responsiveness of nonsouthern Senate elections.

Table 2

MODEL	ABF		TUFTE		HISTORICAL	
Level of Analysis	Class		Class		Class	
Equation Estimated	(1)	(2)	(1)	(2)	(1)	(2)
Measure of Votes	.901** (.443)	3.70*** (.373)	2.96*** (.370)	4.15*** (.412)	2.71*** (.394)	3.09*** (.554)
Direct Election Dummy	-.673** (.285)	.108 (.244)	-.060 (.102)	.207* (.122)	.010 (.024)	-.009 (.031)
Direct Election × Measure of Votes	1.21** (.512)	-.142 (.477)	.144 (.452)	-.860 (.589)	.488 (.466)	.670 (.644)
South Dummy	-	2.03*** (.222)	-	1.40*** (.352)	-	-.031 (.029)
South × Direct Election	-	-.996** (.285)	-	-1.64*** (.380)	-	.013 (.042)
South × Measure of Votes	-	-3.28** (.395)	-	-2.51*** (.896)	-	-3.11*** (.586)
South × Direct Election × Measure of Votes	-	1.17** (.509)	-	1.97* (1.094)	-	.292 (.732)
Constant	.077 (.248)	-1.38*** (.194)	.136 (.085)	-.236* (.101)	.002 (.018)	.000 (.025)
R- Squared	.291	.845	.548	.641	.660	.4845
N	94	188	94	119	88	176

*** p < .001 ** p < .05 * p < .10
Robust standard errors in parentheses.

It should be noted that lack of variation in the dependent variable for southern elections from 1914 through the 1960s likely caused difficulty in estimating the historical swing ratio. If the swing ratios are estimated for elections starting in 1964, the institution of direct elections increases the southern swing ratio by 3.58.¹¹ Meanwhile, the effect of direct elections in the nonsouth remains statistically insignificant. This is consistent with the second hypothesis that there was a regional difference in the effect of instituting directly elections. Therefore across all models a southern specific effect was found regarding the increased responsiveness of direct elections, and there seemed to be little effect in the nonsouth.¹²

¹¹ See Table A.4

¹² The other subject that has received attention in the Seventeenth Amendment electoral literature concerns the bias of direct elections. King and Ellis (1996) explored this subject in the greatest depth and argued that the institution of direct election contributed to a pro-Democratic bias in the allocation of Senate seats. Under their aggregated model looking at elections from 1887 until 1942 using presidential vote, they found the institution of direct elections

Relating these results to those of the previous research, little evidence is found to support the responsiveness conclusions of Engstrom and Kernell or Crook and Hibbing. When comparing separately estimated swing ratios as done in previous research, direct elections can be considered to be more responsive than indirect elections. However under stricter tests, less conclusive evidence is found. It is acknowledged that the null findings presented here do not necessarily refute the consensus that direct elections are more responsive than indirect elections. Although, the tests of my second hypothesis suggest that it is more appropriate to state that after the Seventeenth Amendment direct elections are more responsive than indirect elections in the south, but in the nonsouth there seems to be relatively little difference.

Discussion

By examining an extended time period, this work was able to discover the southern Seventeenth Amendment swing. It was not until the 1960s that southern Republican Senators began to be elected, and southern Democrats started a considerable losing trend across federal, legislative elections. Democrats did not encounter a similar decline at the state legislative level until the 1990s. During this time, there seems to be a disconnect between the partisan preferences of southerners for Senators and state legislators (Figure A.1). For example, Strom Thurmond was directly elected as a Republican in 1966, but the South Carolina state legislature was under Democratic control until 1994. Similarly, Texas Republican John Tower served in the Senate from 1966 to 1985, but Texas had a Democratic state legislature during this time. In these periods, South Carolina and Texas voters wanted Republican Senators, but under indirect

introduced a 14 percent Democratic bias in the allocation of Senate seats. Using the simulated directly and indirectly elected Senates from 1918 - 1942, King and Ellis' aggregated model is estimated using senate vote. A similar 7 percent pro-Democratic bias emerged. However when this model was estimated on elections after 1942, a pro-Republican bias of 10 percent was discovered, and across both time periods little overall bias was found (Table A.5). Therefore when examining the impact of direct elections over larger or different time periods produces different estimates of both responsiveness and bias.

elections, their state legislatures probably would have appointed Democrats. Following the 2006 elections fourteen southern state legislative chambers were controlled by Democrats, but these states only had three Democratic Senators. The institution of direct elections allowed southern Senate elections to be more responsive to voters' preferences in the second half of the twentieth century. However the trend of southerners electing Democrats at high rates to their state legislatures while sending Republicans to Washington may be changing. As shown by Figure A.1, in elections from 1968 – 2002 the southern federal and state legislative Democratic vote shares seem to be converging. This convergence may prove beneficial to southern Republican state legislators.

One explanation already offered in the literature for the federal and state level voting disconnect is the “Republican top-down advancement” theory where the policies and approaches presented by the national Republican party differed with those on the state level starting in the 1960s (Aistrup 1996). Perhaps with national parties' change in strategy and the rise of candidate centered campaigns southern, Republican Senate candidates were able to establish 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). If the differences in responsiveness discussed above are attributable to national forces, the findings presented here do not refute the assertions of Engstrom and Kernell or Crook and Hibbing who argued “that the 17th Amendment effectively nationalized Senate elections.”¹³ However, this work has shown that the increased responsiveness of direct elections found in previous analyses is likely attributable to changes in southern Senate elections. Therefore the “nationalization” of elections may have been felt strongest in this region.

¹³ Engstrom & Kernell (2007) p. 38.

Some of the differences between this work's findings and those of previous research obviously are attributable to the lack of regional disaggregation in earlier analyses. However, temporal trends likely played a significant role in findings of both this and previous work. Narrow time periods may have limited previous analyses from accurately measuring the responsiveness differences between direct and indirect elections in the United States, and by examining southern Senate elections after the 1960s, I was able to include the changing voting patterns amongst the southern electorate. By expanding the time period analyzed and disaggregating Senate elections regionally, a more accurate story regarding the impact of the Seventeenth Amendment and the institution of direct elections emerges. 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, but this increased sensitivity was largely concentrated in the south.

Appendix

Swing Ratio Models

Below are further specifications regarding the swing ratio models used in this paper.

ABF Swing Ratios:

Equation A. 1¹⁴

$$\beta = \frac{\sum (W_t - \bar{W})(S_t - \bar{S})}{\sum (W_t - \bar{W})^2}$$

$$W = \frac{1}{m} \cdot \sum D_i \text{ (the average party proportion across constituencies)}$$

β = Swing ratio

D_i = Proportion of democrat vote in district i

t = Time

m = Number of districts

S = Percent of seats won/held by party

For swing ratios in this paper,

Measure of Vote: W

Measure of Seats: S

Tufte Swing Ratios:

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

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

Using this model, V equals the proportion democratic Senate vote nationwide in an election year, and S equals the number of seats won by the Democratic party in that same election year. For Senate level swing ratios, S equals the proportion of seats held by Democrats following the election.

Following the work of Grofman and Brunell (1997), Tufte swing ratios were estimated using OLS equations similar to:

$$\log\left(\frac{S}{1-S}\right) = \log \alpha + \beta \log\left(\frac{V}{1-V}\right)$$

Historical Swing Ratios:

Measure of Vote (Class Level):

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

Measure of Vote (Senate Level):

$(\text{Proportion Democratic Senate vote Nationwide})_t - (\text{Proportion Democratic Senate vote Nationwide})_{t-2}$

Measure of Seats (Class Level):

$(\text{Proportion Elected Class Democratic})_t - (\text{Proportion Elected Class Democratic})_{t-6}$

Measure of Seats (Senate Level)

$(\text{Proportion Elected Class Democratic})_t - (\text{Proportion Elected Class Democratic})_{t-2}$

¹⁴ Ansolabehere, Stephen, David Brady, and Morris Fiorina. N.d. "Turnout and the Calculation of Swing Ratios." Stanford University Graduate School of Business, Research Paper Series, no. 990: 8.

Table A. 1 Democratic Proportions of Directly Elected and Indirectly Elected Senates¹⁵

Congress	Directly Elected Senate	Indirectly Elected Senate	Congress	Directly Elected Senate	Indirectly Elected Senate
66	0.51	0.40	88	0.71	0.66
67	0.39	0.36	89	0.72	0.68
68	0.45	0.33	90	0.65	0.63
69	0.43	0.34	91	0.60	0.60
70	0.50	0.37	92	0.56	0.54
71	0.41	0.34	93	0.58	0.58
72	0.49	0.34	94	0.64	0.70
73	0.63	0.46	95	0.64	0.78
74	0.73	0.62	96	0.60	0.80
75	0.80	0.74	97	0.48	0.73
76	0.74	0.70	98	0.45	0.70
77	0.73	0.63	99	0.47	0.72
78	<i>0.63</i>	<i>0.53</i>	<i>100</i>	<i>0.55</i>	<i>0.76</i>
79	0.62	0.48	<i>101</i>	<i>0.55</i>	<i>0.77</i>
80	0.51	0.43	<i>102</i>	<i>0.56</i>	<i>0.77</i>
81	0.58	0.46	<i>103</i>	<i>0.56</i>	<i>0.79</i>
82	0.52	0.46	<i>104</i>	<i>0.50</i>	<i>0.70</i>
83	0.53	0.46	105	0.46	0.62
84	0.52	0.45	106	0.45	0.56
85	0.54	0.48	107	0.50	0.55
86	0.68	0.61	108	0.49	0.53
87	0.66	0.66	109	0.45	0.50
			110	0.49	0.52

Table A. 2 – Replication of Table 1 for Senate Level

MODEL	ABF		TUFTE		HISTORICAL	
Level of Analysis	Directly Elected Senate	Indirectly Elected Senate	Directly Elected Senate	Indirectly Elected Senate	Directly Elected Senate	Indirectly Elected Senate
Swing Ratio	1.18***	-.043	1.35***	1.35**	.517***	.246*
Robust Standard Error	.203	.396	.261	.419	.125	.146
Constant	-.086	.597	.165	.227	-.001	.002
R-Squared	.401	.000	.302	.145	.235	.059

¹⁵ Only Senates since 1919 are presented since this was the first year with a fully directly elected Senate. To make it easier for readers to tell who has majority status or enough votes for cloture when considering the omissions and statehood of Alaska and Hawaii compositions are presented as proportions. Changes in majority are bolded and changes in 3/5 status are italicized.

Table A. 3 – Replication of Table 2 for Senate Level

MODEL	ABF		TUFTE		HISTORICAL	
Level of Analysis	Senate		Senate		Senate	
Equation	(1)	(2)	(1)	(2)	(1)	(2)
Measure of Votes	-.043 (.396)	2.22*** (.448)	1.36** (.419)	2.49*** (.446)	.246* (.146)	.251 (.187)
Direct Election Dummy	-.683** (.245)	.148 (.257)	-.062 (.103)	.185 (.126)	-.003 (.012)	-.003 (.016)
Direct Election × Measure of Votes	1.22** (.445)	-.228 (.510)	-.004 (.494)	-.709 (.539)	.271 (.193)	.369 (.260)
South Dummy	-	1.40*** (.240)	-	2.36*** (.318)	-	-.015 (.012)
South × Direct Election	-	-1.00*** (.282)	-	-2.21*** (.367)	-	-.004 (.019)
South × Measure of Votes	-	-1.92*** (.458)	-	.136 (.806)	-	-.253 (.194)
South × Direct Election × Measure of Votes	-	1.19** (.227)	-	-.051 (.920)	-	-.325 (.283)
Constant	.597** (.219)	-.647 (.227)	.227** (.088)	-.243 (.111)	.002 (.009)	.006 (.012)
R- Squared	.134	.837	.200	.611	.152	.120
N	90	180	90	121	88	176

*** p < .001 ** p < .05 * p < .10

Robust standard errors in parentheses.

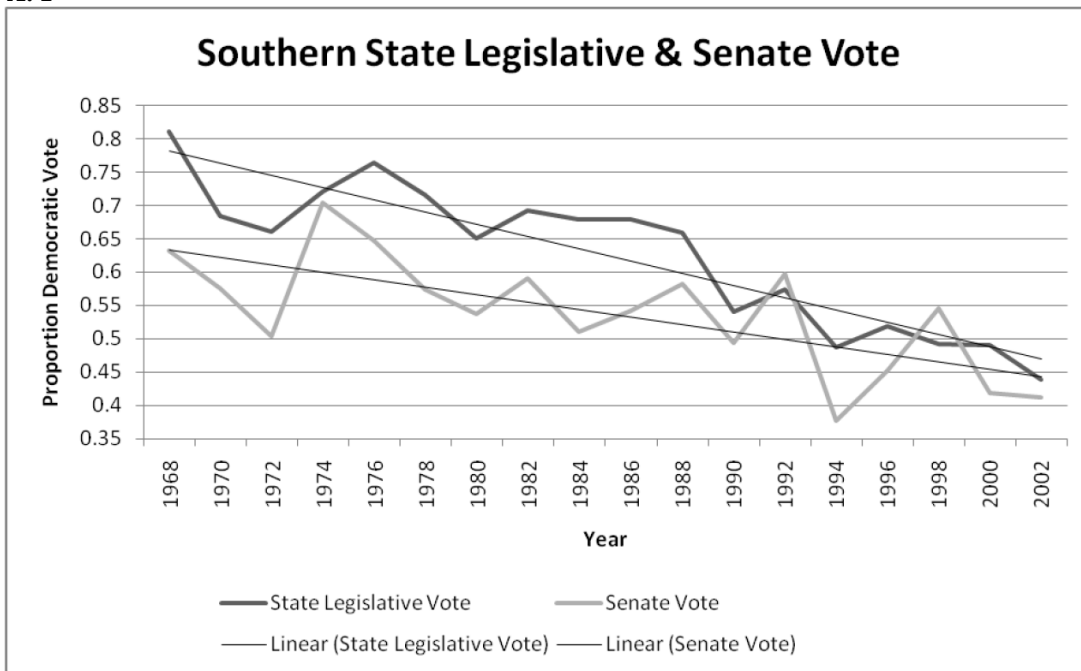
Table A. 4

MODEL	HISTORICAL: CLASS LEVEL 1964 - 2006		
Variable	Coefficient	Robust S.E	P-Value
Measure of Votes	3.61	.961	.000
Direct Election Dummy	.008	.042	.857
South Dummy	-.044	.049	.374
South & Direct Election Interaction	.030	.073	.683
Direct Election × Measure of Votes	-1.51	1.15	.192
South × Measure of Votes	-3.81	1.01	.000
South × Direct Election × Measure of Votes	3.58	1.31	.007
Constant	-.024	.034	.486
R- Squared	.350		
N	88		

Table A. 5

MODEL	KING & ELLIS AGGREGATED OLS MODEL: SENATE LEVEL								
TIME PERIOD	1918 - 1942			1944 - 2006			1918 - 2006		
Variable	Coefficient	Robust S.E	P-Value	Coefficient	Robust S.E	P-Value	Coefficient	Robust S.E	P-Value
Measure of Votes	1.80	.297	.000	.699	.302	.024	1.28	.230	.000
Bias	.099	.044	.035	.057	.024	.021	-.0122	.023	.600
Constant	-.442	.138	.004	.250	.157	.117	.089	.119	.460
R- Squared	.508			.141			.201		
N	26			64			90		

Figure A. 1



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