

# THE ELECTORAL CONNECTION IN THE U.S. SENATE

by

JOEL ALLEN SIEVERT

(Under the direction of Jamie L. Carson)

## ABSTRACT

Congressional scholars largely agree that electoral considerations influence the behavior of members of Congress, a phenomenon referred to as the electoral connection. A disproportionate share of scholarship on the electoral connection, however, focuses on the House of Representatives. This dissertation examines how the U.S. Senate's institutional design, such as staggered terms or different methods of election over time, influence senators relationship with constituents. The analyses reported are based on a series of quasi-experimental and experimental research designs that utilize unique aspects of the Senate's institutional structure. Overall, this project highlights the importance of institutional design for understanding how elections influence the behavior of legislators in the U.S. Congress.

INDEX WORDS: Senate, Congressional Elections, Electoral Institutions, Institutional Design

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JOEL ALLEN SIEVERT

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M.A., Washington University in St. Louis, 2009

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JOEL ALLEN SIEVERT

Approved:

Major Professor: Jamie L. Carson

Committee: Anthony J. Madonna  
Michael Lynch  
Ryan Bakker

Electronic Version Approved:

Suzanne Barbour  
Dean of the Graduate School  
The University of Georgia  
August 2015

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# Chapter 1

## Introduction

With the proposition that members of Congress are “single-minded seekers of reelection,” Mayhew (1974*a*) advanced an important framework for understanding and studying Congress. In terms of theoretical expectations, Mayhew’s proposition, which is known as the electoral connection, states that a desire to be reelected informs legislators’ behavior. Although Mayhew was not the first to argue that the pursuit of reelection guides legislator’s behavior, the electoral connection gives greater weight to the importance of electoral considerations. In his work on committees in the House of Representatives, Fenno (1973) argued that reelection was one of three co-equal goals, policy and institutional prestige being the other goals, that committee assignments help members of Congress to achieve. Mayhew contends that reelection supersedes these other goals because members of Congress cannot influence policy or obtain institutional prestige without first assuring their own reelection. Reelection is therefore a *necessary* condition, or in Mayhew’s terms the proximate goal, for members to achieve before any other goal.

In the four decades since Mayhew (1974*a*) first theorized about the electoral connection, congressional scholars have amassed a remarkable body of evidence to support the theory’s expectations about legislative behavior. These studies generally

focus on the three basic activities – advertising, credit claiming, and position taking – Mayhew assumes reelection-minded legislators will pursue. Mayhew defines advertising as legislative behavior that helps a member of Congress make him or herself more visible to constituents or facilitates the development of a “brand name.” These activities involve little issue content and focus more on a legislator’s positive personal qualities. Subsequent research on legislator’s advertising habits investigates how activities such as the franking privilege (Cover and Brumberg, 1982) and the allocation of staff resources to their district help members of Congress to increase their visibility among constituents (Fenno, 1977). Advertising can prove to be electorally valuable because a legislator’s name recognition can easily translate into votes on election day (Cover and Brumberg, 1982; Mann and Wolfinger, 1980).

Mayhew defines credit claiming as particularized benefits that help a legislator credibly demonstrate support for a specific individual or geographic constituency. In order to maximize the benefit from these opportunities, it is important for a legislator to have a direct hand in securing these benefits. Pork barrel projects, or appropriations more generally, are one such benefit that members exploit for credit claiming purposes (Bickers and Stein, 1994; Crespino and Finocchiaro, 2013; Grimmer, Messing and Westwood, 2012; Lazarus and Steigerwalt, 2009). Casework, such as helping a constituent navigate the bureaucracy to obtain a specific benefit, is another ideal form of credit claiming (Fiorina, 1989). The procurement of particularized benefits can increase electoral support from voters who might not otherwise support a legislator (Bickers and Stein, 1994; Fiorina, 1989) and can potentially scare off serious challengers (Bickers and Stein, 1996).

According to Mayhew, position taking involves the use of roll call votes and public statements to stake out popular positions. In doing so, a member of Congress is engaged as a “speaker rather than a doer” (Mayhew, 1974*a*, 62). That is to say, the position itself is more valuable than the actual policy outcome. Of the three

activities, position taking poses the greatest electoral risks since taking the “wrong” position can be electorally costly (Fiorina, 1989). Indeed, members of Congress can be punished on election day for adopting positions that are either too partisan or too ideologically extreme for their district (Canes-Wrone, Brady and Cogan, 2002; Carson et al., 2010). Furthermore, legislators may even be punished for being on the wrong side of a specific roll call vote (Bianco, Spence and Wilkerson, 1996; Bovitz and Carson, 2006; Carson and Engstrom, 2005). Legislators may also engage in strategic position avoidance when their electoral circumstances make casting difficult votes suboptimal (Jones, 2003). More recently, congressional scholars have become increasingly interested in how members of Congress structure the legislative agenda for position taking purposes (Grimmer, 2013; Harbridge and Malhotra, 2011).

While the activities Mayhew identifies provide a helpful template for formulating a research agenda, the electoral connection has much broader implications. Indeed, Mayhew’s theoretical framework is ultimately about the relationship between legislators and constituent. As Carson and Jenkins (2011) observe, the “linkage between legislators and constituents is electoral in nature; representatives serve at the behest of their constituents and can be voted out of office in regularly occurring elections” (27). In order to win reelection, legislators must not only work to meet the needs of their constituents, but they must also endeavor to explain their behavior to constituents (Fenno, 1977; Grose, Malhotra and Van Houweling, N.d.). Elections and the fear of potential electoral sanctions provide an incentive for legislators to act as faithful agents of their electoral principals. For constituents, elections provide an opportunity to learn about and evaluate an incumbent’s performance (Krasno, 1994). Constituents are then able to use their vote to reward or punish their representative. In this way, elections provide an important mechanism for constituents to communication with legislators.

## 1.1 Elections and Legislator-Constituent Relations

Carson and Jenkins (2011) argue that a representative-constituent linkage grounded in elections depends on four conditions. First, the electoral connection assumes that legislators are politically ambitious. Ambition is often defined as a desire for reelection or a career within Congress (Brady, Buckley and Rivers, 1999; Mayhew, 1974*a*). Others studies, however, argue that ambition should be defined more broadly as a desire for a political career, which can include but is not limited to a congressional career (Carson and Jenkins, 2011). Regardless of whether ambition is defined as political or congressional careerism, the effect of ambition on legislators is the same. Namely, ambition conditions legislators' behavior and makes them responsive to principals who influence or control their political future.

Second, the electoral connection presumes that legislators possess some level of autonomy with respect to decisions about their electoral and political careers. Under this condition, the desire for reelection is not a sufficient condition. Instead, the electoral connection requires that legislators have some control over whether or not they seek another term. Legislator's autonomy is commonly the central focus in studies that examine the consequences of electoral rules and changes to these institutions (Carson and Roberts, 2013; Kernell, 1977; Ware, 2002). Research on strategic politicians also highlights the importance of autonomy, but from the perspective of why politicians would choose to remain in their current elected position or selectively seek higher office (Jacobson, 1989; Rohde, 1979).

Third, the electoral connection posits that members of Congress have the capacity to be and, in general, are responsive to the preferences of their constituents. Responsiveness is usually defined as the relationship between legislators' behavior and the preferences of their constituents. In general, prior research suggests that legislators' voting behavior, committee assignments, and bill introduction decisions are correlated with constituent preferences and demographics (Adler and Lapinski, 1997; Erikson,

1978; Miller and Stokes, 1963; McCarty, Poole, and Rosenthal, 2006; Schiller, 1995). Responsiveness can therefore be defined broadly to include any number of legislative activities that are influenced by constituent's needs and preferences.

Lastly, the electoral connection requires that voters can and do hold legislators accountable for actions taken in their capacity as elected officials. Accountability is generally defined as constituents' ability to use their votes to reward or punish legislators. Prior studies demonstrate that legislators are punished for adopting positions that are too ideological (Canes-Wrone, Brady and Cogan, 2002), too partisan (Carson et al., 2010), or simply incongruent with constituent preferences (Ansolabehere and Jones, 2010; Bianco, Spence and Wilkerson, 1996; Carson and Engstrom, 2005). Furthermore, the threat of or potential for electoral retribution can also be an important factor in keeping legislators accountable to their constituents (Arnold, 1990; Kingdon, 1989; Mayhew, 1974a).<sup>1</sup>

The third and fourth conditions are the primary focus of this dissertation. While ambition and autonomy are certainly important components of the electoral connection, responsiveness and accountability are the primary emphasis here because they speak more directly to the relationship between legislations and constituents. Research on legislative responsiveness is ultimately concerned with how legislators make constituents' views present during governmental decision making. Similarly, studies on legislative accountability attempts to understand when and under what conditions constituents hold a representative responsible for actions taken in government.

The primary contribution of this dissertation is to provide a more detailed examination of how a legislature's institutional design influences responsiveness, accountability, and the general relationship between legislator and constituent. There are a number of reasons that legislative institutions can be crucial for understanding responsiveness and accountability. First, institutional design determines how legisla-

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<sup>1</sup>The fear of electoral punishment can also lead to periods of policy gridlock as legislators are afraid to make decisions that will adversely affect their constituents (Mayer, 1995).

tors can explain their actions in government to constituents. The ability to request a recorded vote on a salient issue (Clinton and Lapinski, 2008; Lynch and Madonna, 2013*b*) and the rules governing bill introduction (Cooper and Young, 1989; Schiller, 1995) both have important consequences for how legislators present themselves as representatives. Second, the rules governing how legislators are selected determine the extent to which voters can hold legislators accountable and the incentives legislators have to be responsive to constituents. Indeed, the adoption of the Australian ballot and direct primary laws are notable both for how they transformed elections as an instrument of accountability (Engstrom and Kernell, 2014; Rusk, 1970) and for their effect on legislative behavior (Carson and Roberts, 2013; Carson and Sievert, 2015; Katz and Sala, 1996; Ware, 2002). In sum, institutional design and the electoral connection are necessarily linked and we cannot fully understand the latter without also considering how the former shapes responsiveness and accountability.

## **1.2 The Institutional Design of the U.S. Senate and the Electoral Connection**

The U.S. Senate is an ideal legislative body in which to study the relationship between institutional design and key components of the electoral connection. Many different aspects of the Senate's institutional design can be particularly fruitful for examining the electoral connection. First, the Senate's staggered, six-year terms present a unique challenge for legislative responsiveness and electoral accountability. Since senators serve for a longer period than members of the U.S. House, some congressional scholars posit that senators have more opportunities, and perhaps even an incentive, to alter their behavior as elections approach (Ahuja, 1994; Bernstein, 1991; Elling, 1982; Stratmann, 2000; Thomas, 1985; Wright and Berkman, 1986). The theoretical argument in these studies is based on the assumption that senators

have greater behavioral discretion earlier in their terms when constituents are not actively monitoring their behavior. As the election draws near, however, constituent monitoring is expected to increase, which encourages legislators to bring their behavior into closer correspondence with constituent preferences (Bernstein, 1991; Elling, 1982; Stratmann, 2000; Thomas, 1985; Wright and Berkman, 1986).

Second, senators, unlike many other legislators, share a geographic constituency with another member of the same legislative body. While the equal apportionment of senators has broad implications for representation (Lee and Oppenheimer, 1999), the concurrent service of two senators from the same state can prove valuable for understanding legislative responsiveness and representation. Although these senators share a geographic constituency, they do not always the same re-election constituency (Schiller, 2000). As such, the comparison of senators from the same state can be valuable for identifying to whom legislators are responsive. Schiller (2000) explores this question by investigating how senators from the same state construct their representational profiles. Not surprisingly, Schiller finds that senators' legislative behavior reflects their needs to appeal to different groups within the geographic constituency. Similarly, Bullock and Brady (1983) examine the voting behavior of senators from states with split-party Senate delegations. In doing so, Bullock and Brady are able to examine the trade-offs legislators must make between representing their constituents and supporting their party.

Third, the method of electing senators has changed markedly since the ratification of the U.S. Constitution. Prior to the adoption of the Seventeenth Amendment in 1913, state legislatures were responsible for the election of senators. Although there is debate over what precisely this electoral arrangement was intended to accomplish (Haynes, 1906; Riker, 1955; Swift, 1996), the move to direct elections is one of the only constitutionally mandated alterations to the workings of the Senate. A growing body of research examines how the switch from indirect to direct elections

influenced representation in the Senate (Bernhard and Sala, 2006; Crook and Hibbing, 1997; Gailmard and Jenkins, 2009; Rogers, 2012; Schiller, 2006; Schiller and Stewart, 2015; Wirls, 1999). While these studies examine both election outcomes (Crook and Hibbing, 1997; Rogers, 2012; Schiller and Stewart, 2015) and legislative behavior (Bernhard and Sala, 2006; Gailmard and Jenkins, 2009), they are all broadly concerned with the question of how institutions influence legislator’s responsiveness.

Lastly, the Senate’s procedural rules also has important implications for the electoral connection. Unlike the House’s majoritarian procedural rules, the Senate’s procedural rules often empower individual senators or the minority party (Binder, 1997; Smith, 1989). As a result, Senate procedure often prevents additional consideration of a legislative proposal and can even result in prolonged policy gridlock (Binder, 2003). The inability to move forward on legislative proposals can have important consequences for understanding how responsiveness and accountability operate in the Senate. In terms of responsiveness, difficulties in obtaining a vote on substantive legislation can lead senators, and their electoral opponents, to adopt creative strategies for explaining the representational value of a senator’s legislative behavior (Grose, Malhotra and Van Houweling, N.d.; Smith, Ostrander and Pope, 2013; Smith, 2014). Senate procedure can also make it difficult for constituents to fully understand how the behavior of their legislative surrogates relates to the policies that influence their lives (Arnold, 1990). The problem can be further exacerbated by media coverage that fails to provide the proper context or full information (Smith, 2014).

### **1.3 Organization**

Each chapter in this dissertation draws on at least one of the aspects of the Senate’s institutional design outlined above to better understand the electoral connection. Although the chapters examine different subjects, the research question in each analysis

relates back to the broader theme of how Senate institutions influence the relationship between senators and constituents. The remainder of the dissertation is organized as follows.

In Chapter 2, I examine the pre-Seventeenth Amendment practice of electing senators by state legislatures. Previous studies explore this subject by comparing senators elected under both indirect and direct elections (Bernhard and Sala, 2006; Gailmard and Jenkins, 2009; Meinke, 2008). I propose an alternative research design that compares indirectly elected senators to members of the House who were elected from statewide districts. My research design takes advantage of two unique features of the nineteenth century congressional districting process. First, during the Antebellum period, some states elected their entire congressional delegation in at-large districts. Second, throughout the nineteenth century, many states that gained a seat during reapportionment would elect the new representative in a statewide contest rather than redrawing district lines. As a result, there are not only more representatives elected statewide, but they also come from a more diverse set of states than in contemporary elections. The comparison of these two types of legislators is predicated on the fact that these legislators share a geographic constituency, but have different reelection constituencies (Bullock and Brady, 1983; Fenno, 1977; Schiller, 2000). Contrary to previous research, I find that indirectly elected legislators were quite comparable to directly elected legislators.

In Chapter 3, I examine whether the Senate's six-year terms influence both senators' responsiveness and constituents' ability to hold legislators accountable. Previous research posits that constituents' attention to a senator's behavior varies as a function of the time until a senator's next election (Ahuja, 1994; Elling, 1982; Thomas, 1985). As a result, senators are assumed to have greater behavioral discretion earlier in their term when constituents do not actively monitor their behavior. The assumption of time-dependent monitoring implies that constituents' evaluations of senators

will vary depending on whether or not a senator is up for re-election. Although there is an intuitive appeal to this assumption, these studies do not always explicitly test whether constituents' evaluations of senators change as a function of electoral context. In order to test the effect of electoral proximity, I focus on states where an incumbent is running for re-election. Since only one of a state's two senators is up for re-election at a time, these states make it possible to compare an individual's evaluations of senators who are and are not up for re-election. Of particular interest is whether elections enhance the relationship between ideological congruence and job approval. I use data from the 2010 and 2012 Cooperative Congressional Election Study (CCES) surveys to examine these relationships. I find that the relationship between preference congruence and constituent evaluations does not differ across electoral context.

In Chapter 4, I examine how the Senate's procedural rules influence constituent's ability to hold senator's accountable for their roll call positions. Prior studies suggest that legislators can hide behind procedure because it can be difficult for constituents to connect procedure to policy effects. The question is of particular importance in the U.S. Senate because its procedural rules can make it difficult to obtain a vote on final passage for major legislation. I use a survey experiment to test whether constituents' assessment of a senators voting record changes when a vote is presented as a substantive or procedural question. The survey experiment randomly assigns respondents to one of two groups. In the first group, respondents are asked whether they would approve of their senator voting for or against a proposal on a salient issue. In the second group, respondents are asked whether they would approve of their senator voting for or against a proposal to end debate and proceed to a vote on a salient issue. If prior research on procedural coverage is correct, then respondents should react more strongly in the case where they are told that the senator voted for or against the proposal. Based on the results presented in this chapter, I argue that the concept of procedural coverage in the Senate is correct in theory, but also fails to

adequately explain the observed actions of senators and their electoral opponents.

In the final chapter, I review the broader implications of the findings presented in each chapter. In particular, I address what these findings tell us about the nature of senator-constituent relations both historically and in the contemporary Senate. I also outline other avenues for future research that builds on various aspects of each chapter.

## Chapter 2

# Indirect Elections and Representation in the U.S. Senate

During the 2012 election, several Republican candidates for the U.S. Senate went on record in support of repealing the Seventeenth Amendment, which instituted the direct popular election of U.S. senators.<sup>1</sup> While campaigning for a U.S. Senate seat in Michigan, former U.S. Representative Peter Hoekstra (R-MI) voiced the concern each candidate shared about this constitutional amendment. Hoekstra argued that “direct election of U.S. Senators made the U.S. Senate act and behave like the House of Representatives. The end result was an erosion of state’s rights” (Lesniewski, 2012). Aspiring senators were not the only ones to express these sentiments. Both former Sen. Zell Miller (D-GA) and Sen. Ted Cruz (R-TX) voiced their support for a repeal of direct elections while serving in the U.S. Senate (Dart, 2004; Gillman, 2013).<sup>2</sup>

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<sup>1</sup>The Senate candidates were then Representative Todd Akin (R-MO), then Representative Peter Hoekstra (R-MI), and Indiana State Treasurer Richard Mourdock. Senator Jeff Flake (R-AZ), who at the time was still a member of the House, was also reported to have voiced support for the repeal of direct election, but later denied that he favored such an action.

<sup>2</sup>Supreme Court Justice Antonin Scalia also voiced concern about the adoption of direct Senate elections. During an event at Texas Tech University Law School, Justice Scalia said the Seventeenth Amendment “has changed things enormously. We changed [the method of election] in a burst of progressivism in 1913, and you can trace the decline of so-called states’ rights throughout the rest of the 20th century” (Blaney, 2010).

The argument against direct election stems from a belief that indirectly elected senators were more responsive to state governments. The assumption is based on the doctrine of instruction by which state legislatures could advise senators on how to vote on particular issues (Riker, 1955). Although state legislatures could instruct members of its U.S. Senate delegation on particular matters, the practice did not always constrain senators and was ultimately abandoned (Eaton, 1952; Hoffmann, 1956; Riker, 1955; Swift, 1996). It would be incorrect, however, to say that indirectly elected senators did not respond to state legislatures. Indirectly elected senators' roll call voting responded to the partisan composition of their state legislature (Bernhard and Sala, 2006; Gailmard and Jenkins, 2009). Similarly, incumbent senators' decision to seek re-nomination and their likelihood of winning re-election were both influenced by the partisan makeup of the state legislatures (Bernhard and Sala, 2006; Schiller, Stewart and Xiong, 2013). Despite the ineffectiveness of instruction, state legislatures were still able to exert some influence on indirectly elected senators.

While indirectly elected senators were influenced by the state legislatures, understanding when they were representative of the broader statewide constituency is more complicated. As the term indirect suggests, these senator's electoral constituency constituted a small subset of the broader statewide electorate. Under indirect election, the mass electorate lacked any recourse to influence the selection of a senator except through their vote for a state legislator. The extent to which indirectly elected senators mirrored the preferences of their statewide electorate was ultimately determined by how well the state legislators reflected the preferences of the mass electorate.

Prior studies do not directly address this question, but instead focus on whether direct election led senators to change their behavior to match the preferences of their expanded constituency (Bernhard and Sala, 2006; Meinke, 2008; Gailmard and Jenkins, 2009). The approach is logical given the natural pre- and post-test created by the adoption of new electoral rules. It does not, however, fully capture the nature

of representation under indirect elections, which is the primary focus of this study. Examining indirect elections in their own right is important for a number of reasons. First, if indirect elections produced legislators that were relatively comparable to those selected by the mass electorate, then the claim that direct election caused the Senate to become more like the House is less tenable. Second, the (in)ability of indirect elections to reflect the mass electorate's preferences fits with recent studies on the relationship between candidate selection rules and legislative behavior (Alvarez and Sinclair, 2012; Bullock and Clinton, 2011; Carson and Sievert, 2015; McGhee et al., 2014). In particular, my analysis of candidate selection under alternative selection rules can speak to the debate over the effect of open versus closed primaries (Bullock and Clinton, 2011; McGhee et al., 2014).

In order to assess the representativeness of the indirect election of senators, I utilize a research design that compares senators to representatives who were elected from statewide congressional districts. The proposed design takes advantage of two unique features of historical congressional districting practices. First, during the Antebellum period, some states elected their entire congressional delegation in at-large districts. Second, throughout the nineteenth century, some states would temporarily create one or more at-large districts when they gained House seats in the decennial reapportionment (Engstrom, 2013). As a result, there were not only more representatives elected statewide during this period, but there was also greater variation with respect to the size of the states they represented.<sup>3</sup> Indirectly elected senators and House members from statewide districts are a natural group to compare because they share a common geographic constituency, but have different re-election constituencies (Fenno, 1977). Although the groups differ with respect to the chamber in which they serve, this approach still provides a test of the relationship between electoral selection

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<sup>3</sup>In the 113th Congress, there would only be seven members of the House who were elected statewide and they all came from states with a single congressional district. In contrast, during the 43rd Congress, 19 members of the House were elected statewide and they represented states with anywhere from one to 30 congressional districts.

mechanisms and legislative behavior. Furthermore, this approach makes it possible to compare indirect elections to the concurrently available alternative electoral rules.

The remainder of this paper proceeds as follows. In the next section, I discuss the intent behind indirect election and the reality of how it operated. I then provide an overview of the literature on the effect of indirect and direct Senate elections and the nature of representation under indirect Senate elections. Next, I provide a detailed discussion of my research design and its theoretical motivations. I then conduct a series of analyses to compare the type of legislators selected under direct and indirect elections before discussing the broader implications of my findings.

## 2.1 Indirect Senate Elections: Intent and Reality

The authors of the Constitution intended the Senate to be different than the popularly elected House. During both the Constitutional Convention and the subsequent debate over ratification, proponents of the Constitution argued that indirect election would make the Senate more responsive to the states and help to check the “excess of democracy” (Haynes, 1906; Riker, 1955). Despite the professed intent of the Senate’s original design, it is not clear that it was every fulfilled (Haynes, 1906; Swift, 1996).

Nearly a decade before the Seventeenth Amendment, Haynes (1906) addressed the concern that popular elections would undermine the Senate’s intended role in the federal system.

But in the first place, it may be said that it is easy to overestimate the degree of representation of the States, as such, which is now either desirable or possible. More than a century of history lies between the First and the Fifty-ninth Congress. During that period, nationalizing forces have been vigorously at work transforming the people of loosely federated and almost alien States into the citizens of one great federal state . . . (160-161).

Haynes’ observation is important because it highlights that the federal-state relationship underwent profound transformations long before the adoption of direct election.

For instance, Ladd (1970) notes that the concept of “state’s rights” had a different meaning under the more localized society and economy of the pre-industrialized United States, but its meaning changed after the nationalization brought about by the post-Civil War industrialization (69-70). Similarly, Balogh (2009) observes that the federal government’s role in citizens lives throughout the nineteenth century was not only real, but was often “hidden in plain sight” (13). In short, nationalization was underway well before direct Senate elections, which makes it difficult to identify them as the causal force behind changes in federal-state relations.

The inability of indirect elections to forestall the centralization of governmental power can be traced, at least in part, to an important omission from the Constitution. Under the Articles of Confederation, states were not only granted the right of instruction, but they could also recall legislators from the national legislature. The omission of these provisions from the Constitution meant that the state governments lost important tools for checking the national government (Riker, 1955; Swift, 1996).<sup>4</sup> State legislatures did, however, continue to send instructions to senators even though they were not explicitly granted the power by the Constitution (Riker, 1955; Swift, 1996). While senators were largely receptive to these instructions in the early congresses (Swift, 1996), there was opposition to the practice even in the First Congress (Riker, 1955, 457). Senatorial opposition grew more forceful in the 1830s and 1840s when state legislators attempted to use it to force the resignation of senators from opposing parties (Eaton, 1952; Hoffmann, 1956). As a result, the practice became discredited and eventually fell out of use by 1860 (Eaton, 1952). In short, members of the U.S. Senate gained considerable independence from from state legislatures long before the adoption of the Seventeenth Amendment.

Indirect Senate elections were plagued by a number of additional problems. First,

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<sup>4</sup>Swift (1996) argues that the exclusion of the right of instruction from the Constitution is evidence that the Senate’s design was not a serious attempt to provide the states with a check on the federal government.

indirect elections were notoriously prone to deadlock because the state legislature could not reach an agreement (Haynes, 1906; Schiller, Stewart and Xiong, 2013). It was not unheard of for a state to go an entire congress, or more, without full representation in the Senate. Indeed, the number of deadlocked races became so problematic that Congress passed a law in 1866 to streamline the process. The absence of gridlock was not, however, always a good sign. Haynes (1906) documents several cases in which a Senate candidate was chosen on the first ballot because of back-room deals made during party caucus meetings.

Second, indirect Senate elections were not only prone to corruption, but the campaign spending in these races could exceed the amounts spent in modern elections (Schiller, Stewart and Xiong, 2013). Business interests that wanted to sway the outcome of Senate elections would bankroll state legislative candidates or bribe them to win voters for a preferred Senate candidate (Squire, 2012). In some cases, a single individual was found to have handed out exorbitant amounts of money in bribes. According to Schiller, Stewart and Xiong (2013)[840], one state senator in Montana testified in 1899 that he distributed over \$200,000 in bribes, which equates to \$5.5 million in 2012 dollars, to buy votes for one of the Senate candidates.<sup>5</sup> It is important to note that this constituted only a fraction of the total *known* spending on this one race.

In sum, indirect elections were far more problematic than some contemporary opponents acknowledge. There is little evidence that indirect election made senators beholden to the state governments. Although senators did follow the directions of the state legislatures, particularly in the early congresses (Swift, 1996), they did not always comply with these instructions. Indeed, there are several notable cases of senators disregarding instructions, even among senators who previously advocated for the

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<sup>5</sup>As a point of comparison, it can be instructive to compare this to a recent U.S. Senate election in Montana. According to OpenSecrets, the combined PAC contributions for the 2012 U.S. Senate race in Montana was just over \$3 million.

doctrine of instruction when they served in the state legislature (Eaton, 1952; Hoffmann, 1956). Similarly, indirect elections were characterized by back-room politics, bribes, and lavish campaign spending (Haynes, 1906; Schiller, Stewart and Xiong, 2013). It is hard to imagine that any of these characteristics would be acceptable to either the public or contemporary opponents of direct election.

## 2.2 Indirect Senate Elections and Representation

Electoral institutions provide the mechanism through which individual or collective preferences are aggregated to choose a winner. Whether the rule determines the set of choices presented to voters or how voters are aggregated, the rules themselves are an essential part of the electoral process. As King and Ellis (1996) observe, “[i]n any electoral system, outcomes are a function of preferences mediated by rules. Different rules can impose a different translation of preferences into outcomes . . .” (76). Given the potential for different sets of rules to translate the *same* preferences into different outcomes, researchers often focus on the extent to which two or more sets of rules produce (dis)similar outcomes. In terms of representation, one point of interest is how the outcome under one rule compares to those obtained through a rule that allows for greater or more direct participation.

The Electoral College is one prominent example in American politics of an electoral institution whose representativeness has been questioned (Edwards, 2011). Although the winner of the Electoral College usually wins the popular vote, there have been notable deviations. Indirect Senate elections represent a similar scenario. In this case, the alternative sets of rules are selection by state legislatures and direct elections by the statewide electorate. Under the former, statewide preferences are filtered through an additional stage – selection of state legislators – in order to elect a U.S. Senator. In contrast, the latter removes this filter and selects senators by a direct vote of the

mass electorate. As with the Electorate College, the primary interest is whether these rules provide a different translation of preferences into outcomes.

Prior studies evaluate the representativeness of indirect Senate elections in terms of both election outcomes and legislative behavior. In the sections below, I outline key findings from previous research on the relationship between the method of electing senators and each type of outcome. I also review prior theoretical treatments of the role of state legislatures in indirect Senate elections. The involvement of state legislatures in the selection of U.S. senators is hypothesized to play a critical role in affecting representation under indirect elections. In particular, the malapportionment of state legislatures in this era is often pinpointed as an impediment to representation.

## **Electoral Outcomes**

Initial empirical studies on the politics of indirect Senate elections focused on how partisan preferences at the state-level translated into the partisan makeup of the Senate under indirect elections. In general, these studies find clear differences between indirect and direct Senate elections. King and Ellis (1996) contend that the method of translating preferences into seats under indirect elections could provide a systematic advantage for one party over the other. The advantage was a consequence of indirect elections overstating the electorates' partisan preferences. King and Ellis posit that the "aggregation by [state legislative] district can mobilize different interests among voters and produce a different result from their votes than aggregation by state as a whole" (75-76). Similarly, Crook and Hibbing (1997) find that national partisan preferences, as captured by presidential vote, were not correlated with the partisan makeup of the Senate until after direct election.

Other studies, however, suggest that indirect elections were generally representative of the statewide electorate's preferences. Wirls (1999) compares the partisan affiliation of indirectly elected senators and the winners of other statewide elections

in states in the Northeast and West. He finds that indirectly elected senators were generally from the same party as the individuals who won under a direct statewide elections. Similarly, Rogers (2012) finds few differences between the partisan affiliation of legislators selected under direct election and the counterfactual outcome that would have occurred if state legislatures still chose senators.

Schiller and Stewart (2015) also utilize a counterfactual analysis to compare indirect and direct Senate elections. Contrary to Rogers (2012), they find sizable differences between indirect and direct elections in the four decades following the adoption of direct elections. Schiller and Stewart do not, however, find a consistent or substantively large differences between the partisan composition of the Senate under indirect elections and the direct election counterfactual (193). They posit that the sparse differences between indirect elections and the direct election counterfactual are not surprising because any earlier change from indirect election by state legislatures to a direct popular vote “would have only shifted the venue of election from one polarized electorate to another” (196). It is important to keep this observation in mind since the primary objective of this analysis is to understand the representativeness of indirect Senate elections relative to concurrently available alternatives.

## **Legislative Behavior**

Unlike research on election outcomes, there is more agreement about the influence of the Seventeenth Amendment on legislative behavior. Before the adoption of direct election, senators were highly responsive to political dynamics within state legislatures (Bernhard and Sala, 2006; Gailmard and Jenkins, 2009; Schiller, 2006; Schiller, Stewart and Xiong, 2013). After the switch to direct elections, however, senators are found to be more responsive to the partisanship of the statewide electorate (Bernhard and Sala, 2006; Meinke, 2008; Gailmard and Jenkins, 2009). These differences are attributed to changes in senators’ electoral constituency before and after

the Seventeenth Amendment. Gailmard and Jenkins (2009) argue that the observed pattern of behavior can be explained by a principal-agent framework. According to Gailmard and Jenkins, the Seventeenth Amendment “made senators direct agents of their ultimate principals, state voters, rather than indirect agents directly accountable to political intermediaries in the state legislature” (324). From this perspective, senators’ behavior should not only differ across the two periods, but would in fact be logical given the strategic context brought about by each set of rules.

The expected pattern of behavior in the pre- and post-Seventeenth Amendment Senate is evident in a number of different legislative activities. Bernhard and Sala (2006) find that senators who stood for reelection under indirect election were more likely to adopt ideologically extreme positions in the year before an election, which moved them closer to the median state legislator. In contrast, senators who stood for reelection under direct election were more likely to moderate their positions in the year before an election in order to shift toward the statewide median voter. Gailmard and Jenkins (2009) find that senators’ voting behavior was positively correlated with the partisan composition of the state legislature prior to the adoption of the Seventeenth Amendment, but the relationship weakens after this point. In contrast, the correlation between the preferences of the statewide electorate increased from the pre- to post-reform period. Meinke (2008) reports similar results for senator’s party unity voting and bill introduction behavior.

It is important to note that many of these studies find evidence of a positive relationship between the statewide electorate’s preferences and legislative behavior under indirect elections. Furthermore, there is also growing evidence that indirectly elected senators engage in many of the same electorally motivated behaviors as contemporary senators (Schiller, 2006; Schiller and Stewart, 2015). Indirectly elected senators not only introduced constituency-oriented legislations (Schiller, 2006), but they also actively worked to secure patronage and appropriations dollars for their state (Schiller

and Stewart, 2015). These activities helped senators build a base of support amongst party officials and the statewide constituencies that could reinforce their support in the state legislature. In short, even if direct elections increased senators interest building a reputation among their statewide constituency, it would be inaccurate to infer that they actively ignored these voters before the Seventeenth Amendment. The high rate of turnover in state legislatures during this era (Schiller, Stewart and Xiong, 2013) meant that senators who ran for reelection would generally serve longer than many of the state legislators who originally voted them into office. As a result, senators had an incentive to court their statewide constituencies well before the adoption of direct elections in order to build a more durable base of support among the general public.

## **State Legislatures**

Before the Supreme Court handed down its “one person, one vote” edict in the 1960s, state legislative districts were often woefully malapportioned. Rather than apportion districts by population, some states adopted rules that assured every county at least one legislative seat. In addition to advantaging rural counties, this practice could, at times, lead to a legislature whose partisan makeup was a poor reflection of the statewide electorate’s preferences. The influence of these districting practices on the partisan composition of the state legislature is important for understanding representation under indirect elections because state legislatures almost always elected a co-partisan to the Senate (Rogers, 2012; Schiller and Stewart, 2015). Severe malapportionment could, in theory, lead to the selection of a senator whose partisan leanings do not match the disposition of the statewide electorate.

Malapportionment in state legislatures is also theorized to have important consequences for the ideological preferences of senators chosen under indirect elections. Bernhard and Sala (2006) are particularly instructive on this point. Their empirical

analysis tests predictions from a spatial model in which the median state legislator serves as the median voter under indirect election and the statewide median voter is decisive under direct elections. Bernhard and Sala's key assumption is that the median state legislator is more ideologically extreme than the statewide median voter. The gap between the statewide median voter and median state legislator is posited as a key reason why senators chosen under indirect elections should be more ideologically extreme than directly elected senators.

Although malapportionment was, at times, quite pronounced, it is important to not overstate its effects. In terms of election outcomes, it is not clear that state legislatures consistently deviated from the preferences of the statewide electorate (Wirks, 1999). Schiller and Stewart (2015) find that, on average, only one to two state legislatures per election year chose a senator whose partisanship did not align with the preferences of the mass electorate. In terms of legislative behavior, indirectly elected senators actively appealed to the statewide constituencies even if these individuals were only indirectly responsible for a senators' political future (Schiller, 2006; Schiller and Stewart, 2015). Even if a malapportioned state legislature was more ideologically extreme, indirectly elected senators' interest in their mass constituency suggests that they behaved in ways similar to directly elected legislators.

## **2.3 Research Design**

Identifying the unique effect of an electoral rule is one of the biggest challenges in the study of electoral institutions. Since previous studies were primarily interested in the effect of the Seventeenth Amendment, their analyses involved some form of comparisons between indirect and direct Senate elections. The first approach can be thought of as a pre- and post-test with adoption of the Seventeenth Amendment serving as the structural break in the electoral rules (Bernhard and Sala, 2006; Crook and

Hibbing, 1997). The representativeness of each electoral method is evaluated through an interaction term that allows the relationship between constituent preferences to vary before and after the rule change. Although there is an intuitive appeal to this approach, it cannot account for a potential spurious result that may arise from other trends or broader changes in the electoral system. This is of particular concern given the rate at which the American electoral system was changing during this period (Burnham, 1965; Ware, 2002).

The second approach can be thought of as a pre- and post-test with a control group. It is designed to address the shortcoming of the first approach. While the analysis still involves the same comparison between indirect and direct Senate elections outlined above, it also replicates the analysis on members of the House (Gailmard and Jenkins, 2009). Since House elections were not altered by the adoption of the Seventeenth Amendment, representatives provide a “untreated comparison group” that should not respond to the treatment since they were not exposed to the change. If senators are the only group affected by the rules change then we can have greater confidence that the results represent the unique effect of changes in the rules and not a broader trend.

Although the second approach is an improvement over the first, the treatment effect is still identified by comparing legislators from electoral systems that differ in ways beyond just indirect and direct election. For example, in 1880, there was not a single state that used either the secret ballot or a direct primary. By the time the Seventeenth Amendment is adopted, however, nearly every state used a secret ballot and many states had also adopted direct primaries. Furthermore, the inclusion of the House as a control group is instructive, but it cannot address this particular concern. Instead, the use of the House as a control group only provides an indication that the treatment occurred at the correct time. It does not, however, rule out the possibility that the observed effect is based on a combination of changes in the ballot laws,

primary rules, and method of election.

An alternative design, which is employed here, is to compare legislators who are elected at the same time from the same geographic unit, but who are chosen under different rules (Stratmann and Baur, 2002). In doing so, I can examine outcomes under indirect elections relative to the alternative that was available at the *same time*. Rather than comparing indirect and direct Senate elections, this research design uses indirectly elected senators and representatives elected from statewide districts as the treatment and control group respectively. The approach is analogous to the one used in studies that compare the behavior of U.S. senators from the same state (Bullock and Brady, 1983; Schiller, 2000). As with studies of senators from the same state, the comparison of indirectly elected senators and House members from statewide districts is motivated by these legislators sharing a common geographic constituency, but having different re-election constituencies (Fenno, 1977).

In order to implement this research design, I take advantage of two unique features of historical of congressional redistricting practice.<sup>6</sup> First, some states, such as Georgia, elected their entire congressional delegate in a single statewide multi-member district (Calabrese, 2000; Engstrom, 2013). Although Congress technically outlawed this practice in 1842, some states continued to use this districting method, which is known as general ticket elections, throughout the late nineteenth and early twentieth centuries Engstrom (2013). Second, states occasionally created at-large districts when they gained seats in the House during the decennial reapportionment (Calabrese, 2000).<sup>7</sup> Engstrom (2013, 72), explains that these districts were the result of a state keeping “its old map intact and elect[ing] any newly gained seats at-large.” While these at-large districts were intended to be used for only a single election, some states kept them in place for multiple elections between reapportionments.

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<sup>6</sup>Wirls (1999) uses the same approach in an earlier analysis. The key difference, however, is that Wirls limited his focus to only Western states over a smaller time period.

<sup>7</sup>The House’s membership had not yet been capped at 435, which meant that more populous states often received more seats every ten years.

Substantively, these alternative districting methods mean that there were three different scenarios – general ticket elections, at-large districts, and states with a single congressional district – during this period in which a state would hold statewide House elections. The existence of these additional statewide House elections is advantageous for a number of reasons. First, there is greater variation within population among the states with statewide House elections. The greater diversity in state population is important since there can be representational differences between senators from more and less populous states (Lee and Oppenheimer, 1999). Second, there is also greater geographic diversity in terms of the states that held statewide House elections.

**Figure 2.1.** Statewide Congressional Elections



**Note:** States in gray are those that held a statewide congressional election at some point during the time period while those in white did not hold a statewide congressional election.

In order, to help demonstrate both of these points, the first panel of Figure 2.1 maps the states that held at least one statewide House election during the period from 1836 to 1912. In total, 41 of the 48 states held at least one statewide House election during this period. As Figure 2.1 demonstrates, this includes states from all regions of the country and a number of larger states such as New York, Ohio, and Pennsylvania. The second panel of Figure 2.1 maps the states that held statewide House elections in 2014. In 2014, only 7 states held a statewide House election and over half of these were four neighboring states.

Lastly, the number of House members elected statewide is much higher during

this era. As noted above, if I were to attempt this analysis for the 2014 election, there would only be seven members of the House who were elected statewide.<sup>8</sup> In contrast, there were 49 members of the House who were chosen in a statewide elections in the election of 1840. Furthermore, in 29 out of the 39 election years I examine, or just over 75 percent, there were more statewide House elections than under the current apportionment scheme. As such, I have more data points with which to make comparisons than I would have under current districting practices.

For the analysis that follows, I identified the population of House members elected statewide for all congresses from the 25th Congress (1837-1839) to the 63rd Congress (1913-1915). I begin with the 25th Congress because it is the earliest period for which I have complete data for all the relevant predictors. I end with the 63rd Congress because it is the last Congress before the nationwide adoption of direct election. One issue to note is that some states (e.g. Nevada) began to adopt direct election before the passage of the Seventeenth Amendment. I therefore exclude observations where a state has both statewide House elections and directly elects senators prior to the adoption of the Seventeenth Amendment.<sup>9</sup>

## 2.4 Data

In order to understand the nature of representation under indirect Senate elections, I examine both election outcomes and legislative behavior. I use the partisan outcome of the indirect Senate election and statewide House election as the outcome variable for the analysis of election outcomes. The variable, Democrat, is coded 1 if a Democratic candidate won the election and 0 if a Whig/Republican won. I use a measure of legislators' revealed preference in order to model legislative behavior.

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<sup>8</sup>The states with a single representative in the U.S. House are Alaska, Delaware, Montana, North Dakota, South Dakota, Vermont, and Wyoming.

<sup>9</sup>There were only 13 cases in which a state held statewide House elections and directly elected its U.S. senator prior to the 1914 election.

One issue in constructing an outcome variable is that senators and representatives must be placed on the same scale. I therefore use each legislators' first dimension DW-NOMINATE Common Space scores (Poole, 1998).<sup>10</sup> Common Space scores are an ideal measure of legislative preferences for this study because the estimation procedure places senators and representatives on a common scale.

Since the main theoretical interest is whether the different methods of election translate the *same* set of preferences into similar outcomes, the key explanatory variable must capture the preferences of the statewide electorate. I follow previous studies' modeling strategy and use presidential vote returns as a proxy measure of the statewide electorate's preferences (Crook and Hibbing, 1997; King and Ellis, 1996; Gailmard and Jenkins, 2009; Meinke, 2008). I code presidential vote as the Democratic share of the two-party vote in the state for the most recent presidential election. I center the variable at 50 so that positive values indicate that the Democratic presidential candidate received more votes in the state and negative values indicate that the Whig/Republican candidate received more votes. In order to allow the effect of the statewide electorate's preference to vary across election times, I interact this variable with a binary variable that denotes whether a legislator was a senator or a representative. The variable, *Senator*, is coded 1 if the legislator was an indirectly elected senator and 0 if legislator was a member of the House who was elected statewide.

I also control for a number of other factors. First, it is important to account for partisan dynamics within the state legislature. Since a candidate could not win without the support of both chambers, it is necessary to calculate a party's combined seat share across both the state House and state Senate. I use Dubin (2007) to calculate the total number of seats held by the Democratic party. The variable,

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<sup>10</sup>In the Appendix, I replicate this analysis using standardized party unity scores as the outcome variable instead of Common Space scores. The substantive interpretation does not change when this alternative measure is used.

Democratic Majority, is coded 1 if the Democrats held 50 percent or more of the seats and 0 otherwise. I also control for year specific-effects in both regression models. The inclusion of year-specific intercepts in these regression models is important for two additional reasons. First, it is necessary to account for differences in partisan fortunes across presidential election years, which could influence the distribution of the measure of constituent preferences. Second, it is important to account for the broader electoral context and year-specific effects are one instrument to do so.<sup>11</sup> In the regression models that examine legislative behavior, I also control for region-specific effects.<sup>12</sup>

## 2.5 Results

Using the data outlined above, I test whether indirect Senate election were as representative of the statewide electorate as concurrently contested direct statewide elections. The results of my analysis are presented in two parts. First, I examine the relationship between the preferences of the statewide electorate and election outcomes. Second, I estimate a series of regression models to test the effect of statewide preferences on legislative behavior.

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<sup>11</sup>Although I could include controls for other electoral laws (e.g. state-level ballot laws), these variables are not necessary to answer the theoretical and empirical questions raised in this project. The inclusion of a variable for state-level ballot laws does not change the substantive interpretation.

<sup>12</sup>I define each of these regions as follows. The observations from the South include legislators from Alabama, Arkansas, Florida, Georgia, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia. The observations from the Northeast include legislators from Connecticut, New Hampshire, New Jersey, New York, Pennsylvania, and Rhode Island. The observations from the West include legislators from California, Colorado, Idaho, Montana, Nevada, Oregon, Utah, Washington, and Wyoming. The observations in the reference category include legislators from Delaware, Illinois, Indiana, Kansas, Michigan, Missouri, Nebraska, North Dakota, South Dakota, and West Virginia.

## Election Outcomes

The main interest in this analysis is whether an indirect Senate election, on average, led to a different partisan outcome than the one observed in a concurrent direct, popular statewide House election. One way to assess the relationship between the method of election and partisan outcomes is to simply compare the partisanship of the indirectly elected senator and the directly elected representative (Wirks, 1999). Indeed, other studies find a remarkably high correspondence between the partisan outcome of indirect Senate elections and statewide popular votes (Schiller and Stewart, 2015; Wirks, 1999). The results based on this data set are no different. The legislators shared a common partisan identification in just under 90% of the senator-representative pairs. These calculations are substantively the same regardless of a senator's partisan identification. For Democratic senators, the rate of partisan congruence was 89.72%. In the case of Whig/Republican senators, the rate of congruence was 88.54%.

Although these calculations are informative, they do not directly model the relationships between preference, election type, and election outcomes. As such, I estimated a series of logistic regressions that model the probability of electing a Democrat as a function of statewide preferences, the type of election, and the interaction between these two predictors. The results of these regression models are presented in Table 2.1. In the first column of Table 2.1, I report estimates from a model that does not control for year-specific effects. The coefficient estimates in the second and third column are from a logistic regression models with intercepts that vary by year.

The interaction term in each model provides the primary means of adjudicating between the competing expectations of previous research. A positive and statistically significant coefficient would provide evidence in support of studies that expect indirect elections to differ from direct elections.<sup>13</sup> In contrast, a coefficient estimate that is not

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<sup>13</sup>According to these studies, changes in the statewide constituency's preferences should lead to

discernible from zero would provide support for studies arguing for no effect for the method of election. Overall, the substantive conclusion about the effect of indirect elections is the same across all three specifications. The coefficient for the interaction term is positive, but it is not statistically significant in any of the models.<sup>14</sup> In short, the coefficient estimates do not provide evidence of systematic differences between election outcomes under indirect and direct elections.

**Table 2.1.** Election Outcomes 1836-1912, Logistic Regression

	Est. (S.E.)	Est. (S.E.)	Est. (S.E.)
Dem. Pres. Vote	0.224* (0.045)	0.315* (0.048)	0.124* (0.045)
Senator	-0.478* (0.281)	-0.359 (0.359)	-0.418 (0.443)
Dem. Pres. Vote × Senator	0.023 (0.045)	0.039 (0.063)	0.066 (0.076)
Dem. Majority			4.669* (0.575)
Constant	-0.142 (0.311)	-0.475 (0.440)	-2.386 (0.437)
AIC	366.44	–	–
$\sigma^2_{\text{Year}}$	–	4.77	1.30
N	423	423	423

**Note:** Cell entries in the first column are from a logistic regression model with standard errors clustered by year reported in parentheses. Cell entries in the second and third columns are from a mixed effects logistic regression model with year-specific intercepts.

\*  $p < 0.05$

larger swings in the probability of electing a Democrat under indirect elections since the method of election is expected to over-respond to the electorate’s partisan preference (King and Ellis, 1996). Since presidential vote is centered at 50, a positive coefficient for the interaction term would indicate that the probability of electing a Democrat was higher (lower) under indirect election than direct election when the statewide electorate supported the Democratic (Whig/Republican) candidate in the last presidential election.

<sup>14</sup>As Brambor, Clark and Golder (2006) note, the substantive and statistical significance of an interaction term that includes a continuous variable needs to be evaluated across levels of the continuous variable. In Appendix A, I report predicted probabilities by election type over the empirical range of Democratic Presidential Vote. The confidence intervals for these predictions overlap at all levels of the covariate.

Although the model estimates are informative, it can be instructive to examine how well the model can predict partisan outcomes for each type of election. The model’s predictive power is of interest since some prior studies suggest that the partisan bias under indirect elections should lead them to over-respond to the preferences of the statewide electorate (King and Ellis, 1996). According to this argument, it should be easier to correctly classify the partisan outcome of a direct House elections than an indirect Senate election. Conversely, the fact that most senators were from the same party as the directly elected representative suggests that there should be little difference.

**Table 2.2.** Correctly Predicted Election Outcomes, 1836-1912

	All Cases (N)	Competitive States (N)
<i>Representative</i>		
Democrat	87.27 (110)	76.08 (46)
Whig/Republican	92.05 (151)	83.01 (53)
<b>Total</b>	<b>90.04</b> (261)	<b>79.80</b> (99)
<i>Senator</i>		
Democrat	87.72 (57)	66.67 (24)
Whig/Republican	94.28 (105)	91.67 (36)
<b>Total</b>	<b>91.97</b> (162)	<b>85.00</b> (60)

**Note:** Cell entries report the proportion of elections for which the partisan outcome was correctly classified.

In Table 2.2, I report the proportion of election outcomes that were correctly classified by both the type of election. The calculations, which are based on the estimates from the second model in Table 2.1, indicate that the outcome of indirect elections can be predicted with a substantively similar level of accuracy as direct election.<sup>15</sup> When

<sup>15</sup>I use the coefficient estimates from the second model because it only includes a measure of voter

only competitive states are examined, however, the predictive power is marginally better for indirect elections.<sup>16</sup> In competitive states, the model correctly predicted a senator's partisan affiliation in 85% of the elections while correctly predicting the party of the winning House candidate in 79.80% of elections. There were, however, more notable differences between the parties in competitive states. Although the total number of cases is relative small, the 25 point difference between the prediction for Democratic senators and Whigs/Republicans senators is still noteworthy.

## Legislative Behavior

While all previous theoretical accounts of indirect Senate elections posit that senators' legislative behavior will be responsive to the state legislatures, they offer differing expectations about the extent to which senators will respond to the statewide electorate. If senators respond primarily to the state legislature, then indirect elections should select a more ideologically extreme legislator than the one who would be chosen by direct election. This expectation is based on the assumption that the median voter in the state legislature is more ideologically extreme than the median voter in the statewide electorate (Bernhard and Sala, 2006). Empirically, this means that an indirectly elected senator should be more liberal (conservative) than the directly elected representative who wins a statewide popular vote. If, however, senators sought to establish a base of support independent of the state legislature (Schiller, 2006), then their behavior should also respond to the preferences of the statewide electorate. Under this scenario, there should not be a meaningful difference between legislators chosen under indirect and direct elections.

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preferences. Since my primary interest is whether the relationship between preferences and elections outcomes differs across the method of election, this model is closer to the comparison I want than the model that also controls for the state legislature. The model-based prediction for the third model do differ from those reported in Table 2.2. In this case, the model correctly predicts the outcome in 98.14% of indirect Senate elections and correctly classifies 89.66% of direct House elections.

<sup>16</sup>I define competitive states as those where the Democratic share of the two-party vote was between 45 and 55

**Table 2.3.** Common Space Scores (First Dimension) 1836-1912, OLS Regression

	Est. (S.E.)	Est. (S.E.)	Est. (S.E.)
Dem. Pres. Vote	-0.020* (0.002)	-0.016* (0.002)	-0.004* (0.001)
Senator	0.053 (0.034)	0.007 (0.025)	-0.000 (0.020)
Dem. Pres. Vote $\times$ Senator	0.002 (0.002)	0.002 (0.002)	0.002 (0.002)
Dem. Majority			-0.481* (0.031)
Constant	-0.019 (0.021)	0.001 (0.093)	0.223* (0.054)
$R^2$	0.277	–	–
$\sigma_{\text{Year}}^2$	–	0.030	0.015
$\sigma_{\text{Region}}^2$	–	0.030	0.009
$\sigma_{\text{Residual}}^2$	–	0.058	0.038
N	423	423	423

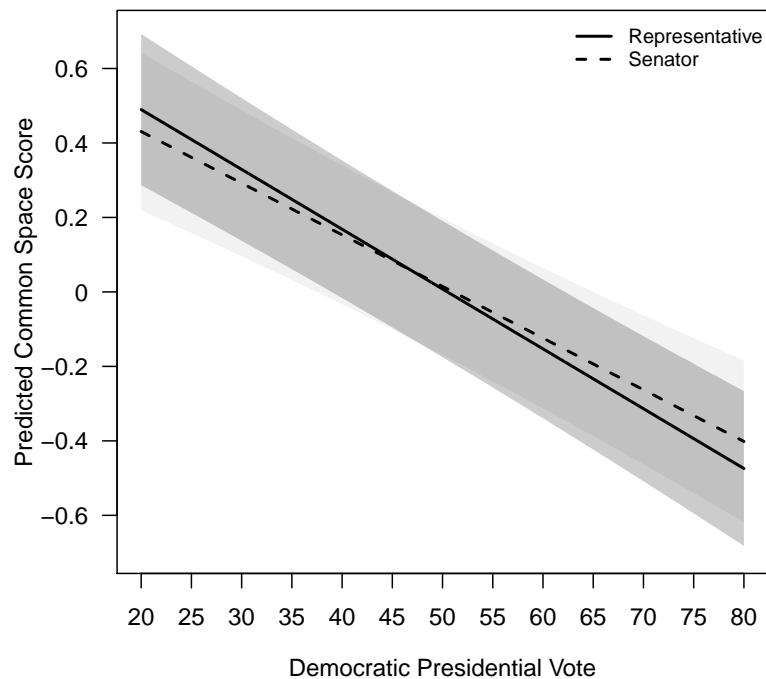
**Note:** Cell entries in the first column are from a OLS regression model with standard errors clustered by year reported in parentheses. Cell entries in the second and third columns are from a mixed effects linear regression model with region and year-specific intercepts.

\*  $p < 0.05$

I estimated a series of regression models where a legislator’s first dimension Common Space score is modeled as a function of statewide preferences, the type of election, and the interaction between these two predictions. The coefficient estimates from these regressions are reported in Table 2.3. As before, the interaction term is the key coefficient for evaluating differences between the two methods of election. A negative and statistically significant interaction term would provide evidence that indirect elections led to the selection of a more ideologically extreme legislator than did direct elections. In terms of an empirical observation, this expectation suggests that, holding the preferences of the statewide constituency constant, an indirectly elected Democratic senator will have a lower negative Common Space score than a directly elected

Democratic representative. Conversely, an indirectly elected Whig/Republican senator would be expected to have a high positive Common Space score than a directly elected Whig/Republican senator. An insignificant interaction term, however, would indicate that there were not systematic differences across the method of election. As the results in Table 2.3 make clear, the coefficient for the interaction term is actually positive and statistically insignificant in all three models.

**Figure 2.2.** Predicted Common Space Scores by Election Type



**Note:** The solid line and dark gray shading denote the predicted ideology and corresponding confidence interval for Representatives and the dashed line and light gray shading represent the predicted ideology and corresponding confidence interval for Senators.

In order to illustrate the results of this analysis, I used the coefficient estimates to predict Common Space scores for each type of legislator. These predictions are reported in Figure 2.2.<sup>17</sup> The predicted Common Space scores for indirectly elected senators (dashed line) is actually more moderate than the prediction for their directly

<sup>17</sup>As with the model-based predictions for election outcomes, I used the coefficient estimates from the second model to calculate these predictions.

elected counterparts (solid line). Although the prediction is counter to the expectation of some prior studies, the confidence intervals for each prediction overlap almost completely. Substantively, there are no differences, on average, across legislators chosen under each method of election.

## 2.6 Conclusion

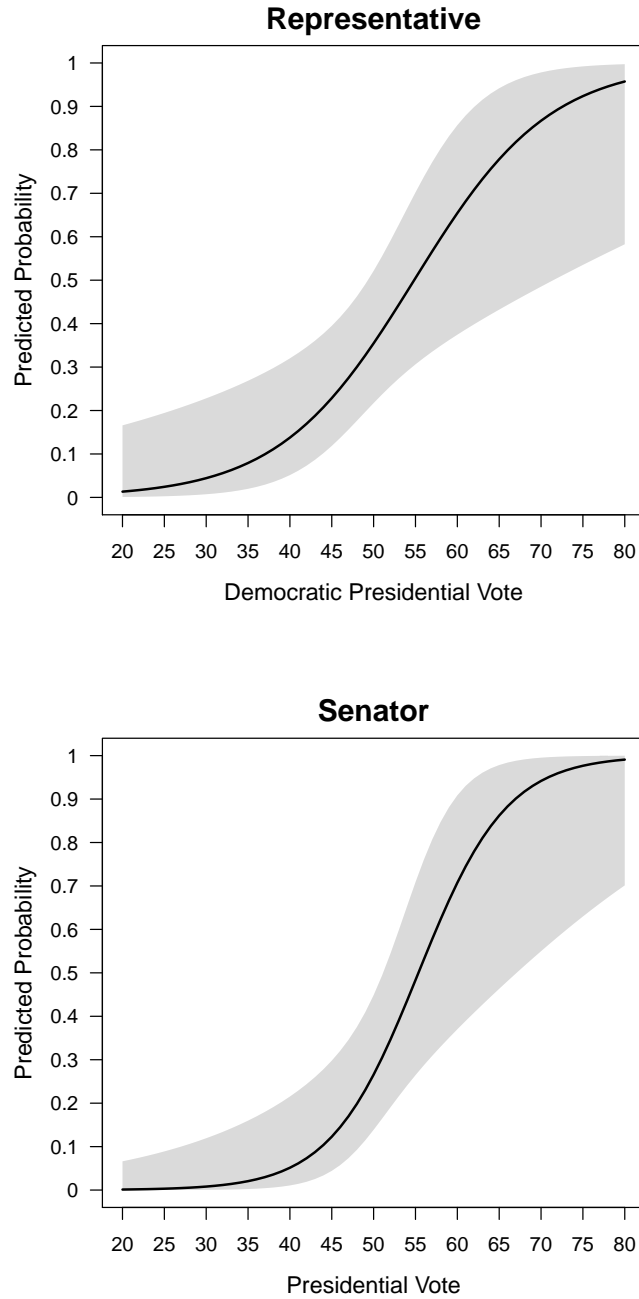
Based on the comparison of indirectly elected senators and directly elected representatives, there is little evidence that indirect elections were consistently less representative than direct elections. As such, there is little support for the claim that the adoption of direct elections made senators more like members of the House. Instead, it appears that indirect and direct elections produced similar partisan outcomes. The high rate of congruence is important since partisan affiliation has been correlated with preferences for the role of the central government throughout large portions of U.S. History (Jenkins, 1999; Poole and Rosenthal, 1997). As such, if partisan outcomes are consistent across both methods of election, then it is hard to pinpoint election method as a causal force.

These findings can also speak to contemporary debates about different electoral rules. In particular, the examination of the relationship between statewide preferences and legislative ideology are also important because they speak to recent work on primary elections. A comparison of indirect and direct elections is similar to the study of closed and open primaries in that both studies are interested in whether changes in the composition of the electorate influence the type of legislators that are chosen. The results presented here suggests support the conclusion that closed primaries do not lead to the selection of more ideologically extreme legislators (McGhee et al., 2014). One advantage of focusing on indirect Senate elections is that we are able to make comparisons within the same state and year rather than making comparisons

across states or focusing on a single state. As such, these results suggest that the theoretical mechanism outlined in prior studies is more broadly generalizable.

## Appendix A: Supplemental Results

Figure 2.3. Probability of Electing a Democrat, by Election Type



**Note:** The solid line and dark gray shading denote the predicted probability of a Democratic victory and corresponding confidence interval for each set of legislator.

**Table 2.4.** Senator and Representative Party Unity Scores (Standardized), 1836-1912

	<i>Democratic Party Unity</i>		
	<b>OLS</b>	<b>OLS</b>	<b>HLM</b>
Dem. Pres. Vote	0.049*	0.049*	0.015*
	(0.007)	(0.004)	(0.004)
Senator	-0.051	0.010	0.040
	(0.077)	(0.073)	(0.056)
Dem. Pres. Vote × Senator	-0.009	-0.009	-0.007
	(0.005)	(0.006)	(0.005)
Dem. Majority			1.426
			(0.079)
Constant	0.006	-0.003	-0.657*
	(0.098)	(0.169)	(0.074)
R <sup>2</sup>	0.291	–	–
$\sigma^2_{\text{Year}}$	–	0.101	0.047
$\sigma^2_{\text{Region}}$	–	0.091	0.006
$\sigma^2_{\text{Residual}}$	–	0.474	0.277
N	408	408	408

**Note:** Cell entries in the first column are from a OLS regression model with standard errors clustered by year reported in parentheses. Cell entries in the second and third columns are from a mixed effects linear regression model with region and year-specific intercepts.

\*  $p < 0.05$

## Appendix B: Representativeness of Sample

**Table 2.5.** Partisan Composition of House and Senate, Difference of Proportions

<b>House</b>	<b>Democrat</b>	<b>Senate</b>	<b>Democrat</b>
Sample	43.87	Sample	37.65
Population	46.89	Population	43.28
Difference	3.02	Difference	5.63

**Note:** Cell entries report the proportion of Democratic legislators for the subset used in the analysis and for the full population of legislators who served in each chamber during the time period examined.  
\*  $p < 0.05$

**Table 2.6.** Comparison of Common Space Scores by District Type

		<b>Democrats</b>		<b>Republicans</b>	
	<b>Whigs</b>	<i>Indirect</i>	<i>Direct</i>	<i>Indirect</i>	<i>Direct</i>
Statewide	-0.024 (0.038)	-0.029* (0.013)	0.034* (0.021)	-0.019 (0.016)	-0.063* (0.025)
Constant	-0.215* (0.054)	-0.520* (0.012)	-0.246* (0.021)	0.422* (0.034)	0.259* (0.034)
N	564	2477	1319	1875	1126
R <sup>2</sup>	0.718	0.421	0.308	0.075	0.078

**Note:** Cell entries are coefficients from an OLS regression model with Huber-White corrected standard errors in parentheses.

\*  $p < 0.05$

## Chapter 3

# Electoral Proximity and Constituent Evaluations of U.S. Senators

In December 2009, the United States Senate began consideration of the Patient Protection and Affordable Care Act (ACA). Although the Senate Democrats held a filibuster proof majority of 60 seats, they needed every member to vote the party line in order to pass the bill. Party unity did not, however, come without its own set of costs. Democratic leaders had to make deals in order to secure the votes of electorally vulnerable members. The most infamous deal, known as the "Cornhusker kickback", targeted Senator Ben Nelson (D-NE) and exempted Nebraska from paying the full cost of expansions to the Medicaid program.

Although the exemption was the primary focus of national media coverage of Nelson's involvement with the legislation, his actions before, during, and after consideration of the ACA are instructive for broader issues of representation and legislative accountability. First, in the months leading up to Senate consideration of the bill, Senator Nelson held meetings with constituents across Nebraska to explain why health

care reform was needed and how it would benefit Nebraskans (Wetzel and Giboney, 2009). Throughout his tour, Nelson interacted directly with constituents and heard their opinions before casting a single vote on the ACA.

Second, in the days before the vote on the ACA, Nelson's offices in Washington D.C. and Nebraska were inundated with phone calls from constituents to the point that the phone lines in both offices were jammed (Morton and Tysver, 2009). Third, after the vote on the ACA, Senator Nelson purchased television advertisements to explain his vote and rebuff claims that the legislation was a government takeover of health care (Walton, 2009).<sup>1</sup> Lastly, Senator Nelson, who no doubt realized his vote was unpopular in Nebraska, voted against the reconciliation bill that finalized health care reform.

Senator Nelson's actions are consistent with the type of legislative-constituent interaction we might expect, especially on major legislation. Senator Nelson's behavior and the response from his constituents are notable in part because he was not up for re-election for nearly three more years. In this way, both Senator Nelson's response and his constituents' awareness of his potential and actual legislative behavior conflicts with previous theoretical treatments of U.S. senators. These studies posit that the linkage between senators and constituents, particularly constituents' attention to senators' behavior, varies as a function of proximity to election (Ahuja, 1994; Bernstein, 1991; Elling, 1982; Lindstadt and Vander Wielen, 2011; Stratmann, 2000; Thomas, 1985; Wright and Berkman, 1986). As a result, senators are assumed to have greater behavioral discretion early in their term when constituents do not actively monitor their behavior.

The assumption of time-dependent monitoring implies that the criteria constituents use to evaluate senators varies as a function of whether or not a senator is up for re-

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<sup>1</sup>Senator Nelson's advertisements were released a few days after a Rasmussen Reports' poll found him trailing Republican Governor Dave Heineman 61 to 30 percent in a hypothetical Senate race (Walton, 2009). While there is no definitive proof that Senator Nelson's decision was triggered by the poll, the timing is certainly suggestive.

election. These differences are attributed to the information about incumbents that emerges during their re-election campaigns (Abramowitz and Segal, 1992; Krasno, 1994). Other work, however, finds few, or at least fewer, differences in constituent monitoring over the election cycle (Franklin, 1991, 1993). I use the Cooperative Congressional Election Study (CCES) surveys from 2010 and 2012 to revisit this important question. Specifically, I examine whether electoral proximity influences how legislator-constituent agreement on key legislative proposals translates into evaluations of senators' job performance. In order to conduct this empirical test, I focus on states where an incumbent senator ran for re-election because it allows me to compare evaluations of senators who are and are not up for re-election.

The remainder of this paper proceeds as follows. I begin by reviewing prior research on the relationship between election frequency and legislative behavior. I then outline the different ways in which elections are expected to condition constituents' evaluations of their elected representatives. Next, I discuss why job approval can serve as a reasonable alternative to election-based measures of constituent evaluations. I also discuss why job approval should condition the behavior of re-election oriented senators. I then discuss the data and methods used to test the claim that elections facilitate evaluations of senators. The results of my analysis suggest that electoral proximity does not influence how (dis)agreement on real legislative issues is translated into evaluations of a senator's job performance.

### **3.1 Election Frequency and Legislative Behavior**

Elections are the primary mechanism through which constituents can reward or punish their elected representatives. Indeed, the relationship between legislators and constituents is primarily "electoral in nature" with constituents' votes serving as "blunt instruments of accountability" (Carson and Jenkins, 2011, 27). It should not

be surprising then that the conditions under which legislators are held accountable on election day has been of considerable interest to scholars of congressional politics (Canes-Wrone, Brady and Cogan, 2002; Carson et al., 2010; Erikson, 1971).

Given the importance of elections to the legislator-constituent relationship, the duration between elections can be a crucial factor in constituents' ability to exercise accountability. In *Federalist* 53, James Madison posits that when deciding upon the length of a term in office, it is important to balance responsiveness against the practicality of the rule. Madison notes that while some colonial legislatures were elected every six months, their governments functioned no better or no worse than the government in states who held legislative elections every two years. Madison concludes that semi-annual elections provide no advantages over biennial elections if the latter leads to a comparable level of representation.

Although two year terms might be sufficient when compared against six month terms, it can be harder to argue that the Senate's six year terms provides the same level of responsiveness as the House of Representatives' two year terms.<sup>2</sup> The House's biennial elections correspond to a single congressional term, which provides a natural focal point for voters to evaluate incumbents. In contrast, incumbent senators have a paper trail that spans three congresses since their last election. If constituents' evaluations and vote choice are based on the most recent information or considerations (Lodge, Steenbergen and Brau, 1995; Zaller, 1992), senators may be evaluated and held accountable for only one-third of their legislative behavior during a given term.

The potential for temporal variation in constituents' monitoring of senators provides the main theoretical motivation for research on the relationship between electoral proximity and legislative behavior. In these studies, senators are assumed to have more behavioral discretion early in their terms, which is posited to allow sena-

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<sup>2</sup>According to Swift (1996), the Senate's original design was in fact intended to be less responsive than the House. Specifically, Swift argues that the Senate was designed to "limit democratic political power by establishing an upper chamber that could, and they hoped would, dominate the House and the people whom the House represented" (11-12).

tors to shift their ideological positions over the course of a six-year term (Ahuja, 1994; Elling, 1982; Lindstadt and Vander Wielen, 2011). Specifically, senators are said to take more ideologically extreme or partisan positions early in a term and then adopt moderate positions as election day nears (Bernstein, 1991; Elling, 1982; Stratmann, 2000; Thomas, 1985; Wright and Berkman, 1986).<sup>3</sup>

While the assertion that six-year terms allow senators to deviate from constituent preferences early in a term has an intuitive appeal, there are reasons to question this claim. First, the evidence of intra-term changes in voting behavior is far from conclusive. Poole (2007) finds that estimating congress-specific ideal points rather than a single ideal point for each legislator produces only a one percentage point improvement in vote classification and comes at the expense of estimating several thousand extra parameters. Based on these results, Poole (2007) concludes that “once elected to Congress, members adopt an ideological position and maintain that position throughout their careers...” (435). Similarly, Poole and Rosenthal (1997) find that the over-time variability in individual legislator’s ideal points has been in a consistent decline since the end of the Civil War. Kingdon (1989) reaches a similar conclusion and argues that legislators have an incentive to develop consistent voting records because it can be “difficult to explain inconsistencies to their constituents” (277). Former Senator John Kerry learned this lesson first hand during the 2004 presidential campaign when he was unable to shake accusations of an inconsistent voting record.

Second, even if a senator does not face immediate electoral consequences for taking the “wrong” position on a vote early in his or her term, it does not mean that these votes are politically inconsequential. If a particular vote has fallen out of the public’s

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<sup>3</sup>One concern with these studies is that they do not account for changes in the legislative agenda over time. Changes in the legislative agenda are particularly troubling in this context because some of these studies use measures based on a subset of votes that vary in number and content over time (e.g. interest group scores). See Madonna (2011) for a more detailed discussion about the importance of controlling for changes in the legislative agenda.

attention, a senator's opponent or an interested third party can bring it back to their attention prior to the election (Arnold, 1990). Similarly, constituents and third party groups can voice their opposition to a particular vote when it occurs and influence senator's behavior in real time. Senator Nelson's change in position between the initial vote on the ACA and the vote on the reconciliation measure illustrate this point. In some ways, senators face a more difficult challenge than their counterparts in the House because they have six years of votes to defend. Senators may therefore have to justify *multiple* votes on a particular issue whereas a member of the House may only have to defend a single difficult vote. As Kingdon (1989) observes, it is harder to explain away a series of votes, or inconsistent positions, than a single miscue.

Lastly, there is little reason to expect senators to be more inclined to cast electorally difficult votes simply because they are not immediately up for re-election. That is to say, the timing of a vote should matter less than its substance. As such, re-election oriented senators should be sensitive to the electoral implications of votes at all points in a term (Arnold, 1990; Mayhew, 1974*b*). Furthermore, unpopular or difficult votes can be consequential for and influence evaluations of a senator's job performance. During the 113th Congress, a number of senators saw their job approval ratings fall by 10 to almost 20 points after opposing a proposal to expand background checks for gun sales (Sink, 2013). These type of dramatic changes in support should be of particular concern for re-election oriented senators because their standing with the public is related to future election outcomes (Highton, 2008).

## 3.2 Elections and Citizens' Evaluations

The argument that constituents are less aware of a senator's legislative records when he or she is not up for re-election implies that elections should influence how constituents evaluate a senator. Prior research generally attributes these differences to an

enhanced focus on an incumbent's job performance during the campaign (Abramowitz and Segal, 1992; Krasno, 1994). Abramowitz and Segal (1992) provide a clear articulation of this assumption.

For the first approximately five-and-a-half years of their terms, Senators enjoy a virtual monopoly in explaining their performance to the voters. . . . It is only during the few months of the campaign that the voters are likely to receive any information critical of the incumbent's performance in office (231).

Under this theoretical framework, elections influence constituents' evaluations because they affect the type of information that is available. Namely, weaker constituent monitoring early in a term makes it more difficult for a constituent to effectively evaluate a senator.

The assumption of weaker monitoring raises two important questions. First, do elections in fact facilitate evaluations? That is to say, are there differences in constituent's ability to evaluate senators who are and are not up for re-election? Second, is there any evidence in the CCES surveys that elections made it easier to evaluate senator's job performance?

## **Do Elections Facilitate Evaluations?**

In order to answer the first question raised above, it is helpful to start with the previous literature. Krasno (1994) uses data from the 1988 and 1990 Senate Election Study (SES) to test this proposition. Although Krasno compares several categories of senators, the most relevant pairing for the current discussion is the comparison of senators from the same state – one of whom is up for re-election and one who is not. Krasno finds the largest differences in evaluations between these two groups, approximately 13 percentage points, with respect to name recall, but finds little difference in name recognition (138). The other evaluative difference can be seen in the proportion of respondents who respond with “Do Not Know” when asked

to evaluate a senator. The differences in respondents' ability to rate their feelings toward senators or provide "likes" and "dislikes" about each senator are relatively modest, around two and four percentage points respectively. There was, however, an approximately seven percentage point gap in the proportion of respondents' who could rate each senators' job performance.

Franklin (1993) also uses the 1988 and 1990 SES, but instead compares senators' "visibility" over the election cycle. Franklin finds modest differences in a senator's name recognition, constituents' recall of seeing the senator in the media, and constituents' ability to rate senators.<sup>4</sup> Furthermore, he finds that constituents' familiarity with incumbents generally increases over the course of a senator's career. Franklin also finds substantial changes in respondents' ability to provide "likes" or "dislikes" about a senator over the election cycle, which he also attributes to the campaigns.

Both Franklin (1993) and Krasno (1994) find that although constituents' recognition of senators changes only modestly over the election cycle, their ability to offer an evaluation of senators' job performance can be more variable. Differences in constituents' ability to evaluate legislators could be problematic for comparing senators who are and are not up for re-election. If constituents cannot assess these groups of senators at similar rates then a comparison of these two groups becomes less tenable.

## **Electoral Proximity and Evaluations in the CCES Surveys**

While Franklin (1993) and Krasno (1994) use the SES studies, the present analysis relies on the newer CCES surveys.<sup>5</sup> As such, it is important to revisit the findings of these earlier studies to determine if similar concerns might plague the current

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<sup>4</sup>Krasno's (1994) comparison of same state senators who are and are not up for re-election largely comports with Franklin's findings. The largest differences between these groups of senators is with respect to whether or not a respondent saw the senator on television, a six-point difference, and whether or not the respondent received mail from the senator, a nine-point difference (145).

<sup>5</sup>One of the primary advantages of the CCES over the SES is that the former asks respondents how they would vote on actual roll calls from the most recent congress. These questions make it possible to measure constituent-senator preference congruence.

analysis. In order to examine whether constituents' evaluations of senators varies over electoral context, I looked at the subset of states where an incumbent was running for re-election. Specifically, I calculated the proportion of respondents who were able to rate the job performance of the senator who was, and the senator who was not, up for re-election.

**Table 3.1.** Proportion of Respondents Rating Senator's Job Performance

<b>Year</b>	<b>Election (%)</b>	<b>N</b>	<b>No Election (%)</b>	<b>N</b>
2010	93.53	21039	91.06	21039
2012	87.59	29074	86.26	29074

**Note:** Cell entries report the proportion of respondents who were able to rate a senator's job performance across electoral context.

As Table 3.1 makes clear, there is little difference in constituents' ability to evaluate senators who are and are not up for re-election. In the 2010 CCES, 93.5% of respondents were able to give an approval rating for a senator who was up for re-election while 91.06% were able to do so for a senator who was not up for re-election. In the 2012 CCES, the proportion of constituents who could rate this same set of senators was 87.59 and 86.26 respectively. While there are differences across electoral context, they are only 1 to 2 percentage points, which is not substantively meaningful.

The evidence needed to answer the first question posed above appears to be mixed. While prior studies suggest that elections make it easier for constituents to evaluate senators, this may be a function of the survey used in prior studies. The SES data is now over 20 years old and much has changed in terms of both political context and technology since this time. Indeed, the newer CCES data does not support the findings of prior research. Respondents from the 2010 and 2012 CCES surveys were able to evaluate a senator's job performance at comparable rates regardless of whether a senator was up for re-election.

The figures reported in Table 3.1 are therefore of note for a number of reasons.

First, these figures help to justify the research design proposed in this study. If constituents are not able to evaluate senators at similar rates then we might be worried that potential censoring of observations might impact any inferences we might make. Second, the differences between the SES and CCES do not suggest that prior research is wrong, but may instead pick up on changes in political context or technology. This latter point will be particularly pertinent to keep in mind when interpreting the empirical results presented below.

## Approval and Strategic Senators

Election outcomes are one of the most common metrics by which political scientists measure constituents' evaluations of legislators. Indeed, prior studies use either vote margins (Canes-Wrone, Brady and Cogan, 2002; Carson et al., 2010; Erikson, 1971; Shaffer and Chressanthi, 1991) or survey data on vote choice (Ansolabehere and Jones, 2010; Jones, 2011) to assess how constituents hold legislators accountable. The focus on election outcomes is logical since elections are the only way to remove a senator who is out of step with his or her constituents. Elections are not, however, the sole mechanism through which constituents can express their approval or disapproval of a senator. Citizens' evaluations of senatorial job performance, as captured through public opinion polls, provide another useful metric.

To be clear, the argument is not that job approval ratings function the same as an election, but rather that it can provide important feedback that can condition a senator's legislative behavior.<sup>6</sup> Indeed, political scientists frequently use approval ratings to examine how voter and constituent evaluations influence political outcomes (Grant and Rudolph, 2004; Highton, 2008; MacKuen, Erikson and Stimson, 1992). The intuition in this particular case is that re-election oriented senators should use

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<sup>6</sup>Prior research indicates that favorable evaluations can be more easily translated into votes in subsequent elections than can unfavorable evaluations (Cover and Brumberg, 1982; Mann and Wolfinger, 1980).

new information to update their beliefs about constituent preferences (Stratmann, 2000).<sup>7</sup> Since legislators' perceptions of constituent preferences influence decision making (Arnold, 1990; Kingdon, 1989; Miller and Stokes, 1963), new information about job approval should provide an important cue about what behavior is in-line with constituent preferences.

Job approval ratings are therefore helpful for understanding senatorial behavior because they should inform and condition future actions. Specifically, senators should update their behavior on the basis of new information about constituent preferences. In this context, updating can occur in one of two ways. First, in the presence of unfavorable or falling approval ratings, senators should change their behavior in order to bring it more in-line with constituent preferences.<sup>8</sup> Second, senators who are viewed unfavorably should be more likely to strategically retire rather than face a possible electoral defeat (Carson, 2005; Highton, 2008).

Focusing on senatorial approval has an additional advantage, namely that it allows for a comparison of whether an individual's evaluations of their senators varies as a function of electoral context. With election-based measures, comparisons of senators from the same state can only be made across time, which can make it more difficult to control for all of the relevant political or contextual variables that might also influence evaluations. In contrast, job approval can be measured for both senators at the same time, which makes it easier to rule out contextual variables as a cause of any observed differences in evaluations.

The research question in this case is whether senator-constituent agreement on important roll calls has the same influence on job approval for senators who are and are not up for re-election. In order to test for the effect of electoral proximity, I examine states where an incumbent is running for re-election. Since only one of a

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<sup>7</sup>The assumption that re-election is the proximate goal for senators seems reasonable since three quarters or more of Senate incumbents whose terms are up ultimately run for re-election (Jacobson, 2013).

<sup>8</sup>Senator Nelson's decision to vote against the reconciliation bill is instructive on this point.

state's two senators is up for re-election at a time, these states make it possible to compare the same individual's evaluations of the senator who is up for re-election and the senator who is not up for re-election (Franklin, 1991). Of particular interest is whether elections enhance the relationship between preference congruence and job approval. Such a relationship would be problematic if, as is assumed above, job approval helps senators update their beliefs about their constituents' most preferred positions. A weaker relationship between preference congruence and the approval of senators who are not up for election would suggest that constituents' disapproval is not always translated into their assessments. As a result, these senators may not always receive adequate information to effectively update their beliefs and more closely represent their constituents' interests.

## **Data and Methods**

The data for this analysis come from the 2010 and 2012 CCES surveys. These surveys are ideal because they provide two key pieces of information. First, the CCES asks respondents to evaluate the job performance of each of their state's two senators, which means I have respondents' evaluations of senators who are and are not up for election. Second, respondents are asked how they would vote on a series of roll calls that occurred in the most recent Congress. A respondent's answers to these questions makes it possible to construct a measure of roll call agreement between the respondent and senator (Ansolabehere and Jones, 2010; Jones, 2011; Lawrence, Binder and Maltzman, 2011). With these two pieces of information it is possible to examine whether electoral proximity influences how constituent-legislator (dis)agreement translates into evaluations of senatorial job performance.

## Outcome Variable

The CCES survey asks respondents whether they approve or disapprove of the job each current U.S. senator is doing. For the 2010 and 2012 CCES surveys, the possible responses were Strongly Approve, Approve, Disapprove, and Strongly Disapprove.<sup>9</sup> While I could treat this measure as an ordinal variable, a binary measure provides a better approximation of how senators likely think about their approval and how approval ratings are reported by the media. In this way, the binary measure relates more closely to how people think and talk about real world politics. I therefore coded the outcome variable, which I refer to as Approve, as 1 if a respondent approved of a senator's job performance and 0 otherwise.

For both the 2010 and 2012 CCES surveys, there was considerable variation in the proportion of respondents who stated that they approved of their senator's job performance. In the 2010 survey, the median approval rating for the senators included in the analysis that follows was 50.67% while the minimum and maximum approval ratings were 26.63% and 80.68% respectively. In the 2012 survey, the median approval rating was 57.99% while the minimum and maximum approval ratings were 42.08% and 74.00% respectively.

## Predictors

One consideration with using the CCES questions in this analysis is that the question or questions used to construct covariates should measure the respondent's positions or opinions relative to an individual senator. Since the outcome variable is the respondent's evaluation of the senator's job performance, the immediate interest is not the connection between respondent's evaluation of broader political or economic context (e.g. approval of Congress or economic evaluations), but simply

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<sup>9</sup>Respondents could also choose the option "Never Heard Of." I treat these cases as missing values when coding the approval measure. The substantive conclusions are not altered if I impute approval ratings for these respondents.

the constituent-senator relationship. The number of predictors used in this analysis is limited, but is appropriate for the research question.

The key predictor, which I refer to as Roll Call Agreement, captures the agreement between a respondent and senator on a series of votes from the most recent Congress. As discussed above, the CCES asks respondents how they would have voted on a number of actual legislative proposals in the most recent Congress. Since these issues can be matched with roll call votes, we can determine how often a senator and respondent agree (Ansolabehere and Jones, 2010; Jones, 2011; Lawrence, Binder and Maltzman, 2011). A measure based on a senator's real world legislative behavior, rather than broader issues, is ideal in this case since the outcome variable is the respondent's assessment of a senator's job performance. Similarly, the measure is advantageous because it is not based on perceptions of a senator. While a respondent's perceived ideological agreement with a senator is certainly important, it is arguably more important to determine whether elections influence a constituent's ability to translate real world (dis)agreement into an assessment of job performance.

In order to construct the measure of Roll Call Agreement, I follow the coding scheme first used by Ansolabehere and Jones (2010). Each roll call vote is coded as follows: +1 if the respondent and senator took the same position, 0 if the respondent did not have a position, and -1 if the respondent and senator disagree.<sup>10</sup> After each individual vote was coded, I then calculated the average across each vote, which is simply the sum of each individual vote divided by the total number of votes. The resulting variable can take values over the range from -1 (perfect disagreement) to 1 (perfect agreement). Respondents with higher agreement scores should be more likely

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<sup>10</sup>One potential concern with this coding procedure is that a value of 0 could reflect either a true split between agreement and disagreement or it could mean the respondent failed to take a position on any of the roll call questions. In order to address this concern, I only used cases in which the respondent answered at least half of the roll call questions. For example, in the 2010 CCES there were six roll call questions on which there was also a recorded vote in the Senate. A respondent would therefore have to answer at least three of the roll call questions in order to be included in the analysis.

to hold favorable views of a senator’s job performance.

In Table 3.2, I report summary information about Roll Call Agreement for senators who are up for re-election, senators who are not up for re-election, and for all cases. Of particular interest is whether there are substantial differences in the measure across electoral context. The calculated agreement scores from the 2010 survey is nearly identical across electoral context with the mean and standard deviation only differing by 0.01 and 0.02 respectively. The mean and standard deviation of agreement scores from the 2012 survey are essentially identical through at least the second decimal point. In short, the distribution of the key predictor for this analysis does not itself appear to be affected by electoral status. As such, we can have greater confidence that the results of the empirical analysis capture the effect of electoral status.

**Table 3.2.** Roll Call Agreement by Electoral Status

	Mean	SD	N
<i>2010</i>			
Election	0.09	0.76	18037
No Election	0.10	0.78	18037
All Cases	0.09	0.77	18037
<i>2012</i>			
Election	-0.01	0.35	23854
No Election	-0.01	0.36	23854
All Cases	-0.01	0.36	23854

**Note:** Cell entries report the mean and standard deviation of Roll Call Agreement for each category of electoral context and for all cases used in the empirical analysis below.

I also include two additional control variables, Perceived Party Agreement and Ideological Agreement, both of which are also coded to match Ansolabehere and Jones (2010). Both of these variables are included to control for a respondent’s perception of a senator. While my primary interest is how roll call agreement influences evaluations of senator’s job performance, it is still important to account for a respondent’s perceptions of a senator (Ansolabehere and Jones, 2010). In each case, the covariate

is derived in part from a question that asks each respondent to identify a senator's partisan affiliation.

Perceived Party Agreement is coded +1 if a respondent identified a senator as being of the same party as him/her, 0 for respondents who did not identify with a party or did not know their senator's party, and -1 if a respondent identified a senator to be of a different party. Respondents who believe they share the same partisan affiliation with their senator should be more likely to approve of that senator. Ideological Agreement captures whether a respondent's stated ideological position matches his or her perception of a senator's party affiliation. If a respondent identifies as a conservative (liberal) and believes that his or her senator is a Republican (Democrat) then the observation is coded as +1. If, however, a respondent identifies as a conservative (liberal) and believes his or her senator is a Democrat (Republican) then the observation is coded as -1. Respondents who did not know their senator's partisan affiliation, believed their senator was an Independent, or who identified as a moderate are coded as a 0. Respondents whose ideological leanings match their perception of a senator's party affiliation should be more likely to approve of that senator.

## **Model**

There are two important considerations with respect to selecting an empirical model. First, there are multiple observations per senator, which means that the data are clustered by senator. Second, as was discussed above, the mean level of job approval varies across senators and should be accounted for in the empirical model. Each of these features of the data can easily be accounted for in a multilevel modeling framework (Gelman and Hill, 2007).

Another advantage of the multilevel model framework is that it allows for a straightforward test of whether the relationship between roll call agreement and job approval varies over electoral context. Specifically, the coefficient for Roll Call Agree-

ment can be allowed to vary across senators and the effect of electoral context can be assessed by including a indicator variable that denotes whether or not the senator is up for re-election as a level two predictor. The coefficient estimate for this indicator variable serves as the key test of whether elections influence how constituents evaluate senators. If elections do in fact make it easier for constituents to evaluate senators, then this coefficient should be positive and have a credible interval that does not include 0. An estimate that is either negative or has a posterior distribution whose density covers regions on both side of 0 would not provide support for the claim that elections facilitate evaluations.

In order to examine this question, I utilize a Bayesian multilevel logistic regression model with varying intercepts and varying slopes for the coefficient for Roll Call Agreement. The model can be formally represented as follows:

$$\begin{aligned} \Pr(\text{Approve}_{ij} = 1) &= \text{logit}^{-1}(\alpha_j + \beta_j \text{R.C. Agree}_i + \gamma X_i) \\ \alpha_j &\sim \mathcal{N}(\mu_\alpha, \tau_\alpha) \\ \beta_j &\sim \mathcal{N}(\delta_0 + \delta_1 \text{Election}_j, \tau_\beta) \\ \gamma &\sim \mathcal{N}(0, 100) \\ \mu_\alpha &\sim \mathcal{N}(0, 100) \\ \tau_\alpha &\sim \text{Gamma}(0.1, 0.1) \\ \tau_\beta &\sim \text{Gamma}(0.1, 0.1) \end{aligned}$$

Here  $\alpha_j$  represents the senator-specific intercepts,  $\beta_j$  represents the senator-specific coefficients for Roll Call Agreement,  $\gamma$  is the vector of coefficients for the remaining predictors, and  $\delta_0$  and  $\delta_1$  are the coefficients from the level two equation for the varying coefficient term. It is important to note that  $\delta_0$  represented the level one estimate for Roll Call Agreement while  $\delta_1$  represents the effect of Election.

### 3.3 Results

Before discussing the full model results, it can be instructive to first examine whether the relationship between Approve and Roll Call Agreement is comparable across electoral status. In order to answer this question, I calculated the mean agreement score for respondents who approved and disapproved of a senator’s job performance for both categories of Election. The mean agreement score for each category of electoral status and approval are reported in Table 3.3 for both the 2010 and 2012 surveys.

**Table 3.3.** Mean Roll Call Agreement by Job Approval

	Approve	N	Disapprove	N
<i>2010</i>				
Election	0.60	9027	-0.43	9010
No Election	0.63	8924	-0.42	9113
<i>2012</i>				
Election	0.04	13292	-0.06	10562
No Election	0.06	13487	-0.11	10376

**Note:** Cell entries report the mean Roll Call Agreement for respondents who approved and disapproved of a senator’s job performance by each category of Election.

Overall, there are only minimal differences in the mean agreement score across electoral status. There are no categories for which the difference in mean agreement across electoral status is more than 0.05, which is small given that the range of the covariate is -1 to 1. Although the mean agreement scores were farther from 0 in 2010 than in 2012, the difference between respondents who approve and disapprove is statistically significant for both years.<sup>11</sup> More importantly, the mean agreement score for respondents who approve and disapprove are always on the “correct” side of 0 for both 2010 and 2012.

In Table 3.4, I report the estimates of a Bayesian Multilevel logistic regression that

<sup>11</sup>These differences can, however, be attributed in part to differences in the questions that were asked in each survey (see Appendix A).

corresponds to the model discussed above.<sup>12</sup> I estimated two models for each year of the survey. The first model only includes respondents who answered the approval question for both senators. As noted before, these individual respondents are ideal for this analysis because they make it possible to compare the same individual's evaluations of the senator who is up for re-election and the senator who is not up for re-election (Franklin, 1991). Restricting the sample in this way, however, means that some respondents will be dropped from the analysis even though they evaluated one of the two senators. As shown in Table 3.1, some of these dropped cases will include respondents who evaluated the senator who was up for re-election, but not the senator who was not up for re-election. One potential concern then is whether the exclusion of these cases influences the inferences drawn from the resulting analysis.

In order to address this point, the second model includes all cases in which the respondent answered all of the questions needed to code the predictors, but failed to evaluate a senator's job performance. In a Bayesian framework, these cases can be included in the analysis because missing data in the outcome variable can be treated as a random variable that we can "learn" about through the sampling procedure (Jackman, 2000). The practical implication is that these cases need not be dropped because the Bayesian MCMC sampling algorithm will impute values for these missing cases at each iteration.

The second set of model results are of interest as both a robustness check on the first model and for more substantive reasons. As a robustness check, the second set of results indicates whether the inclusion of these additional respondents changes our substantive interpretation. Substantively, these results can speak to the quality of the information senators receive from opinion polls about their job performance. Berinsky (2004) suggests that respondents who can offer an opinion on a survey question will, at times, differ from those who cannot. If the inclusion of respondents

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<sup>12</sup>The Gelman-Rubin diagnostic statistic converged to 1 for all coefficients, which indicates that there was no evidence of non-convergence (Gelman et al., 2013).

who did not answer the job approval question changes our substantive conclusions, then public opinion polls may not provide adequate information for senators to update their beliefs about constituent preferences. For this analysis, the primary interest is whether the inclusion of respondents who could not rate a senator’s job performance influences the coefficient estimates for either Roll Call Agreement or Election.

**Table 3.4.** Approval of Senators, Bayesian Multilevel Logit Regression Models

	<i>2010</i>		<i>2012</i>	
	<b>Mean</b> <b>(95% C.I.)</b>	<b>Mean</b> <b>(95% C.I.)</b>	<b>Mean</b> <b>(95% C.I.)</b>	<b>Mean</b> <b>(95% C.I.)</b>
<i>Level One</i>				
Roll Call Agreement	1.94 (1.39, 2.51)	1.94 (1.38, 2.51)	0.44 (0.09, 0.77)	0.44 (0.10, 0.77)
Party Agreement	0.94 (0.89, 0.99)	0.94 (0.89, 0.99)	1.06 (1.02, 1.09)	1.06 (1.02, 1.09)
Ideological Agreement	0.55 (0.47, 0.60)	0.55 (0.50, 0.60)	1.24 (1.20, 1.27)	1.24 (1.20, 1.27)
Constant	-0.25 (-0.46, -0.04)	-0.25 (-0.46, -0.03)	0.34 (0.21, 0.46)	0.34 (0.21, 0.48)
<i>Level Two</i>				
Election	-0.21 (-1.01, 0.57)	-0.21 (-1.01, 0.57)	-0.01 (-0.49, 0.48)	-0.02 (-0.48, 0.46)
<i>Random Effects</i>				
$\sigma_\alpha$	0.67 (0.53, 0.85)	0.67 (0.53, 0.85)	0.43 (0.34, 0.55)	0.43 (0.34, 0.55)
$\sigma_\beta$	1.25 (0.97, 1.62)	1.25 (0.98, 1.62)	0.73 (0.54, 0.96)	0.71 (0.53, 0.94)
N	36074	39202	47708	49092
DIC	25073.00	25072.90	42868.90	43042.40

**Note:** Cell entries are the posterior means with 95% Credible Intervals in parentheses. Results are based on a post-burn-in MCMC sample of 80000 iterations each on two chains with a 40000 iteration burn-in.  $\sigma_\alpha$  represents the random effect for the varying intercepts while  $\sigma_\beta$  represents the random effect for the varying coefficient for Roll Call Agreement.

The first column of Table 3.4 reports the coefficient estimates for the 2010 survey respondents who rated both of their senators’ job performance. As expected, Roll Call Agreement is positively related with job approval and the 95% credible interval does

not cross 0. It is important to remember that this estimate is a weighted average of the senator-specific coefficients since the coefficient was allowed to vary across senators.<sup>13</sup> Indeed, the random effect for this varying coefficient,  $\sigma_\beta$ , indicates that there is considerable variation in the senator-specific estimates.

The estimate for Election indicates that electoral proximity does not explain the observed variability in the senator-specific coefficients in the manner expected if elections facilitate evaluations of senators. Indeed, approximately 70% of the posterior density for Election is negative. While the majority of the posterior is negative, the 95% credible interval does cross 0. In short, there is little evidence that electoral proximity influenced 2010 survey respondents' ability to translate roll call (dis)agreement into an assessment of a senator's job performance.

In order to help make the above point more clear, Figure 3.1 summarizes the posterior densities for each senator-specific coefficient. The posterior mean for each senator-specific coefficient is represented by a dot and the error bars plot the 95% credible interval for each of the varying slopes. In total, 39 out of the 42 varying slopes are positive and have credible intervals that do not contain 0, which is the expected effect of agreement. More importantly, Figure 3.1 makes it quite clear that there were few differences in the posterior means across electoral context. Indeed, many of the posteriors distributions for the senator-specific estimates have 95% credible intervals that contain 1.94, which is denoted by the dashed line in the first panel of Figure 3.1.

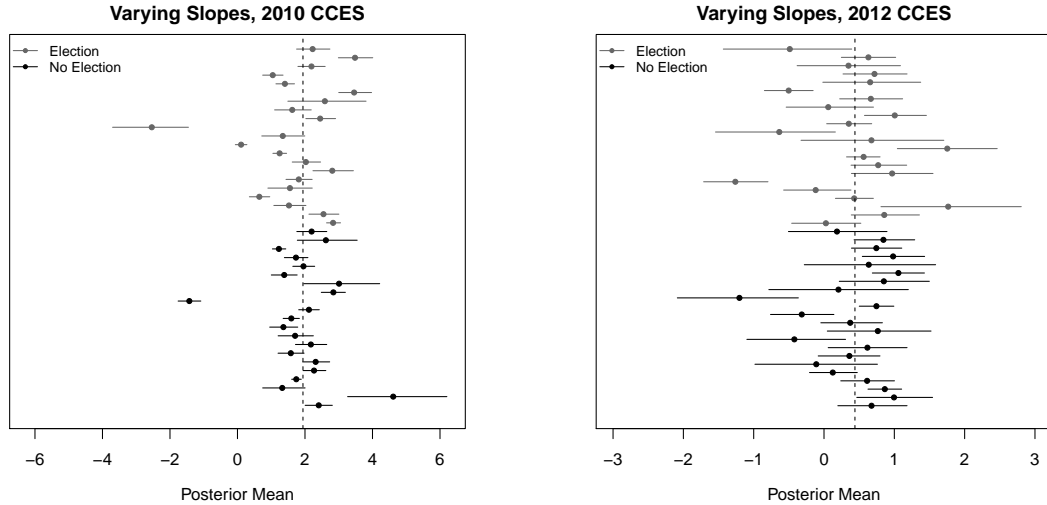
The three senators with coefficient estimates that do not fit with expectations are not necessarily surprising. For both Sen. Lisa Murkowski (R-AK) and Sen. Lindsey Graham (R-SC), the coefficient for Roll Call Agreement is negative and the 95% credible interval does not cross 0. During the 2010 election, Sen. Murkowski ran in the general election as a write-in candidate after failing to secure re-nomination in her party's primary. Similarly, Sen. Graham, who was not up for re-election in

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<sup>13</sup>See Gelman and Hill (2007) for a more detailed discussion about how each senator-specific coefficient estimate contributes to the mean estimate.

2010, often faces criticism from within his own party. Indeed, he faced six primary challengers in the 2014 election, which was the next time he was up for re-election. Lastly, Sen. John McCain (R-AZ) was the only senator for whom the 95% credible interval for Roll Call Agreement contained 0.

**Figure 3.1.** Varying Slopes for Roll Call Agreement



**Note:** The first panel reports the varying slopes for Roll Call Agreement for the estimates from the first column of Table 3.4 while the second panel reports the varying slopes for the model estimates from the third column. A dot denotes the posterior mean for each senator with gray dots corresponding to senators who are up for re-election and black dots denoting senators how are not up for re-election. The bars plot the 95% credible interval for each posterior.

The second column of Table 3.4 reports the coefficient estimates when 2010 CCES respondents who failed to evaluate one of their senator’s job performance are included in the analysis. In practical terms, this means that a respondent who evaluated one senator, but not the other is no longer excluded from the analysis. As noted above, it is important to remember that these respondents can be included because the MCMC sampling algorithm will impute their missing approval rating. The posterior distributions for each covariate is effectively identical in this second model even though there are over 3000 more observations. As such, there is no evidence that substantive conclusions based on the first model were somehow a result of some respondents being able to evaluate the senator who was up for re-election, but not the senator who was

not up for re-election.

The third column of Table 3.4 reports the coefficient estimates for 2012 survey respondents who rated the job performance of both of their senators. Although the coefficient for Roll Call Agreement is still positive and the 95% credible interval does not cross 0, the effect is considerably smaller than in the models based on the 2010 CCES. A smaller effect is hardly surprising given the clear differences in the distribution of agreement scores across survey years (see Table 3.3). The important point is that the substantive conclusions regarding the effect of Roll Call Agreement are the same across both surveys.

As with the 2010 CCES data, the estimate for Election has a credible interval that crosses 0. Indeed, the posterior is essentially centered at 0, as approximately 52% of the posterior distribution for the coefficient estimate takes a negative value. There is little evidence then that electoral proximity influenced 2012 survey respondents' evaluations. As before, Figure 3.1 demonstrates this point quite clearly as there are no systematic differences in the senator-specific coefficients across electoral context. Indeed, 34 out of the 44 senator-specific estimates have credible intervals that include 0.44, which is denoted by the dashed line in the second panel of Figure 3.1.

Unlike the 2010 CCES, however, a number of the senator-specific coefficients have credible intervals that cross 0. In total, the posterior distribution for 17 of the 44 senator-specific coefficients have credible intervals that cross 0, which is no doubt a function of the lower mean agreement scores for the 2012 CCES survey. There were, however, only three senators for whom the posterior mean of the coefficient was negative and the 95% credible interval did not include 0. As before, most of the cases with this unexpected result are not entirely surprising. For example, Sen. Claire McCaskill (D-MO), who was up for re-election in 2012, was widely expected to lose until her opponent, then Rep. Todd Akin (R-MO) made a major political gaffe late in the campaign. Similarly, Sen. Max Baucus (D-MT) would eventually

decide to not seek re-nomination in 2014, but instead vacated his seat in order to be appointed the U.S. Ambassador to China. The final case, Sen. Bob Casey (D-PA), was somewhat more surprising and there is not an immediate explanation for this result. One potential explanation is that it may simply be a result of the survey sample with complete answers. Sen. Casey won re-election by almost 10 percentage points, but his approval rating among CCES respondents was only 45%.

The final column of Table 4.2 reports the coefficient estimates when the respondents who did not rate the job performance of a senator are included in the analysis. As with models for the 2010 CCES, the posterior distributions for each covariate are nearly identical across both models for the 2012 CCES data. While the posterior means and credible intervals do change, these changes are never more than a few tenths and the changes are never in a consistent direction. In sum, there is little evidence that including respondents who could not rate both senators' job performance changes our substantive conclusions.

Lastly, both of the control variables for respondents' perceptions of senators, Party Agreement and Ideological Agreement, have effects in the expected direction. The estimated effect size does, however, differ somewhat across years. For example, the upper bound on the effect of a one unit change in Ideological Agreement (e.g. 0 to 1) was a 0.14 percentage point change in the probability of approval for the model based on the 2010 CCES.<sup>14</sup> In contrast, the estimated maximum effect for the model based on the 2010 CCES was a 0.31 percentage point change in the probability of approval. The maximum effect for a unit change in Party Agreement was effectively the same across years – 0.24 percentage point change for 2010 and 0.26 percentage point change for 2012.

Up to this point, the role of political knowledge has not been explicitly addressed. Political knowledge is a relevant consideration for this analysis because it can have im-

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<sup>14</sup>See Gelman and Hill (2007, 82) for a discussion of the “divide by 4 rule” for interpreting logistic regression coefficients.

portant consequences for inferences based on survey data (Althaus, 1998; Delli Carpini and Keeter, 1996; Gilens, 2001). Indeed, Jessee (2009, 2010) documents the effects of political knowledge on vote choice in both the 2004 and 2008 presidential elections. He finds that individuals with higher levels of political knowledge are more likely to support the “correct” candidate in terms of the spatial model of voting than are individuals with lower levels of political knowledge.

Given prior evidence of information effects, it can be instructive to examine whether the results for electoral proximity reported above are influenced by political knowledge. For example, individuals with lower levels of political knowledge may “benefit” more from the type of legislator-specific information that emerges through a political campaign. Said differently, the potential effects of electoral proximity might be found among those with the least knowledge about Congress and their state’s senators. In order to construct a measure of political knowledge, I use three survey questions that were asked on both the 2010 and 2012 CCES surveys. These questions are ideal for this specific research question because they are related to general knowledge about congressional politics. First, each respondent was asked to identify the party affiliation of each senator. Second, each respondent was asked to identify the majority party in the House of Representatives and the Senate.

**Table 3.5.** Number of Questions Correctly Answered, Proportion

Correct Answers	2010 (%)	N	2012 (%)	N
0-1	10.97	3952	28.31	13512
2	9.15	3299	15.58	7433
3	79.87	2878	56.09	26763

**Note:** Cell entries report the proportion of respondents who were able to correctly answer 0-1, 2, or 3 of the 3 political knowledge questions.

Based on these questions, I was able to construct a measure of political knowledge about congressional politics. The first level of political knowledge includes all respondents who answered 0 to 1 of the questions correctly. Since there are effectively

only two answers that could reasonably be considered correct, I chose to include respondents who answered 0 and 1 questions correctly to account for the chance that a respondent simply made a good guess on one of the questions. The second level of political knowledge includes all respondents who correctly answered 2 out of the 3 questions. The final level of political knowledge includes all respondents who answered all 3 questions correctly. In Table 3.5, I report the proportion of respondents who fall into each of the three categories for both the 2010 and 2012 CCES. A majority of respondents were able to answer all three questions correctly in both 2010 and 2012, 79.9% and 56.1% respectively.

After identifying which subgroup a respondent belonged to, I subset the dataset for each year to correspond to these three groups. I then re-estimated the model for the 2010 and 2012 CCES respondents for each of the subsets.<sup>15</sup> For this analysis, I focus only on respondents who were able to rate both senators, which are the cases used to obtain the estimates reported in columns one and three of Table 3.4. In Figure 3.2, I report summaries of the posterior distribution of Roll Call Agreement and Election across all three levels of political knowledge for both the 2010 and 2012 CCES.

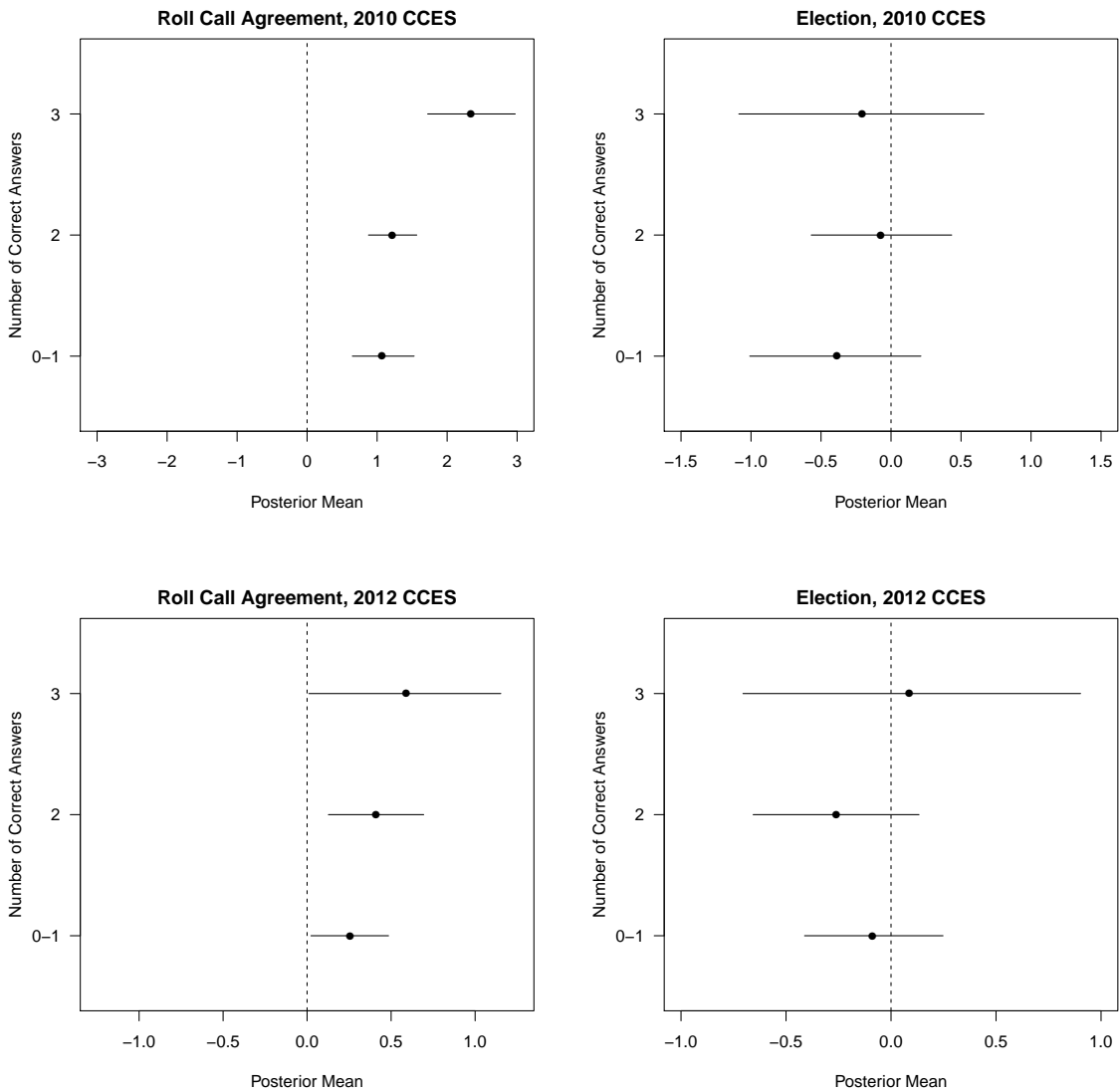
The top left panel of Figure 3.2 reports summaries of the posterior distribution for Roll Call Agreement across levels of political knowledge for the 2010 CCES data. The estimates for Roll Call Agreement suggest that there is at least some evidence of information effects in the 2010 survey. The 95% credible interval from the model for respondents who answered all three questions does not overlap with the credible interval of the estimates for other groups. In fact, the posterior mean for this category is almost double that of the second level of political knowledge, 2.34 and 1.22 respectively. The results for the 2012 survey, which are reported in the bottom left panel of Figure 3.2, do not provide evidence of information effects. While the

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<sup>15</sup>The Gelman-Rubin diagnostic statistic converged to 1 for all coefficients in all six of the models, which indicates that there was no evidence of non-convergence.

posterior mean of Roll Call Agreement does increase monotonically from the lowest to the highest category – 0.26, 0.41, and 0.59 respectively – the credible intervals for each of these coefficients overlap at some point. In sum, there is mixed evidence of information effects with respect to Roll Call Agreement.

**Figure 3.2.** Summary of Posteriors by Number of Correct Answers



**Note:** The top two panels reports the estimates for the 2010 CCES data while the bottom two panels report the estimates for the 2012 CCES. A dot denotes the posterior mean and the error bars represent the 95% credible interval.

The two right panels of Figure 3.2 report summaries of the posterior distribution of Election across levels of political knowledge. Unlike the estimates for Roll Call Agreement, the substantive conclusion is largely the same for both surveys. For both 2010 and 2012 CCES data, the 95% credible interval on the estimate for all three subgroups includes 0. There are, however, still some differences between the levels of political knowledge within each survey. For the 2010 CCES, 89.9% of the posterior distribution of the coefficient for those in the lowest level of political knowledge is less than 0. For the 2012 CCES, 90.5% of the posterior distribution of the coefficient for the second level of political knowledge is less than 0. As such, any potential evidence of information effects with respect to electoral proximity is actually in the opposite direction of what would be expected if elections facilitated evaluations.

Overall, there is little evidence then that electoral proximity impacts a respondent's ability to translate (dis)agreement into evaluations of senator's job performance. Indeed, as the senator-specific estimates in Figure 3.1 demonstrate, the effect is remarkably similar across electoral context. This substantive conclusion holds up to two additional robustness checks. First, the inclusion of respondents who were unable to evaluate the job performance of the senator who was not up for re-election produced nearly identical coefficient estimates. Second, the effect of electoral proximity is even consistent across different levels of political knowledge. Based on these results then there is little evidence, at least in 2010 and 2012, that elections make it easier for citizens to evaluate their senators. Instead, the evidence suggests that (dis)agreement on real legislative issues has a similar effect on job approval across electoral context.

### 3.4 Conclusion

Senate elections are unquestionably unique given both the length of a senator's term and the fact that only a third of the seats are contested in an election cycle. Given these features it is not surprising that scholars of congressional politics have posited that proximity to re-election conditions senators' behavior. The results presented here, however, suggest that the criteria constituents use does not vary as a function of electoral context. As such, the mechanism prior studies posit, namely time dependent monitoring and evaluations, is not supported by this analysis. It is important to note though that a failure to find support for the proposed mechanism does speak directly to senatorial behavior. Instead, it merely suggests that constituents' evaluations are not impacted by electoral proximity as has been previously assumed.

Both the case study of Senator Nelson's behavior before, during, and after the passage of the ACA and the empirical results presented demonstrate this point. Senator Nelson's experience indicates that, at least on major legislation, constituents are both aware of and attentive to their representatives' legislative activity even if they are not up for re-election that cycle. All three iterations of the empirical analysis suggest a similar conclusion, which is that constituents' evaluations of senator's job performance does not vary across electoral context. Indeed, the robustness of these results to multiple specification and tests leaves little doubt about this conclusion for both the 2010 and 2012 U.S. Senate elections.

While the results are persuasive it is important to note that the effects might be a modern phenomenon. Bernhard and Sala (2006) offer a compelling historical account of the relationship between electoral proximity and senatorial behavior. In the contemporary media environment, information about legislative activity is more readily available and can be accessed almost in real-time. While this does not mean that all citizens will take advantage of this wealth of information (Prior, 2005), it certainly facilitates the dissemination of this information by political organizations,

such as interest groups and political parties, to constituents. The fact that these results may be more applicable to the contemporary Senate is important because it suggests that these results may not disprove previous scholarship so much as they may update our understanding of the electoral connection in the Senate.

## Appendix A: Roll Call Vote Questions

### 2010 CCES

**Table 3.6.** Vote Totals for Roll Call Questions, 2010 CCES

Roll Call	<i>Senators</i>		<i>Respondents</i>		
	Y	N	Y(%)	N(%)	DK(%)
Stimulus Bill (H.R. 1)	61	37	48.17	50.55	1.27
SCHIP (H.R. 2)	66	32	67.84	31.37	0.79
Health Care Reform (H.R. 3590)	60	39	49.30	49.95	0.75
Dodd-Frank (H.R. 4173)	60	39	65.71	32.47	1.82
Don't Ask, Don't Tell Repeal (H.R. 2965)	65	31	56.47	41.68	1.57
Kagan SCOTUS Nomination	62	37	46.57	49.66	3.76

**Note:** Cell entries under the Senators category report the number of senators who voted in favor of or against a specific measure. The ye and nay positions are coded so as to match the positions as described in the survey question, which means they may be the inverse of the actual ye and nay positions on the roll call. Cell entries under the Respondents category show the proportion of respondents who said they would support or oppose the measure and the proportion who failed to take a position.

### 2012 CCES

**Table 3.7.** Vote Totals for Roll Call Questions, 2012 CCES

Roll Call	<i>Senators</i>		<i>Respondents</i>		
	Y	N	Y(%)	N(%)	DK(%)
Ryan Budget (H.Con.Res 34)	40	57	17.61	80.00	2.37
U.S.-Korea Free Trade (H.R. 3080)	83	15	48.23	47.62	4.14
Birth Control Exemption (S. 1813)	56	42	36.72	61.24	2.04
Keystone Pipeline (S. 1813)	56	42	64.60	24.91	10.48
Middle Class Tax Cut Act (S. 3412)	45	54	58.73	36.44	4.83
Tax Hike Prevention (S. 3412)	62	37	24.20	70.77	5.02

**Note:** Cell entries under the Senators category report the number of senators who voted in favor of or against a specific measure. The ye and nay positions are coded so as to match the positions as described in the survey question, which means they may be the inverse of the actual ye and nay positions on the roll call. Cell entries under the Respondents category show the proportion of respondents who said they would support or oppose the measure and the proportion who failed to take a position.

## Chapter 4

# Can Senators Hide Behind Procedure?

In the final weeks of the 111th Congress, Senate Majority Leader Harry Reid (D-NV) filed a cloture petition on the DREAM Act, an immigration reform proposal. Majority Leader Reid and his fellow Senate Democrats did not, however, expect to win this procedural battle. Earlier in the session, the Senate fail to move forward with a similar proposal, and there was no indication that this proposal would have the votes needed to invoke cloture. Senate Democrats' primary objective with this vote was not to change policy. Instead, the vote was an opportunity to “draw a clear and very public distinction between themselves and Republicans on the topic of immigration reform...” (Lillis, 2010). As expected, the cloture motion fell short of the 60 votes needed to invoke cloture, but gave senators on both sides of the aisle one last opportunity to affirm their position on immigration reform.

Despite the outcome of the vote, Senate Republicans were nonetheless displeased with Majority Leader Reid's decision to schedule an additional vote on immigration reform. Despite voting to invoke cloture, Senator Lisa Murkowski (R-AK) issued a statement criticizing the Senate majority leadership for bringing a bill to the floor

it knew would fail (Herszenhorn, 2010). Senator Jeff Sessions (R-AL), argued that it was emblematic of the Democratic leaderships' behavior during the previous two years. "For 2 years, Democratic leaders have ignored the public. They have rammed through a lot of unpopular legislation . . . So we are at it again, in these last hours, attempting to force through legislation that is not acceptable to the people" (*Congressional Record*, December 18, 2010, 22917).<sup>1</sup>

Although position taking is known to be important for members of Congress (Mayhew, 1974a), the decision to use a cloture vote for the express purpose of differentiating one party from another on a particular issue is surprising given conventional accounts of Senate procedural votes. Indeed, prior research focuses largely on the policy consequences of the Senate procedure (Binder and Smith, 1997; Koger, 2010; Madonna, 2011; Wawro and Schickler, 2007), rather than on the use of procedure for electoral politicking. The oversight can be explained, at least in part, by the fact that procedural votes are thought to be less salient than votes on amendments and final passage of legislation (Arnold, 1990; Cox and McCubbins, 2005; Froman and Ripley, 1965). Changes in the Senate's procedural environment over the last few decades, however, have led Senate leaders to adapt their strategies to find innovative ways to boost their electoral fortunes (Smith, 2014).

Recently, congressional scholars sought to address this omission and have given more attention to how, or if, the Senate majority can use procedural tactics to avoid electorally difficult votes (Den Hartog and Monroe, 2011; Smith, Ostrander and Pope, 2013). Despite disagreement about the extent to which senators can use procedure to provide political cover, there is growing evidence that such cover is far from guaranteed (Smith, 2014). Indeed, the manner in which political opponents and the news media discuss procedural votes can have important implications for the electoral significance

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<sup>1</sup>In the closing debate over the cloture motion, Senator Reid fired back with his own denunciation of Republicans. "Some Republicans are trying to demonize these young men and women, who love this country and want to contribute to it and fight for it. The real faces of the DREAM Act are the dreamers" (*Congressional Record*, December 18, 2010, 22934).

of these roll calls (Smith, 2014; Smith, Ostrander and Pope, 2013). Furthermore, the Senate’s action on the DREAM Act indicates that Senate leaders will rely on procedure for position taking rather than obfuscation.

I rely on a number of different sources in order to examine the role of procedural votes in modern Senate electoral politics. First, I provide a descriptive account of how procedural votes are treated by Senator’s political opponents and interest groups. The primary objective is to demonstrate that votes of all types are open to interpretation and strategic misrepresentation come election season. Second, I report the results of a survey experiment that tests how evaluations change depending on how a vote is reported. In doing so, I am able to measure how evaluations of the same roll call vote vary depending on whether it is presented as a substantive proposal or procedural question.

The remainder of this paper proceeds as follows. In the next section, I review previous research on procedural votes in Congress. From there, I outline the importance of roll call votes for electoral politics and discuss why procedural votes may have greater electoral importance than previous studies suggest. Next, I provide a series of case studies and descriptive accounts of how procedural votes were used for either electoral purposes. Then, I turn to a more systematic analysis of a survey experiment. Lastly, I conclude with a general discussion about the implications of my findings.

## **4.1 Procedural Votes in Congress**

The question of why legislators vote the way they do is of considerable interest to congressional scholars (Kingdon, 1989; Matthews and Stimson, 1975). Kingdon (1989) posits that a legislator’s decision is influenced by a number of considerations and may even require balancing multiple goals. Constituent preferences, saliency,

policy, and the position of party leaders both in and out of Congress are among the many factors legislators may consider. Changes in the context of each vote ultimately determines which considerations are brought to bear on a legislator's decision making process. It can be important to account for differences in the type of legislative proposals on which legislators are asked to vote to determine why they vote the way they do.

One important consideration is whether legislators are voting on a substantive or procedural question. Froman and Ripley (1965) were among the first to systematically examine differences between legislator's votes on procedural motions and substantive votes (e.g. amendments or final passage votes). They found that the House majority party was considerably more cohesive on procedural votes than on final passage votes. Subsequent research on the House finds similarly high levels of party unity for votes on special rules (Monroe and Robinson, 2008; Sinclair, 1994; Young and Wilkins, 2007) and previous question motions (Finocchiaro and Rohde, 2008). Indeed, some scholars posit that the House majority party's ability to influence policy depends on its ability to operate as a "procedural cartel" (Cox and McCubbins, 2005). The partisan nature of procedural votes is not, however, a House-specific phenomenon. Binder and Smith (1997) find a monotonic increase in the level of partisanship from the 90th Congress through the 104th Congress, the last year of their study. Lee (2009) finds that Senate procedural votes in general, and not just cloture votes, are, on average, more partisan than votes on substantive questions. These differences remain even after controlling for issue area.

What accounts for the stark differences between votes on procedural motions and substantive votes? One explanation points to the role of congressional parties. In the House, the majority party leadership can use special rules and other procedural tools to exert influence over the legislative agenda that will benefit most members of the majority party (Cox and McCubbins, 2005). It is argued that majority party

legislators will vote the party line on the procedural question even if they do not support the party's position on the substantive vote.<sup>2</sup> The Senate majority party, however, is not able to exercise the same type of agenda control as its counterpart in the House (Smith, Ostrander and Pope, 2013). Despite the Senate majority party's weaker agenda setting power, senators' policy preferences alone cannot account for the high levels of partisanship on procedural matters (Binder and Smith, 1997; Lee, 2009). Rather than party pressure, however, procedural partisanship in the Senate is argued to result from cooperation in order to further the party's' collective interests (Lee, 2009) or to promote efficiency in the legislative process (Smith, Ostrander and Pope, 2013).

A second, and arguably related, explanation focuses on the saliency of procedural votes.<sup>3</sup> Prior studies posit that votes on procedural motions are less salient because it can be more difficult to identify the policy implications of these votes (Arnold, 1990; Cox and McCubbins, 2005; Froman and Ripley, 1965; Monroe and Robinson, 2008). Arnold's (1990) concept of "traceability" is instructive for understanding the logic behind this argument. According to Arnold, an effect is not traceable unless citizens can identify an individual legislator's contribution to the observed policy effect.<sup>4</sup> Legislative procedure can be a particularly useful tool for leaders who want to help cover the contribution of individual legislators. Because procedural votes occur before the vote on an amendment or final passage, legislators can vote with their party on the procedural vote and then vote their electoral interests on the

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<sup>2</sup>For example, a special rule that limits amendments can serve both the electoral and policy interests of the majority party. By limiting amendments, a restrictive rule can secure policy outcomes that are attractive to or electorally viable for all majority party members (Jenkins and Monroe, 2012; Monroe and Robinson, 2008). The same rule, however, can also ensure that more moderate members are not forced to vote on amendments that might have undesirable electoral ramifications.

<sup>3</sup>The two explanations are related in that members of Congress are more likely to defer to party leaders precisely because procedural votes are less salient (Arnold, 1990; Carson, Crespín, and Madonna, 2014; Cox and McCubbins, 2005; Froman and Ripley, 1965; Jenkins and Monroe, 2012).

<sup>4</sup>A visible contribution by a legislator is one of three conditions Arnold (1990) identifies as necessary conditions for traceability. The other two conditions include a perceptible policy effect and an identifiable government action. All three conditions must be met in order for traceable effects to be present (Arnold, 1990, 47-49).

substantive vote (Cox and McCubbins, 2005; Jenkins and Monroe, 2012; Monroe and Robinson, 2008). Although the procedural vote makes the substantive vote possible, the procedural vote adds complexity to the causal chain by introducing additional steps in the policy process. In doing so, voters are expected to have a more difficult time identifying an individual legislator's contribution to the policy outcome.

The added complexity is hypothesized to have important electoral consequences. Specifically, prior studies contend that legislators are less likely to be held electorally accountable for procedural votes than they are for substantive votes, even when a procedural vote effectively determines the final outcome (Arnold, 1990; Carson, Crespin, and Madonna, 2014; Den Hartog and Monroe, 2011; Monroe and Robinson, 2008). In an institution where electoral pressures exert a strong influence over members' behavior, it is assumed that legislators will support their party on a procedural motion, but vote against their party on passage whenever it is politically expedient (Den Hartog and Monroe, 2011; Monroe and Robinson, 2008; Young and Wilkins, 2007).

These arguments are based primarily on the procedural politics of the House and it is not clear that the same logic can be applied to the Senate, at least not without some modification (Smith, 2007; Smith, Ostrander and Pope, 2013). In particular, many Senate procedural votes, such as cloture or the motion to table, often prevent additional consideration of the bill or amendment. In these cases, the procedural vote will be the only roll call on a given bill or subject. Indeed, the Democratic leadership scheduled an additional cloture vote on the DREAM Act precisely because they could not get a vote on passage. Senate procedural votes may in fact be the only option for both position taking and for electoral branding.

## 4.2 Roll Call Votes and Electoral Politics

Interest in the electoral consequences of a legislator's voting behavior is understandable since roll calls are the most visible indication of where the legislator stands on salient issues. In this respect, roll call votes provide valuable information about how well a legislator represents constituents' interests and the types of policies he or she will pursue if reelected. Legislators and their potential opponents are therefore attentive to the reputation roll call votes project and attempt to use them to further their own electoral goals (Arnold, 1990; Kingdon, 1989; Mayhew, 1974*a*). Since roll call votes will ultimately be used to both promote and undermine an incumbent's electoral prospects, it should not be surprising that a legislator's voting record can have both positive and negative consequences.

In terms of positive benefits, roll call votes are an invaluable tool for legislators who wish to engage in position taking (Mayhew, 1974*a*). As a position taking device, roll call votes allow legislators to claim support for issues that are popular with the public. It is important to note that the value of the position does not depend upon passing legislation. What matters is simply the opportunity to voice support for or opposition to an issue. Mayhew (1974*a*) posits that the electoral requirement for a member of Congress is "not that he make pleasing things happen but that he make pleasing judgmental statements. The position itself is the political commodity" (62). The implication of this argument is that legislators have an incentive to cast multiple votes on a popular or electorally beneficial issue even if they know it will not change policy.

For example, since reclaiming majority control in 2011, House Republicans have held four separate votes on repealing the Affordable Care Act (ACA). The first three votes occurred when the Democrats still held a majority of the seats in the Senate, which means the House Republicans had no expectation that the repeal legislation would become law. Despite gaining a majority in the Senate, the House Republicans'

fourth vote was still a symbolic act intended to reaffirm their opposition to the ACA since President Obama would veto the legislation if it passed the Senate. The Senate Democrats' decision to schedule a cloture vote on the DREAM Act in the final month of the 111th Congress is another example of structuring the legislative agenda purely for position taking purposes. The important difference between these two examples, however, is that the latter involved a procedural vote whereas the former entailed passing legislation. As noted above, the contrast between these two cases speaks to the fact that a procedural vote may be the only way for senators to take a position on a salient issue.

A legislator's voting history can also harm his or her electoral fortunes (Bianco, Spence and Wilkerson, 1996; Canes-Wrone, Brady and Cogan, 2002; Carson et al., 2010). Members of Congress are therefore concerned about and attempt to avoid being on the *wrong* side of a roll call vote (Arnold, 1990; Mayhew, 1974*a*; Kingdon, 1989). There are a number of different ways in which legislators can end up taking the incorrect position. Legislators who develop a voting record that is either too ideological (Canes-Wrone, Brady and Cogan, 2002) or too partisan (Carson et al., 2010) for their district may be punished by voters on election day. Conversely, legislators from more lopsidedly partisan districts may be hurt electorally if they vote in support of bipartisan legislative proposals (Harbridge and Malhotra, 2011).<sup>5</sup>

During elections, legislators may have to address accusations that their support for party leaders or the president is out of step with their constituents' preferences. During the 2014 Senate elections, for example, a number of vulnerable Democratic were targeted for voting with President Obama on 96 percent or more of votes on which he took a position (Lesniewski, 2014). These high presidential support scores were due in large part to the Senate majority leadership's decision to restrict amend-

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<sup>5</sup>The House and Senate incumbents, including then House Majority Leader Eric Cantor (R-VA), who recently faced primary challenges for not being "conservative enough" are illustrative on this point.

ment votes. In this case, the procedural strategy not only failed to provide coverage for vulnerable majority party members, but led them to accumulate a voting history that was not electorally desirable. Not surprisingly, many of these incumbents faced a number of tough questions during the campaign and were portrayed by both their opponents and the National Republican Senatorial Committee as being in lockstep with the president (Lesniewski, 2014). The strategy was successful as five of the eight incumbent Democrats that *Roll Call* identified as vulnerable failed to win reelection.

In sum, the electoral implications of a legislator's voting record are determined by one of two considerations. First, did he or she take the correct position on salient issues? Second, did the legislator cultivate a voting record that was out of alignment with constituents' preferences? If neither of these questions can be answered in the affirmative, a legislator may have little cause for concern. When the answer is less favorable, legislators are more likely to be punished on election day. There is no requirement, however, that the votes used by either an incumbent or a challenger are only those on substantive votes. In practice, the only requirement is that there is *a vote* to support ones claim, which means electoral campaigns will turn to both procedural and substantive votes.

### **4.3 An Electoral Side to Procedural Votes?**

The argument that procedural votes are less visible and therefore not electorally costly rests on the assumption that certain types of roll call votes are treated differently in the electoral arena. It is not clear, however, that this distinction is in fact made in real world politics (Carson, Crespin, and Madonna, 2014; Smith, Ostrander and Pope, 2013). In order to elucidate this point it can be instructive to examine the different ways in which procedural votes are used to further electoral goals. Below, I consider two such examples.

First, electoral campaigns for the U.S. Senate are replete with messages about senator's legislative record and performance. Given the value of being able to point to specific votes or legislative actions (Arnold, 1990; Mayhew, 1974*a*; Grose, Malhotra and Van Houweling, N.d.; Sellers, 1998), some opponents, and perhaps even some senators, will reference procedural votes as evidence of where the senator stands on key issues.<sup>6</sup> In support of this point, I document a series of cases where procedural votes appeared in candidates' campaign materials. Second, political candidates are not the only interested party that might selectively use procedural votes to build a narrative about a senator's issue positions. Smith, Ostrander and Pope (2013) found that interest groups were just as likely to include votes on the motion to table in their yearly scorecards as they were to include votes on amendments. I build on their analysis by examining how often two interest groups included all types of procedural votes in their legislative scorecards.

## **Procedural Votes and Senate Elections**

Roll call votes leave a paper trail that both supporters and opponents can use to portray an incumbent as either in or out of step with constituents. Senator's votes can be particularly important in electoral campaigns because they provide a concrete example to explain where a senator stands on an issue (Grose, Malhotra and Van Houweling, N.d.; Sellers, 1998). Indeed, Sellers (1998) contends that a campaign message will be more effective when it can be tied to a candidate's legislative record. Sellers notes these "background-supported messages" highlight the candidate's record and "indicate a history of commitment to a particular issue and position" (160).

Although many votes will not be salient enough to capture public attention when they are cast, it would be inaccurate to assume that these votes will be electorally irrelevant. If a particular vote is no longer part of the conversation about a senator's

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<sup>6</sup>Grose, Malhotra and Van Houweling (N.d.) find that a number of senators highlighted their vote on a cloture motion when contacted about immigration reform.

record, an opponent or an interested third party can bring it back to the public's attention prior to the election (Arnold, 1990). More importantly, the meaning of a particular vote is often subject to interpretation, and even manipulation, come campaign season. The dubious interpretation of a vote is in fact the primary manner in which procedural votes are used to tie a candidate to a particular issue (Carson, Crespin, and Madonna, 2014; Smith, Ostrander and Pope, 2013). In order to demonstrate this point more clearly, it can be helpful to look at examples from high profile Senate elections in 2010, 2012, and 2014.

## 2010

In the closing month of the 2010 election, Majority Leader Reid and Republican nominee Sharron Angle were in a near statistical dead heat in the polls with Angle enjoying a slight advantage.<sup>7</sup> During the last month of the campaign, Angle released a campaign advertisement with a devastating accusation that she hoped would pull her comfortably ahead of Senator Reid. The advertisement claimed that Senator Reid voted in favor of using taxpayer dollars to provide Viagra for child molesters and sex offenders.<sup>8</sup>

Although Angle's charge against Senator Reid was certainly attention grabbing, her claim rested on a relatively shaky reading of the roll call record. The vote in question was the result of an amendment offered by Senator Tom Coburn (R-OK) during consideration of the Affordable Care Act. Senator Coburn's amendment would have prohibited coverage of Viagra for sex offenders, but the amendment never received a direct vote. Instead, the Senate voted on a motion to table Senator Coburn's amendment and the motion passed with 57 votes in favor and 42 votes against. Senator

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<sup>7</sup>According to Real Clear Politics, the polls over roughly the last month of the election, October 1 onward, the average for Senator Reid and Angle were 45.64 and 47.64 respectively ([http://www.realclearpolitics.com/epolls/2010/senate/nv/nevada\\_senate\\_angle\\_vs\\_reid-1517.html](http://www.realclearpolitics.com/epolls/2010/senate/nv/nevada_senate_angle_vs_reid-1517.html), accessed March 5, 2015).

<sup>8</sup>Sharon Angle, "Big Clue" (<http://youtu.be/B5pK3Tnsx8g>, accessed March 5, 2015).

Reid did not in fact vote to provide taxpayer subsidized Viagra to sex offenders, but rather voted to forgo considering an amendment on the subject. There is a clear substantive difference between what Senate Reid's vote actually did and what Angle claimed, but explaining this to voters in a concise manner could no doubt prove to be difficult.

Angle was not alone in using procedural votes to make claims about Senator Reid's legislative record. Clean Energy Works, a pro-environment group, ran a television advertisement that thanked Senator Reid for voting against a "big oil bailout worth \$47 million."<sup>9</sup> The legislative action in question, however, was a vote on a cloture petition on the motion to proceed to consideration of Senator Lisa Murkowski's (R-AK) bill to block Environmental Protection Agency regulations. According to the Annenberg Center, the claim that the bill was in anyway a bailout was based on suspect assumptions about the policy consequences of the bill.<sup>10</sup> More importantly, Senator Reid never voted on the regulations or any purported bailout. Instead, he voted against ending debate on a motion that would allow for consideration of the legislation.

## **2012**

In the 2010 election, the Senate Democrats lost six seats and retained its majority by a single seat. The Democrat's electoral woes were the consequence of pursuing an ambitious legislative agenda, which led to some incumbents opting for strategic retirements or being voted out of office (Carson, 2005; Carson and Pettigrew, 2013). The 2010 West Virginia Senate race, however, revolved around different dynamics as then Governor Joe Manchin (D-WV) and the Republican nominee, John Raese, competed to fill the vacancy caused by Senator Robert Byrd's (D-WV) death. Although the pre-election public opinion polls suggested a close race, then Governor Manchin

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<sup>9</sup>Clean Energy Works, "Sliming" (<http://www.factcheck.org/2010/07/bailout-baloney/>).

<sup>10</sup>(<http://www.factcheck.org/2010/07/bailout-baloney/>, accessed March 5, 2015).

ultimately won by ten points.

The 2012 West Virginia Senate race was a rematch between Senator Manchin and Raese. Although Senator Manchin was not in office when the more controversial votes were cast in 2009 and 2010, the Raese campaign tried to link Senator Manchin to President Obama and some of the more unpopular policies of his administration. Raese's decision made sense given the West Virginian's political mood. According to the 2012 Cooperative Congressional Election Survey (CCES), only 41.41% of West Virginians approved of President Obama's job performance and only 45.73% said they would have voted in favor of the ACA when it was passed in 2010 (Ansolabehere and Schaffner, 2012).

Senator Manchin's more limited voting record and the fact that he did not vote on the ACA should, in theory, represent a considerable challenge for the Raese campaign. In order to get around this potential setback, Raese relied upon Senator Manchin's votes on procedural motions. In the "Issues" section of his campaign website, Raese claimed Senator Manchin broke his campaign promise to vote to repeal the ACA.<sup>11</sup> In this case, the vote was on whether to waive the Budget Act for Senate Minority Leader Mitch McConnell's (R-KY) amendment to an authorization bill for the Federal Aviation Administration. While Senator McConnell's amendment did seek to repeal portions of the ACA, the vote was on the germaneness of the amendment rather than repealing the ACA. Similarly, Raese issued a press releases stating that Senator Manchin voted for the "liberal position" 85% of the time.<sup>12</sup> The figure mentioned in the press release was based on 17 votes compiled by the American Conservative Union. In total, seven of the 17 votes were on procedural motions.

What is surprising about this particular example is not that the Raese campaign relied on Senator Manchin's procedural votes for their messages, but rather the extent

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<sup>11</sup><http://www.raeseforsenate.org/index.php/category/west-virginia-senate-issues/>, accessed March 5, 2015

<sup>12</sup><http://www.raeseforsenate.org/index.php/category/west-virginia-senate-campaign-news/>, accessed March 5, 2015

to which they did so. It does, however, reaffirm the earlier observation that being able to point to some kind of legislative behavior is crucial for electoral campaigns. More importantly, it demonstrates that in real world politics the type of vote often matters little so long as it supports the broader narrative a campaign wishes to push. Indeed, this problem will only be exacerbated when a legislator has a more limited voting record.

## 2014

The 2010 and 2014 Senate elections share a number of similarities. For one, both elections saw the Democrats lose a sizable number of seats, with the latter leading to the first Republican majority in 8 years. In both election years, a member of the Senate leadership faced a high profile challenge. As discussed above, Senator Reid fended off a challenge from Sharron Angle during the 2010 election. In 2014, it was then Senate Minority Leader McConnell's turn to face a challenge from a well-funded opponent, Kentucky Secretary of State Allison Lundergan Grimes.

Like Senator Reid before him, Senator McConnell was attacked for his vote on a procedural motion. In a July campaign advertisement, Grimes claimed that Senator McConnell voted to increase senior citizens' Medicare costs by \$6,000.<sup>13</sup> The accusation against Senator McConnell stemmed from his vote on the motion to proceed to consideration of a budget resolution authored by Representative Paul Ryan (R-WI). While Grimes later clarified that the \$6,000 figure was based on several studies of the Ryan budget (Contorno, 2014), Senator McConnell never had an opportunity to vote on the actual budget since the motion to proceed was not approved.

Senator McConnell was not the only incumbent senator up for reelection in 2014 to be attacked for a vote on the motion to proceed. In the North Carolina Senate race, the conservative political group Americans for Prosperity ran an advertisement

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<sup>13</sup>Allison Lundergran Grimes, "Question From Don – Retired Coal Miner" (<http://youtu.be/HnG72T6t3-s>, accessed March 5, 2015).

accusing Senator Kay Hagan (D) of taking an inconsistent position on health care reform.<sup>14</sup> Americans for Prosperity's attack was based on Senator Hagan's vote against the motion to proceed to consideration of a resolution introduced by Senator Michael Enzi (R-WY). Senator Enzi's proposal sought congressional disapproval of a rule on grandfathered health care plans under the ACA. While Senator Hagan's vote against the motion to proceed may indicate how she would vote on the bill, she never actually voted against the proposed change. The distinction is important since time considerations or general legislative efficiency, rather than policy considerations, can determine how senators vote on these types of procedural motions (Oppenheimer, 1985; Smith, Ostrander and Pope, 2013).

## **Procedural Votes and Interest Group Ratings**

Interest group's legislative scorecards are intended to help identify which members of Congress are "friends" and which are "foes" of a particular interest (Fowler, 1982). Since these groups focus on a limited set of votes relative to the full roll call record, it should not be surprising that interest groups will, at times, use procedural votes to identify a senator's position on important issues (Smith, Ostrander and Pope, 2013). The question of interest here is not, however, the relationship between a specific procedural vote and its substantive counterpart (Smith, Ostrander and Pope, 2013), but rather how often interest groups incorporate procedural votes into their legislative scorecards.

I utilize three sets of voting scores from the 97th (1981-82) to the 112th (2011-12) Congresses in order to examine this question.<sup>15</sup> The first set of votes comes from the Americans for Democratic Action (ADA), a liberal organization whose scores

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<sup>14</sup>Americans for Prosperity, "Tell Kay Hagan Obamacare Hurts North Carolina", ([https://www.youtube.com/watch?v=j0DEmS\\_J\\_ho](https://www.youtube.com/watch?v=j0DEmS_J_ho), accessed March 5, 2015)

<sup>15</sup>I aggregate scores by Congress because the League of Conservation Voters scores were, at times, reported by Congress rather than by year. The substantive conclusions are not changed if votes are aggregated by year rather than by Congress.

attempt to quantify a legislator's overall liberalism. The second set of votes comes from the League of Conservative Voters (LCV). These votes are of interest because issue specific groups may be more likely to utilize procedural votes when constructing their legislative scorecards since they must rely upon fewer votes.<sup>16</sup> The last set of votes are Congressional Quarterly's (CQ) key votes, which are votes CQ identifies as being the most influential votes in a given year. I include these votes to provide an approximate baseline for the overall saliency of procedural votes in a given congress.

In Figure 4.1, I plot the proportion of each groups' key votes that were procedural votes.<sup>17</sup> Overall, these interest groups frequently use votes on procedural motions to construct their legislative scorecards. Procedural votes accounted for well over 40% of the roll calls identified by each group as the averages for the ADA and LCV were 44.49% and 49.01% respectively. Furthermore, there were four congresses in which procedural votes account for at least half of the ADA's key votes and eight congresses where the LCV's scorecard was derived predominantly for votes on procedural motions. Although there is variability in how often these groups utilize procedural votes, the proportion is never less than 25% for the ADA and is less than 25% in only two congresses for the LCV.

It is also instructive to compare the ADA and LCV score cards with the CQ's key votes. There is only one congress, the 106th, in which the ADA's scores include a lower proportion of procedural votes than the CQ's key votes. The difference is quite small, only 1.2 percentage points, and is not substantively meaningful. There were three congresses in which the LCV's scores included a lower proportion of pro-

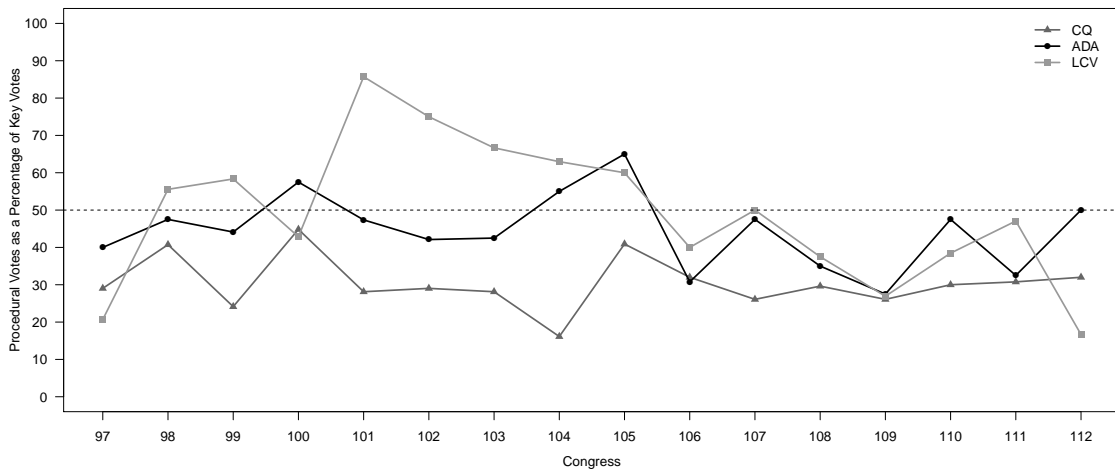
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<sup>16</sup>Although it would be preferable to include more groups, particularly more issue specific scores (e.g. the National Rifle Association), obtaining these votes over the full time span is often impractical. Several groups either do not have a publicly available archive of their scorecards or they will only release their scorecards to dues paying members.

<sup>17</sup>The following types of votes were coded as procedural votes: cloture, motion to table, motion to proceed, motion to recommit, motion to reconsider, germaneness, point of order, motion to waive the Budget Act, ruling of the chair, motion to instruct, and motion to suspend the rules. The following types of votes were coded as substantive votes: amendment, conference reports, passage, veto overrides, nominations, treaty, and resolutions.

cedural votes. In two cases, the 97th and 112th Congresses, the differences were over 8 percentage points, but these two congresses are also the low points for the LCV time series. In short, interest groups rely on procedural votes to identify “key votes” more than would be expected relative to a non-partisan measure of the importance of procedural votes. In short, these time series provide additional evidence Senators cannot count on legislative procedure to insulate them from manipulation and subjective interpretations of what counts as a liberal (conservative) position or a vote in favor of a specific issue.

**Figure 4.1.** Procedural Votes as Percentage of Key Votes



**Note:** Each data point denotes to the proportion of votes a group identified as key votes that were procedural votes for a given congress.

## Complexity and Political Cover

Each of the examples discussed above runs counter to prior arguments about procedural coverage (Arnold, 1990; Den Hartog and Monroe, 2011). Indeed, these cases demonstrate that the additional complexity of procedure is hardly sufficient to inoculate senators from electoral consequences. If anything, the complexity of legislative procedure may in fact make it easier for opponents and other interested parties to define a senator’s voting history. For example, the accusation that a senator

voted with party leaders over 90% of the time is an easy point to raise in a campaign advertisement, press release, or speech. It can be a more difficult task, however, to come up with a concise explanation that many of these votes were procedural questions and did not carry the same weight as other votes. Indeed, senators may avoid this explanation all together and instead point to other legislative actions that demonstrate their stance on the issue (Grose, Malhotra and Van Houweling, N.d.).

Although these examples are informative, they do not provide systematic evidence about whether the strategic misrepresentation of procedural votes is in fact an effective strategy. That is to say, knowing that senators and their opponents use procedural votes to highlight legislative positions is not sufficient to assess arguments about procedural coverage. What is needed is a test of whether constituent's evaluations are influenced by the selective use of procedural votes. In the next section, I discuss the results from a survey experiment designed to address this question.

## 4.4 Survey Experiment

In order to understand the effect of using procedural votes to highlight a senator's roll call positions, it is necessary to examine whether constituents' evaluations differ across legislative context. By holding the policy issue constant and varying the reported procedural context of a vote, I am able to assess the potential returns to using procedural votes to define a senator's roll call record. I designed a survey experiment, which was part of the University of Georgia's module for the 2014 CCES, in which respondents were randomly assigned to one of two groups. One group was told that their senator voted for or against a specific legislative proposal, which is the substantive vote category.<sup>18</sup> In contrast, the other group was told that the senator voted for or against a proposal to end debate on the proposal and proceed to a final vote, which is the procedural vote category. The design was chosen because the wording

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<sup>18</sup>A senator's vote choice, either yea or nay, was also randomly assigned.

for the substantive vote category closely mirrors how procedural votes are used in actual campaign communications. Similarly, the description for the procedural vote category adds the complexity to a legislator's behavior that is expected to impede constituents understanding of a legislator's role in the policy outcome (Arnold, 1990).

Before discussing the experiment in more detail, it is important to note that the results should be interpreted with care. In particular, the survey experiment has the potential to provide both evidence of procedural coverage while also raising questions about the theory's ability to explain how Senate electoral politics work in practice. Since senators and their opponents do in fact use procedural votes in campaign advertisements, we have a priori evidence to undermine part of the theoretical mechanism. What the test presented here does, however, is to examine what happens when a vote is presented more or less accurately. That is to say, the experiment examines how evaluations change when the *same* vote is presented incorrectly as a substantive position or correctly as a procedural vote. If we do find differences between the two types of votes, this may in fact be support for the argument that the complexity of procedural votes makes it harder for constituents to trace a legislator's actions. At the same time, it will also provide further evidence that procedural votes can be used effectively for position taking or to define a senator's roll call record.

## **Case Selection**

When choosing a policy issue for the survey experiment, the primary selection criteria was that it must have been part of a legislative proposal that received consideration in the most recent Congress at the time of the survey. In particular, I focused on issues that were considered and debated during the second session of the 113th Congress. The primary advantage to this approach is that respondents are asked to evaluate a current and salient issue rather than a policy question that senators had not or were not likely to consider in the 113th Congress. Similarly, utilizing an

actual legislative proposal increases the likelihood that the policy issue chosen is one that may appear in campaign advertisements either during the 2014 election or at a later date. The potential for the issue to be used in an electoral campaign is particularly desirable given the prior examples of procedural votes being used in campaign advertisements.

Based on these considerations, I chose the proposal to construct the Keystone XL pipeline as the policy issue for the survey experiment. There are number of reasons why this particular piece of legislation is appropriate for this study. First, the Keystone pipeline received a fair amount of news coverage and has been a talking point for both parties in Congress as well as President Obama. Indeed, over 75% of respondents reported having heard at least something about the Keystone pipeline while only 18% said they had heard nothing at all.<sup>19</sup> Second, while the pipeline can, and often is, associated with issues that have been salient for some time (e.g. energy independence, domestic oil production) the proposal itself is a comparatively new issue. As such, questions based on the Keystone pipeline should provide a more conservative test than an issue like gun control or gay marriage.

## Results

As noted above, respondents were randomly assigned to one of two groups. The first group, the substantive vote category, was asked: “Over the last year, the Senate considered proposals related to approval of the Keystone pipeline. Would you approve of your Senator voting for (against) the proposal?” The second group, the procedural vote category, was asked the same question except the language was changed to read, “Would you approve of your Senator voting for (against) the proposal to end debate on the bill and proceed to a final vote?” In Table 4.1, I report the proportion of respondents who approved of the senator’s vote choice by the type of vote and the

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<sup>19</sup>Just over 40% of respondents said they heard “A great deal” about the proposal while roughly 34.5% of respondents reported that they heard “A little” about the proposal.

position they were told the senator took.

A majority of respondents approved of their senator’s vote choice regardless of whether the vote was presented as a procedural question or substantive position. Respondents were, on average, more supportive of a senator’s position on the procedural vote regardless of whether the stated position was for or against. There was a nearly 15 point difference in approval between the two categories. These differences become more pronounced when we account for the senator’s reported vote choice. While a majority of respondents, nearly 65%, approved of their senator’s decision to vote against ending debate, approval drops to 46.67% when the vote is presented as a substantive position. When the senator was said to have voted yea, a majority of respondents approved of their senator’s decision regardless of how the vote is represented. These preliminary figures indicate that the manner in which a vote is depicted can influence evaluations of legislative behavior.

**Table 4.1.** Approval of Senator’s Vote Choice, by Vote Type

	Procedural (%)	<i>Vote Type</i>		N
		N	Substantive (%)	
All Cases	72.42	330	57.59	323
<i>Senator Voted</i>				
Against	64.56	158	46.67	158
For	79.65	172	68.99	165

**Note:** Each cell denotes the proportion of respondents who approve of a senator’s vote choice in a specific vote type and vote position category.

In order to examine this relationship more systematically, I estimated a series of regression models where the outcome variable, Approval, is coded 1 if a respondent approved of his or her senator’s vote choice and 0 otherwise.<sup>20</sup> The key predictors are a measure of issue agreement, an indicator for whether the respondent was as-

<sup>20</sup>For this analysis, I exclude respondents who answered “Don’t Know.” I provide a supplementary analysis in the Appendix that examines the respondents who answered Don’t Know. These cases do not appear to influence the substantive conclusions.

signed to the procedural or substantive vote category, and the interaction of these two covariates. The measure of issue agreement, Agreement, is based on the respondent's position on building the Keystone pipeline and the position they were told the senator took. Agreement is coded 1 when a respondent supports (opposes) the Keystone pipeline and the senator was said to have voted yea (nay). Conversely, it is coded 0 when a respondent supports (opposes) the pipeline and the senator was reported to have voted nay (yea).<sup>21</sup> The vote type measure, Procedural, is coded 1 if the respondent was assigned to the procedural vote treatment and 0 otherwise. The interaction term allows for the effect of Agreement to vary across the type of vote.

I also include a series of control variables. First, I include a nominal measure of partisan identification – Democrat, Republican, and Independent – with Independents treated as the reference category. Second, I used a respondent's assessment of the state's two senators job performance to control for overall satisfaction with the Senate delegation. The first category includes respondents who only approve of one of the state's two senators, the second category includes respondents who approve of both senators, and the final category, which is treated as the reference category, includes respondents who approve of neither senator's job performance.

In Table 4.2, I report the estimates from two logistic regression models. The first model includes only the the main predictors, Agreement and Procedural, and their interaction while the second model incorporates the control variables. As noted above, the interaction term in both models is of interest because it indicates whether the effect of Agreement varies across the vote category. In both specifications, the interaction term is negative and statistically significant, which means respondents who agreed with a senator's vote choice were more likely to approve of that action

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<sup>21</sup>The coding scheme treats issue agreement the same regardless of the assigned vote type treatment. Although there are differences between voting for (against) cloture and voting for (against) final passage, the interaction term allows for the effect to vary based on the type of vote. Furthermore, the coding procedure is analogous to the one used in previous studies (Grose, Malhotra and Van Houweling, N.d.).

when it was reported as a vote on the pipeline itself. The effect of agreement is still positive, however, when the vote was identified as a procedural vote. It is interesting to note that this is a change from the pattern reported in Table 4.1 where the proportion of respondents approving of a senator’s vote was higher for the procedural vote category than for the substantive vote category. Once issue agreement is accounted for, however, respondents’ evaluations more closely adhere to expectations.

**Table 4.2.** Approval of Senator’s Vote Choice, Logistic Regression

	Coef. (S.E.)	Coef. (S.E.)
Agreement	2.738* (0.323)	2.886* (0.334)
Procedural	1.597* (0.269)	1.698* (0.280)
Agreement × Procedural	-1.983* (0.436)	-2.125* (0.447)
Democrat		-0.385 (0.267)
Republican		0.509 (0.275)
Approve, One Senator		-0.480* (0.276)
Approve, Both Senators		-0.358 (0.264)
Constant	-0.880* (0.190)	-0.712* (0.281)
N	518	518
AIC	555.787	547.118
PRE	0.311	0.311

**Note:** Cell coefficient entries are from a logistic regression model with standard errors in parentheses.  
\* p < 0.05

While the model results indicate that the effect of Agreement is conditional on how a vote is reported, the model estimates do not fully convey the substantive implications of the survey experiment. Indeed, the primary quantity of interest is the likelihood of supporting a senator’s vote choice across each of the four possible

categories: there is issue agreement and the vote is said to be a substantive position, there is issue agreement and the vote is reported a procedural motion, there is not issue agreement and the vote is identified as a substantive vote, and there is not issue agreement and the vote is reported as a procedural vote. In Figure 4.2, I report the model-based predicted probability, along with 95% confidence intervals, for each of the four categories

When the vote was reported as a vote on the proposal, the probability of approving of a senator's vote choice was 0.19 and 0.81 respectively for respondents who disagreed and agreed with the senator's position.<sup>22</sup> The difference between these two groups of just over a 60 percentage points is both statistically and substantively meaningful. In contrast, the difference in the predicted probability for the procedural vote treatment is a more modest 17 percentage points. The predicted probability for the Disagree category is 0.56 while the prediction is 0.73 for the Agree category.<sup>23</sup> Furthermore, these predictions are not statistically discernible from one another, which can be inferred from their overlapping confidence interval.

In sum, these results demonstrate that the way in which a vote is presented to constituents matters. As discussed earlier, these results provide both support for and refutation of the theoretical arguments articulated under the theory of procedural coverage. It provides support in that the inclusion of information about Senate procedure made it more difficult for respondents to assign blame to senators who voted against constituent preferences. These results reaffirm the earlier observation that legislative procedure will not always be sufficient to provide political cover in real world politics. Indeed, constituents' evaluations differ markedly when a procedural vote is offered as evidence of a senator's position on a particular issue. While the case studies above made it clear that procedural votes are used in electoral campaigns,

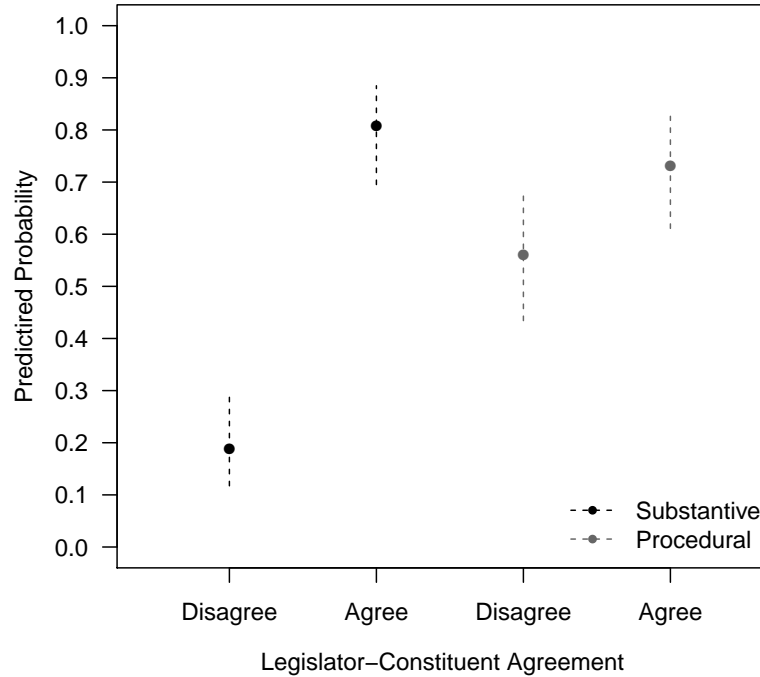
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<sup>22</sup>The 95% confidence interval for the Disagree category is [0.12 , 0.29] and [0.70, 0.88] for the Agree category.

<sup>23</sup>The Disagree category has a 95% confidence interval that ranges from [0.43, 0.68] while Agree category's confidence interval is [0.61, 0.83].

the survey experiment provides evidence that the strategic misrepresentation of these votes can prove to be a fruitful strategy.

**Figure 4.2.** Predicted Probability of Approval by Vote Type



**Note:** The black (gray) dots report the predicted probability for the substantive (procedural) treatment group while the dashed lines denote the 95% confidence interval.

## 4.5 Conclusion

The primary objective of this chapter was to evaluate whether legislative procedure provides political cover for senators. Overall, the evidence presented here suggests that the idea of procedural coverage in the Senate is correct in theory, but wrong in practice. First, the Senate’s rules can make procedural votes a viable option for position taking or for tying an incumbent to an unpopular issue. Since a number of the Senate’s procedural motions effectively prevent additional consideration of a legislative proposal, the procedural vote will often be the only roll call on a given issue. Second, both Senate candidates and interest groups use procedural votes to build a

narrative about a legislator's voting history and issue positions. These examples demonstrate that the connection between a roll call and the policy position it is said to represent can often be tenuous at best. Furthermore, it can be difficult for a candidate to explain the intricacies of and provide the proper context for a procedural vote. As such, there is little reason to expect Senate candidates to stop using these votes in campaign communications.

Lastly, the results from the survey experiment indicate that the way roll calls votes are reported to the public matters. When a vote is presented as a procedural question, constituents are less likely to offer a negative evaluation of how a senator votes, even if the senator voted in a manner that is incongruent with the constituent's preferences. In contrast, when the vote is portrayed as a yea or nay on a salient issue, constituents' evaluations are based solely on whether the senator took the "correct" position. Smith (2014) notes that reporting procedural votes in this manner can be particularly concerning since the public is not given an accurate view of the workings of Congress.

These findings have several implications for broader issues related to the relationship between members of Congress and the public. If procedural votes are used to explain a senator's legislative positions, then we may have reason to be concerned about the quality of information that constituents receive. Although some of these votes will likely reflect a senator's actual position on an issue, they can also distort a senator's legislative record. For example, the use of procedural votes to calculate how often an incumbent voted with party leaders will likely provide a biased measure of certain incumbent's issue positions. More importantly, these results suggest that these inaccuracies can have real consequences for incumbents come election time.

## Appendix A: Questions for Survey Experiment

### UGA335, Cloture on Keystone Pipeline

**Assignment to Treatment:** Question wording was randomized in the following manner: first, rotate between “voting for” and “voting against.” Second, rotate between “the proposal” and “the proposal to end debate on the bill and proceed to a final vote.”

**Question:** Over the last year, the Senate considered proposals related to approval of the Keystone pipeline. Would you approve of your Senator voting for (against) the proposal (the proposal to end debate on the bill and proceed to a final vote)?

**Answer Choices:** Approve, Disapprove, Dont know

### UGA420, Pipeline Support

**Question:** In general, would you support or oppose efforts like this to build oil pipelines from Canada to American oil refineries?

**Answer Choices:** Strongly Support, Somewhat Support, Somewhat Oppose, Strongly Oppose, Dont know

## Appendix B: Supplemental Results

**Table 4.3.** Approval of Vote Choice on Keystone Pipeline, by Vote Type

	<b>Approve (%)</b>	<b>Disapprove (%)</b>	<b>DK (%)</b>	<b>N</b>
All Cases	42.93	23.03	34.04	990
<i>Senator Voted Against</i>				
Substantive	30.31	34.65	35.04	254
Procedural	43.40	23.83	32.77	235
<i>Senator Voted For</i>				
Substantive	45.04	20.25	34.71	242
Procedural	52.90	13.51	33.59	259

**Table 4.4.** Probability of Don't Know Answer for Approval of Vote Choice, Logistic Regression

	Coef. (S.E.)	Coef. (S.E.)
Agreement	-0.029 (0.191)	-0.075 (0.207)
Procedural	0.096 (0.191)	0.076 (0.205)
Democrat	0.244 (0.221)	0.309 (0.241)
Republican	-0.761* (0.266)	-0.559* (0.284)
Approve, One	0.049 (0.242)	0.108 (0.263)
Approve, Both	-0.021 (0.233)	-0.023 (0.251)
<i>Pipeline Awareness</i>		
A little		1.536* (0.248)
Don't know		1.950* (0.803)
Nothing at all		2.507* (0.306)
Constant	-1.212* (0.252)	-2.405* (0.329)
N	662	662
AIC	691.625	612.100

**Note:** Cell coefficient entries are from a logistic regression model with standard errors in parentheses.  
\* p < 0.05

# Chapter 5

## Conclusion

Although there is considerable evidence to support the theoretical expectations of the electoral connection, a disproportionate share of the research to date only considers the House. The House-centric focus is not surprising since the structure of the Senate can pose challenges for developing a research design to study different aspects of the electoral connection. For example, if we wish to study the effects of advertising, credit claiming, or position taking on House elections, we can utilize data on legislative behavior from a single congress to estimate the relationship between observed legislative behavior and electoral margins. A similar study in the Senate can be complicated by the fact that the researcher must make decisions about how to aggregate data and whether behavior from early in a senator's six-year term should be weighed the same as behavior in congress immediately preceding the election.

While studying the electoral connection in the Senate can be complicated, it is hardly impossible. Indeed, the same features of the Senate's institutional design that present challenges for researchers, can also provide unique opportunities and leverage for understanding the electoral connection. Although senators' six-year terms can pose a problem for deciding how to aggregate data, the lengthier terms also provide an opportunity to examine whether a legislator's responsiveness to constituents

varies over his or her term. Similarly, the Senate's staggered terms provide leverage on understanding how constituents exercise accountability and the mechanisms that enhance or diminish it. The absence of direct Senate elections until 1914 can make it difficult to conduct historical analyses on election outcomes or to construct a lengthy time series of certain historical trends (e.g. electoral marginality). Historical differences in the method for election of senators does, however, provide a valuable opportunity to examine how electoral institutions influence representation.

In short, understanding the electoral connection in the Senate may, at times, require a shift away from methods used to study the House. To be clear, it is not the case that the same approach cannot be used to examine both the House and Senate. Indeed, several studies have successfully applied research on the House to questions about politics in the Senate (Gerber, 1998; Krasno, 1994). In doing so, these studies offer valuable insights about how electoral politics in the Senate is similar to and different from the House. The broader goal of this dissertation, however, is to highlight how the Senate provides unique opportunities to examine the electoral ties between legislators and constituents. In particular, this project offers a detailed examination of how the institutional design of the Senate influences the electoral connection.

As discussed before, each chapter explores the relationship between the Senate's institutional structure and one or more components of the electoral connection. Specifically, each chapter examines how features of the Senate's institutional design influence either responsiveness, accountability, or both.<sup>1</sup> In Chapter 2, I examined whether practice of electing U.S. senators via state legislatures impinged upon senator's responsiveness. In Chapter 3, I evaluated whether the Senate's lengthier terms present a threat to electoral accountability. Lastly, in Chapter 4, I analyze the effect of the Senate's procedural rules on both responsiveness and accountability. Overall,

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<sup>1</sup>According to Carson and Jenkins (2011), responsiveness requires legislator's behavior to reflect constituent preferences while accountability is the ability of voters to reward or punish legislators for their actions in office.

these results demonstrate that the Senate's rules have the potential to influence both responsiveness and accountability.

The remainder of this chapter is organized as follows. First, I briefly summarize the findings reported in this dissertation. In doing so, I give particular attention to how these findings relate to the electoral connection and their broader implications for legislator-constituent relations. Lastly, I outline a number of avenues for future research that builds on the findings and arguments presented earlier. In particular, I discuss how to extend this project to examine ambition and autonomy, which Carson and Jenkins (2011) identify as two additional components of the electoral connection.

## 5.1 Summary of Findings

In Chapter 2, I investigated the conditions under which pre-Seventeenth Amendment Senate elections were representative of the statewide electorate's preferences. My research design, which compares senators to representatives who were elected from statewide districts, takes advantage of unique features of the nineteenth century congressional districting process that led to a greater number of representatives being elected statewide. Senators and members of the House who are elected statewide make a natural comparison group because they share a common geographic constituency, but are ultimately elected by, and responsible to, different reelection constituencies. I find that the partisan outcome of indirect Senate elections was nearly always the same as those for direct, statewide House elections. Similarly, indirect elected senators legislative behavior was as representative of constituent preferences as directly elected representatives.

These findings demonstrate that the insulation of Senate elections from the mass electorate did not impede legislative responsiveness. As such, this study adds to the growing body of evidence that legislators engaged in constituent-oriented behavior

even when the electoral rules would seem to minimize the benefit of such behavior (Gamm and Shepsle, 1989; Schiller, 2006; Schiller and Stewart, 2015). Indeed, developing a reputation for being responsiveness was arguably imperative for indirectly elected senators who wished to be reelected because it could help them build a base of support outside of the state legislature (Schiller, 2006). The high rates of turnover in state legislatures during this era made it important to build electoral support outside of the state legislature because a senator who sought reelection would likely serve longer than the individuals who first elected them to office (Schiller and Stewart, 2015).

In Chapter 3, I revisited the important question of whether elections influence the manner in which constituents evaluate senator's job performance. For my empirical analysis I take advantage of the Senate's staggered terms and focus on states where an incumbent is running for reelection, which allows me to compare the *same* individuals' evaluations of senators who are and are not up for re-election. In order to examine this relationship systematically, I examined whether elections alter the relationship between senator-constituent roll call agreement and job approval. I find that constituents' ability to translate (dis)agreement into their evaluations of senators does not vary over electoral context.

Contrary to prior research, the findings in this chapter indicate that senators' six-year terms do not undermine accountability. While there is evidence that senators alter their aggregate roll call behavior over time (Ahuja, 1994; Bernstein, 1991; Elling, 1982; Stratmann, 2000), the findings reported in this chapter suggest that this behavior is unlikely to have its desired effect among voters. Indeed, legislative shirking has the same negative consequences for approval ratings early in a term as it does in an election year. Although these results may seem surprising given the evidence reported in research that focuses only on roll call voting, it is more intuitive when we consider that legislators can find it difficult to explain away inconsistent positions

(Kingdon, 1989). As such, re-election minded senator's must consider the electoral implications of votes at all time points, which in turn allows voters to exercise a more consistent level of influence throughout a six-year term.

In Chapter 4, I used a survey experiment to test whether constituents' assessment of a senator's voting record changes when a roll call is presented as a substantive or procedural question. Prior studies suggest that legislators can hide behind procedure because it can be difficult for constituents to connect procedure to policy effects. The question is of particular importance in the Senate given the chamber's supermajority procedural hurdles. I find that prior theoretical arguments about procedural coverage are correct in theory, but do not fully account for the actual practices observed in the U.S. Senate. In particular, a senator who votes in opposition to a constituent's preference is not penalized as much when the roll call is described as a procedural vote. Unfortunately for senators, most electoral opponents do not respect this same distinction between procedural and substantive positions when they try to build a case against an incumbent.

The results from this chapter inform our understanding of both responsiveness and accountability. The Senate's procedural rules can encourage senators to find creative ways use procedural votes in order to explain how they are responsive to constituent preferences. Similarly, the complexity of Senate procedure can encourage a senator's electoral opponent to use procedural votes to present a misleading view a senator's legislative behavior. Both of these strategies demonstrate that the Senate's rules of procedure can be present serious challenges for accountability. Indeed, the complexity of Senate procedure can undermine constituents' ability to assign blame for undesirable policy outcomes (Smith, 2014).

## 5.2 Future Research

Taken as a whole, these results indicate that congressional scholars cannot understand the electoral connection in the Senate without also considering the importance of the institutions that make up this legislative body. These results, however, constitute only an initial step toward a more complete understanding of the electoral connection in the U.S. Senate. Indeed, there are still a number of unanswered questions about the effect of the Senate's institutional design and responsiveness and accountability. Furthermore, this dissertation does not address the relationship between legislative institutions and ambition or autonomy. In the remainder of this chapter, I outline both potential extensions of the research presented here as well as possibly avenues for research on ambition.

There is still much to learn about representation under indirect Senate elections. While the analysis presented here focused on election outcomes and aggregate voting behavior, the research design employed could be applied more broadly. One area that seems potentially fruitful is to delve more deeply into the voting behavior of these same senator-representative pairs. For example, it could be instructive to compare each legislators' voting behavior on questions related to the role of the the federal government. Bill introduction is another area of legislative behavior in which a comparison of these two groups could provide insights about the nature of the electoral connection during this period. Indeed, prior research suggests that indirectly elected senators had an interest in constituent-oriented legislation (Schiller, 2006). By comparing the behavior of senators and representatives with the same geographic constituency, we might be able to provide a more complete picture of the connection between indirectly elected senators and their statewide constituents.

Furthermore, we know comparatively less about why indirect Senate elections failed to tie the Senate to the states in the way envisioned by the authors of the Constitution. Although Swift's (1996) observation that the states were never given the

tools necessary to control senators, that did not prevent states from attempting to instruct senators throughout much of the early to mid-nineteenth century (Riker, 1955; Eaton, 1952; Hoffmann, 1956). Previous historical accounts of senatorial opposition to these efforts have largely focused on either a limited time period or a particular state or region (Eaton, 1952; Hoffmann, 1956). What is missing then is a more systematic account of the relationship between senators and the state governments that selected them. It could therefore be instructive to conduct archival research of both state legislatures and newspapers to develop a data set of communications between state legislatures and U.S. senators on important legislation.

While the results presented here uncover little evidence of an effect of electoral proximity, the analysis is based on comparisons within a single Congress. Ideally, the analysis would also include a more longitudinal analysis where the same set of senators is examined over their six-year term. Under this approach, it would be possible to track the relationship between preference congruence and job approval over time. Such an analysis is problematic at present due to inconsistent question wording across different versions of the CCES. The analysis will be possible, however, once there are a consistent set of questions across three different iterations of the survey. Furthermore, the measure of preference congruence employed in this dissertation could be improved upon. With a larger number of survey items it would be possible to scale respondents and legislators onto the same ideological space (Jessee, 2009, 2010). These more refined measures would be helpful in developing a more detailed empirical analysis of the influences of electoral proximity.

The electoral implications of Senate procedure is arguably the subject with the most room for additional research. There are a number of potential ways in which to extend this analysis. First, the survey experiment focused on a single vote. One of the larger concerns about the (mis)use of procedural votes in electoral politics, however, is that including these votes can distort a senator's broader voting record. For example,

procedural votes and procedural strategies can make it appear as if moderate senators were more partisan than their substantive legislative record would suggest. As such, it would be instructive to repeat this analysis by altering the type of information respondents are given (e.g. co-sponsorship versus voting records). Second, the present study focuses on evaluations of individual legislators, but it may be fruitful to also examine the effects on evaluations of Congress as an institution. One could conduct a similar survey experiment where the Senate, or Congress, is the entity being evaluated rather than an individual senator. This analysis could be particularly instructive since the news media often fails to report on the specific procedural context of legislation that is under consideration in Congress (Smith, 2014). As Smith (2014) suggests, it would be particularly insightful to see if evaluations of the minority party are altered when additional information about the procedural context (i.e. the minority obstructing on a bill) is provided to the public.

While each chapter identifies how an aspect of the Senate's institutional design influences either responsiveness or accountability, it says little about either ambition or autonomy. An examination of historical changes in senatorial career ambitions offer one way of studying both ambition and autonomy. Previous studies generally equate ambition and autonomy with the ability to actively seek out or make decisions about a career within Congress (Brady, Buckley and Rivers, 1999; Mayhew, 1974*a*; Polsby, 1968), but it is not clear that it is an appropriate standard for historical congresses (Carson and Jenkins, 2011; Kernell, 1977). If, for example, we define career ambition in terms of a career within politics rather than a career in Congress then we might expect senators in earlier eras to behave in a manner similar to modern day senators. A historical examination of senators' career decisions could also be valuable for understanding the electoral connection over a broader time span.

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